Making Subsidies Work: Rules vs. Discretion*

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November 17, 2022

Abstract

We estimate the employment effects of a large program of public investment subsidies to private firms that ranked applicants on a score reflecting both objective rules and local politicians' discretion. Leveraging the rationing of funds as an ideal RDD, we characterize the heterogeneity of treatment effects and cost-per-new-job across inframarginal firms, and we estimate the cost effectiveness of subsidies under factual and counterfactual allocations. Firms ranking high on objective rules and firms preferred by local politicians generated larger employment growth on average, but the latter did so at a higher cost-per-job. We estimate that relying only on objective criteria would reduce the cost-per-job by 11%, while relying only on political discretion would increase such cost by 42%.

JEL Classification: H25, J08

Key words: Public subsidies, investment, employment, political discretion, regression discontinuity

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*We thank Josh Angrist, Oriana Bandiera, Pierre Cahuc, Augusto Cerqua, Luigi Guiso, Claudio Michelacci, Alari Paulus, Andrea Pugnana, the CLEAN group at Bocconi University, and seminar participants at various universities and at the Bank of Italy for useful comments. Simone Valle provided excellent research assistance. Paolo Pinotti gratefully acknowledges financial support from the European Research Council (ERC) grant CoG 866181. The opinions expressed herein do not reflect those of the Bank of Italy or the ESCB.

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1 Introduction

Public subsidies to firms located in disadvantaged areas are an important component of public spending. Before the Covid-19 pandemic, the total budget of place-based policies in the United States was $61 billion per year, 80% of which was cash grants and tax credits to firms (Bartik 2020). During the same period (2014-2020), the European Union's Regional Development Fund (ERDF) promoted the economic development of poorer European regions with funding of €279 billion (€46.5 billion per year), to which one should add the resources invested by individual member states. The pandemic recovery budgets are bound to increase this financial support by an order of magnitude.

The effects of such policies depend crucially on the allocation of funds, and yet there is uncertainty about the marginal effects of subsidies across different types of beneficiaries. For example, many believe that small (or infant) firms generate the highest returns to public capital, which helps them to overcome their liquidity constraints (see e.g. Chodorow-Reich 2014, Schmalz, Sraer & Thesmar 2017, Criscuolo, Martin, Overman & Van Reenen 2019, Siemer 2019), but market frictions may also force larger, more mature producers to forego investment opportunities (see e.g. Hsieh & Olken 2014, Akcigit, Akgunduz, Cilasun, Ozcan-Tok & Yilmaz 2020).

In light of this uncertainty, discretion by bureaucrats and politicians may improve on rigid policy rules by allowing subsidy allocation to incorporate additional information about the quality of firms and projects. At the same time, such discretion may be used for private benefits rather than for the public interest, as in the case of political connections (see, e.g. Fisman 2001). This "rules vs. discretion" trade-off is a classical theme in macroeconomic policy (Persson & Tabellini 2002), but carries through to other areas of government intervention, including industrial policy (Laffont 1996).

In this paper, we investigate the relevance of firm characteristics and allocation criteria – notably, the rules vs. discretion trade-off – for the effectiveness of public subsidies to private firms. Specifically, we investigate the impact of Law 488/92 (L488/92 henceforth), the largest program of investment subsidies ever implemented in Italy, and one of the largest in Europe (Giavazzi, D'Alberti, Moliterni, Polo & Schivardi 2012). Between 1996 and 2007, L488/92 funded 77 thousand investment projects over 35 open calls, with a total budget of nearly €26 billion (at constant 2010 prices), partly supplied through the ERDF.

We estimate the causal impact of these funds on firm investment, job creation, and productivity. In order to do so, we must address three main challenges that are common to most policy evaluations. First, to achieve *internal validity* we must compare subsidized and non-subsidized firms that are similar in all dimensions except for the fact that the former group received the subsidy while the latter did not. Second, the effect of the subsidy may be very *heterogeneous* across firms. Third, and relatedly, it is hard to interpret the *external validity* of estimates in different contexts, or under different allocation criteria. These three objectives involve important tradeoffs: to achieve internal validity, we typically estimate average treatment effects across a subset of "compliers" with plausibly exogenous variation; but average treatment effects may mask significant heterogeneity, and restricting the analysis to (possibly small) sub-populations of compliers may severely limit the external validity of estimates. These limitations prevent us, in turn, from evaluating the effectiveness of alternative allocation schemes, which would be most useful for the purposes of policy evaluation.

To overcome these limitations, we leverage on recent methodological advances in Regression Discontinuity (RD) analysis (Angrist & Rokkanen 2015, Dong & Lewbel 2015, Cattaneo, Keele, Titiunik & Vazquez-Bare 2021, Bertanha 2020). These methods provide testable restrictions under which one can extrapolate estimated treatment effects to different sub-populations of inframarginal units away from the RD cutoff. Together with the specific features of L488/92 and detailed firm-level data, these results allow us to characterize the heterogeneity of treatment effects across different types of firms, and to compute policy effects under actual and counterfactual allocations.

L488/92 subsidies were allocated through open calls for projects. The total budget of each call was preliminarily allocated across the 20 Italian regions, giving preference to economically under-developed regions in the South. Then, applicant firms could submit projects that were ranked within each call-region according to a numerical score of project quality, and were funded on a first-ranked, first-served basis until the available funds were fully allocated. Importantly, the numerical score weighted two main components relating, respectively, to objective criteria ("rules") and regional priorities indicated by local politicians ("discretion"). Using machine-learning methods, we show that the discretionary component of the overall score tends to favor projects submitted by smaller firms demanding relatively larger subsidies, compared to the projects that would be selected based only on objective criteria.

This allocation mechanism entails an ideal RD design, as most projects scoring just above the cutoff were subsidized by L488/92 over the following three years (unless some reason for non-compliance was detected) while projects scoring just below the cutoff did not access the same subsidy – though they could re-apply in the following calls for projects. We find that applicant firms submitting projects that scored just above the cutoff increased investment by almost 40 percent during the subsidy period, compared to applicant firms submitting projects that scored just below the cutoff. This transitory shock to investment generated, on average, a 10 percent rise in firm's employment during the same period, which increased to 17 percent over the following three years (i.e., six years after being awarded the three-year subsidy). Allowing for spillover effects within local labor markets, we show that subsidized firms do not expand at the expense of other non-subsidized firms, so our estimated effects capture a net increase in local employment. Revenues and value added increased by a similar amount, implying that firm productivity remained approximately constant. Firm survival increased by 3 percentage points (+6 percent over the baseline).

When extending the analysis to inframarginal firms away from the cutoff, we cannot maintain the assumption that subsidies are as-good-as-randomly assigned. However, Angrist & Rokkanen (2015) note that, unlike in other settings, selection into treatment in RD designs is entirely determined by the running variable — in our case, the application score. They then show how to extrapolate treatment effects for any value of the running variable provided that two (testable) restrictions hold: (i) potential outcomes are mean-independent from the running variable conditional on a vector of covariates X; and (ii) there is common support between treated and controls on X. Both conditions (i) and (ii) hold in our case for a parsimonious vector X of firm characteristics including, among others, firm age, workers' skills, and lagged firm growth.

Conditioning on X, we can thus estimate the distribution of treatment effect along the application score as well as the overall (cost) effectiveness of the policy. The estimated cost-per-new-job at $t+6$ stands at €178,000, with a stark divide between Northern and Southern regions – €68,000 and €241,000 per job, respectively. The cost of investment shows a similar gradient, as each Euro of subsidy generates nearly three Euros of investment in the North, but only one Euro in the South.

We then show that not only the overall score obtained by the project but, also, its two components summarizing the objective indicators and the discretional evaluation obtained by local politicians are irrelevant for the outcome conditional on X; leveraging on this result, we can characterize the heterogeneity of treatment effects along these two sub-scores. Successful applicants ranking high on either dimension generate the largest percent effect on employment upon receiving the subsidy: the effect increases from +11% among applicants scoring in the first quintile of both sub-scores to 16% among applicants reaching the top quintile of either sub-score, and to 19% among applicants in the top quintile of both sub-scores. However, the same percent increase translates into a lower *number* of jobs-per-euro-of-subsidy for high-on-discretion applicants than for high-on-rules applicants, because the former are on average smaller — so the same percent increase corresponds to a lower number of new jobs — and, in addition, they demand larger subsidies than other applicants.

To gauge the economic implications of political discretion, we compare the overall cost-per-new-job under two counterfactual policies: the first policy completely ignores the subjective preferences of local politicians, thus eliminating them from the score used to rank applicants; the second policy relies exclusively on such preferences. In each case, we re-rank applicants under the new rule and integrate treatment effects over the set of firms that would be funded under the counterfactual ranking. This exercise maintains that firms' decisions to apply for L488/92 funds as well as the projects they submit are invariant to the criteria used to award the subsidies. While admittedly strong, this assumption is supported by evidence that applicants' observable characteristics as well as their projects remain very similar between the first two calls for projects, for which political discretion was not part of the selection criteria, and the two calls for projects issued immediately after the introduction of political discretion.

In the absence of political discretion, the cost per new job and the cost of new investment decrease by 11% and 13%, compared to the actual policy; at the opposite, relying exclusively on political discretion increases the cost of job creation and additional investment by 42% and 22%. Under both counterfactual policies, political discretion is particularly detrimental in economically disadvantaged Southern regions, which received more funds and had a higher cost-per-job under the actual policy. We also compute the optimal ranking of applicant firms based on the vector of observable covariates X. Adopting this alternative criterion would reduce the cost per new job by over a half. Once again, the largest benefits would accrue to southern regions.

These results contribute to a large literature on the causal impact of public subsidies on investment, employment, and economic activity. The seminal paper by Hall & Jorgenson (1967) estimates significant effects of investment subsidies in the US during the 1950s and 1960s. More recent work focused on fiscal policies targeting disadvantaged areas (e.g., Greenstone & Moretti 2003, Greenstone, Hornbeck & Moretti 2010, Bloom, Brynjolfsson, Foster, Jarmin, Patnaik, Saporta-Eksten & Van Reenen 2019) or stimulating recovery after recessions (e.g., Wilson 2012, Chodorow-Reich, Feiveson, Liscow & Woolston 2012), generally finds positive impacts on employment and output.[1] Most of these previous papers measure policy effectiveness by the cost-per-new-job, reporting figures that are remarkably similar to those we find here; see Chodorow-Reich (2019), Bartik (2020), and Slattery & Zidar (2020) for recent surveys.[2]

Turning to Europe, Becker, Egger & von Ehrlich (2010) and Becker, Egger & Von Ehrlich (2013) evaluate the impact of the ERDF (which also contributed to the budget of L488/92) across European regions. They find that eligibility for additional funds increases GDP growth by 1.6 percentage points, but has no significant effect on employment. The size of the effect varies dramatically with the "absorptive capacity" of recipient regions, as determined by human capital endowments and quality of local governments. The importance of political economy constraints for the allocation of transfers across European regions is investigated, both theoretically and empirically, in a closely related work by D'Amico (2021). He argues that voting by low-skilled workers distorts spending away from technological development, innovation, and research precisely in regions where these activities are most needed.

The above papers only rely on aggregate, regional-level data, while firm-level evidence on the direct effects of public subsidies on firm investment and employment remains limited. Notable exceptions include Criscuolo, Martin, Overman & Van Reenen (2019), who estimate a positive effect of the UK Regional Selective Assistance on firm investment and employment; and Bronzini & Iachini (2014), who find, instead, that subsidies for R&D in a single Italian region were largely ineffective in raising investment. Compared to these previous papers, we extensively characterize the distribution of treatment effects across different types of firms, and we compute the cost-effectiveness of public subsidies under alternative allocation criteria.

Two previous papers have evaluated the effects of L488/92. Using a difference-in-differences approach, Bronzini & de Blasio (2006) estimate positive impacts on firm investment in the first two years after receiving the subsidy, followed by a negative impact over longer time horizons. Based on this evidence, they conclude that funded firms simply brought forward already-planned investment projects, so the net effect on firm investment is not different from zero. Using an RD approach, Cerqua & Pellegrini (2014) reach an opposite conclusion, namely positive effects on investment and employment, but they estimate a much lower cost-per-job than we do – €60,000-€100,000 at 2010 prices, compared to almost €200,000 in our case. This

[1] A related strand of literature estimates the local effects of enterprise zones, both in the United States (Bondonio & Greenbaum 2007, Ham, Swenson, İmrohoroğlu & Song 2011, Busso, Gregory & Kline 2013) and in Europe (Gobillon, Magnac & Selod 2012, Mayer, Mayneris & Py 2017, Ehrlich & Seidel 2018).

[2] In Section 6.2, we compare in more detail our estimates of the cost-per-job of L488/92 subsidies with this existing evidence.

difference may reflect different samples, data coverage, and methodology. Specifically, Cerqua & Pellegrini (2014) include only data on the second, third, and fourth call for projects in six Southern regions (1,702 applicant firms in total), while our data cover almost all calls for projects and regions (over 40,000 projects submitted by 27,000 firms). Most importantly, our methodology allows us to also estimate the effect of the subsidy on inframarginal firms away from the cutoff, and uncovers considerable heterogeneity in cost-per-job across different types of firms.[3]

Finally, we improve on previous evaluations of L488/92 and similar policies by computing treatment effects and cost-effectiveness under alternative allocation criteria – notably, along the rules vs. (political) discretion trade-off. In this respect, we contribute to a burgeoning literature on the effect of discretion for the effectiveness of public policies. In a field experiment conducted in Pakistan, Bandiera, Best, Khan & Prat (2020) find that shifting authority from monitors to procurement officers reduces prices without reducing quality. In the Italian context, several papers estimate the impact of a series of reforms implemented between 2008 and 2011 that increased from €100,000 to €1 million the value of procurement contracts that could be awarded under discretionary procedures. Overall, greater discretion does *not* deteriorate observable procurement outcomes (Coviello, Guglielmo & Spagnolo 2018), but its effect varies dramatically across procuring agencies. In particular, the use of discretionary procedures by less transparent and less qualified procuring agencies increases the probability of selecting politically connected firms (Baltrunaite, Giorgiantonio, Mocetti & Orlando 2018) and firms owned or run by individuals with a criminal record (Decarolis, Fisman, Pinotti & Vannutelli 2020).[4] Compared to these previous studies, the institutional features of L488/92 provide us with an observable indicator of politicians' preferences – as measured by the sub-component of the applicant score decided by local politicians – allowing us, in turn, to estimate policy effectiveness under different levels of discretion. This is particularly relevant in the Italian context, which is characterized by pervasive political clientelism as well as by an important role of the state in the economy (see, e.g. Golden & Picci 2008, Cingano & Pinotti 2013).

In the next section we describe the institutional context, and in Section 3 and 4 we introduce the data and empirical strategy. In Section 5 we show the results for marginal firms near the cutoff,

[3]In addition, we consider institutional rules and budgets prioritizing specific categories of firms and projects (e.g., those presented by small firms and projects eligible for EU funds), which are crucial for correctly constructing the RD design but are neglected by Cerqua & Pellegrini (2014). We discuss these issues in detail in Section 2, and in Section S1 of the Supplementary Materials.

[4]Szucs (2017) and Baránek (2020) study the effects of bureaucratic discretion in public procurement in the Czech Republic, while Bosio, Djankov, Glaeser & Shleifer (2020) provide evidence across countries.

and in Section 6 the results for inframarginal firms away from the cutoff, the heterogeneity of treatment effects, and the overall policy effect under alternative allocation criteria. Section 7 is the conclusion.

2 Institutional framework

Italy has long been characterized by large economic divides between north and south.[5] In 2001, median value added per capita across local labor markets in northern regions (€18,500) was twice as high as that in southern regions (€9,500). The level of economic activity also varied widely *within* southern regions, with a 90/10 ratio in value added across local labor markets of 3 – compared to just 2 within northern regions.[6] At the same time, the last few decades witnessed a marked reduction in workers' mobility. In 2005, the one-year mobility rate was one third of that in the United States, and one of the lowest across countries (Molloy, Smith & Wozniak 2011).

Large territorial divides and low worker mobility provide a strong rationale for spatially-targeted subsidies (Kline & Moretti 2014, Bartik 2020). During the postwar period, southern regions received massive aid flows from both the Italian government and the European Union. Between the mid-1990s and mid-2000s, Law 488/92 was the main policy instrument employed by the central government to allocate these funds across regions as well as across (private) investment projects within each region. The aim of L488/92 was to "stimulate fixed investment in underdeveloped areas of the country", and interventions had to be "concentrated in poor areas and in sectors with the highest social returns [in terms of employment]" (UVAL 2012). The law passed at the end of 1992 but became effective only in 1996, and it remained in place until 2007; the total budget over this period was €26 billion (at constant 2010 prices).

Several categories of projects were eligible to receive L488/92 subsidies: industrial projects aimed at creating, expanding, and modernizing establishments; projects relating to the production and distribution of energy, steam or hot water; projects relating to the construction sector, and lastly, IT sector projects (up to 5% of the program total budget).

[5]Italy is divided into 20 regions, corresponding to level 2 of the European "Nomenclature of Territorial Units for Statistics" (NUTS). Throughout the paper, the term "northern regions" refers to regions classified as North and Center by the Italian National Statistical Institute (ISTAT) – 8 and 4 regions, respectively.

[6]Local labor markets are clusters of contiguous municipalities defined by ISTAT on the basis of workers' commuting patterns – similar to the US commuting zones. For additional details, see https://www.istat.it/en/labour-market-areas.

Funds were allocated through open calls for tenders, each one targeting a specific economic sector – primarily industry, but also tourism and trade – and the funds available for each call were then allocated across the 20 Italian regions. Table 1 shows the distribution of L488/92 funds across sectors and regions over the entire period 1996-2007. Industry obtained the lion's share (€21.9 billion), followed by tourism (€2.7 billion). In line with the main objectives of the policy, almost 85% of the funds were allocated to less economically developed areas in the South. For instance, two of the poorest regions of the country, Campania and Sicily, received nearly €6 billion and €5 billion, respectively, compared to €0.25 billion and €0.13 billion for Lombardy and Emilia Romagna.[7]

Table 1: *L488/92 funds by geographical region, source of funds, and economic sector*

	All Italy	**North**	**Center**	**South**
Total funds	25.98	2.34	1.68	21.95
Allocation across economic sectors				
Industry	21.89	1.97	1.37	18.55
Tourism	2.68	0.21	0.19	2.28
Trade	0.73	0.06	0.06	0.61
Special	0.45	0.09	0.05	0.31
Craftwork	0.23	0.02	0.01	0.20
Source of funds				
National	19.77	1.95	1.52	16.30
EU	6.21	0.39	0.17	5.65

Notes: This table shows the allocation of L488/92 budget by geographical area and economic sector, as well as the source of funding. All amounts are expressed as billion euros at constant 2010 prices.

Projects submitted by applicant firms within each call-region were then ranked and funded. The ranking was based on quantitative indicators of project quality, combined with rules regarding minimum quotas of L488/92 funds reserved for specific categories of applicants (e.g., small-medium firms) or eligibility for co-financing with EU funds.

[7]Online Appendix Figure A1 shows a clear, negative relationship between L488/92 funds and regional GDP per capita, while Figures A2 and A3 provide additional descriptive evidence on the evolution and composition of funding over time and across geographical areas.

2.1 Ranking projects on objective rules (1996-1997)

In the first two calls for projects, issued respectively in 1996 and 1997, there were three indicators of quality:

I1) the ratio of the applicant's own investment in the project relative to the amount requested ("*skin in the game*");

I2) the number of jobs created by the project ("*job creation*");

I3) the proportion of funds requested in relation to an ad-hoc benchmark set by the EU Commission ("*no waste*").

Indicators 1 and 3 captured the entrepreneurial stake in a project, privileging projects with a higher level of involvement, while indicator 2 follows naturally from the main goal of L488/92, namely stimulating employment. The information required to compute these three indicators was elicited directly in the application for funds and it was transmitted to the Ministry of Economic Development by the local branches of a set of authorized banks, which were also in charge of a preliminary screening of applications.

The three numerical indicators were standardized within each call and region and combined into a single *score* of project quality as follows:

$$S_i = \sum_{j=1}^{3} \frac{(I_{ir}^j - \mu_r^j)}{\sigma_r^j}, \tag{2.1}$$

where I_{ir}^j is the value of the j-th indicator for project i in call-region r, and μ_r^j and σ_r^j are the mean and the standard deviation of the same indicator across all projects presented in the same call-region.

2.2 Adding political discretion (1998-2007)

Starting from the third call for project, issued in 1998, two additional indicators were introduced:

I4) points attributed by the regional government on the basis of its priorities ("*political discretion*");

I5) compliance with the requirements of an environmental management system, e.g. ISO 14001 or EMAS ("*environmental responsibility*").

The introduction of indicator I4 followed a general tendency toward federalism and decentralization initiated with Law 59/1997 – the so-called "Bassanini" Law, by the name of Ministry of the Public Administration at that time. Such law delegated the central Government to transfer to the 20 Italian Regions "all administrative functions and duties promoting the development of their respective communities", including "the discipline relating to economic and industrial activities concerning, in particular, the support and development of firms in manufacturing, commerce, in the agro-industrial sector and in production services". In the specific case of L488/92, regional governments could assign 0-10 points to municipalities within the region and industrial sectors in which the project could be realized, and the type of investment to be implemented (e.g., "new productions", "expansion of existing productions", "change of line of work", and so on). Importantly, the allocation of points by municipality-industry-type of investment had to be set ex-ante and communicated by the regional government to the Ministry of Economic Development by October 30th of the year *before* each call was issued, and it was not circulated publicly. Indicator I4 equalled the (normalized) sum of points obtained along each dimension depending on the municipality and industrial sector in which it was realized, and the type of investment.

To compute the overall project score in the calls after 1998, the new indicators I4 and I5 were standardized and aggregated using the same formula as in (2.1).[8] For the same period, we refer to the sum of the first three (standardized) indicators, I1-I3, as the sub-score for objective "rules", SR (or the "objective sub-score"), and to the standardized indicator I4 as the sub-score for "discretionary", SD (or the "discretionary sub-score"). Since the indicator for environmental responsibility, I5, cannot be clearly defined as either objective or discretionary, we include it neither in SR nor in SD.

[8] In some calls, the fifth indicator (I5) was not added to all others to form the final score of a project. Rather, if the project was compliant with the environmental certifications, I5 increased by 5% all other sub-scores. In such cases the correct formula for the score is

$$S_i = \sum_{j=1}^{4} \frac{I_{ir}^{j} \times I_{ir}^{5} - \mu_r^{j}}{\sigma_r^{j}}$$

where $I_{ir}^5 = 1.05$ if the applicant i is compliant with environmental certification, and $I_{ir}^5 = 1$ otherwise.

2.3 Ranking and funding of projects

To determine the allocation of funds in each call for projects, applicants were ranked by the overall score S within each region on the basis of additional rules prioritizing specific categories of applicants and projects. There were three such rules:

1. at least 50% of the budget within each region was reserved for small-medium enterprises, defined as those having fewer than 250 employees and either a turnover smaller than €50 million or a balance sheet smaller than €43 million;

2. at most 5% of the budget within each region could be allocated to firms operating in the service sector;

3. projects eligible for additional co-funding from EU structural funds were funded ahead of higher-ranked projects eligible only for national funding.

These rules, which are explained in more detail in Section S1 of the Supplementary Materials, define multiple sub-rankings within each regional ranking published in the *Gazzetta Ufficiale*. We recovered these multiple sub-rankings exploiting additional information on firm size, sector, eligibility for co-financing, and geographical area, also provided in the *Gazzetta Ufficiale*, and identify "*cells*" of firms competing for L488/92 funds within the same call, region, and (possibly) special category of applicants.[9]

The outcome of the selection process was published within four months of applications closing, and subsidies were paid to winning applicants in three equal instalments. The first instalment was paid within two months of the publication of the ranking, while the other two were paid one and two years later, conditional on compliance with the planned execution of the project. The second instalment was paid only if 2/3 of the project had been realized, while the last one was only paid if the project had been completed; if this was not the case, either or both of the last two instalments were not paid, and the firm would have to repay previous instalment(s) plus an additional fine. This monitoring system ensured coherence between the projects proposed in the applications and their execution.

[9]Previous evaluations of L488/92 constructed the RD design only by call and region. In the Supplementary Materials, we explore the implications of neglecting the existence of special categories of applicants, and we provide a more precise explanation of how we correctly identify our cells of applicants.

3 Data

Our analysis is based on a unique dataset combining administrative data on applications for L488/92 subsidies, registry data on applicant firms and their employees, and a proprietary database of balance sheet data (see Online Appendix Section B for a description). The Italian Ministry of Economic Development provided detailed information on all applications from 26 calls for L488/92 funds made between 1996 and 2007. These data cover 74,584 projects worth almost €22 billion (out of a total of €26 billion funded by L488/92), submitted by 49,082 firms; see Online Appendix Table A1.[10] For each project, the dataset reports the fiscal identifier of the applicant firm, together with its location and sector; the subsidy requested; the applicant's final score and its components (I1-I3 in 1996-97, and I1-I5 after 1998); and the amount eventually awarded to applicants scoring above the cutoff. We complemented these data with additional information from the *Gazzetta Ufficiale* so we could identify the cell of applicants competing for funds within the same call, region, and category, as explained in the previous section.

Nearly 33,000 projects scored above the cutoff and were thus eligible for funding. About 20% of these projects were not funded, eventually, for a number of reasons such as (i) failure to provide the documentation showing compliance of the investment with the conditions and limits set in the auction; (ii) non-compliance with nation-wide legislation concerning, e.g., labor laws, environmental or urban real estate legislation; (iii) large deviations from the targets underlying the objective score; (iv) violations of the non-cumulation requirements; (v) the rental, disposal or sale of the subsidized asset. Unfortunately, our data do not report the exact reason why the subsidy was not paid out to each eligible applicant. As explained in the next section, we will focus throughout the paper on the effect of being eligible for the subsidy (i.e., scoring above the cutoff), which provides a lower bound to the effect of actually receiving the subsidy. In any event, we will discuss the implication of (non-random) selection into receiving the subsidy among eligible applicants whenever it is relevant for interpreting our results – particularly, when comparing the effect between applicants selected on objective rules vs. political discretion.

The second source of data are the administrative registries of the Italian Social Security Institute (INPS), which cover the universe of Italian firms with at least one employee (around 1.6 million firms each year). These data report firm employment at monthly frequencies as well as business foundation and cessation dates. We can therefore precisely track job numbers at applicant firms, both before and after applying for (and possibly obtaining) the subsidy, as well as firms' survival

[10] The dataset did not cover 5 of the 35 calls (21, 24, 25, 26, 30), while for 4 of the included calls (5, 18, 23, 34) we could not retrieve firm-level subsidies.

rates over long periods of time.

Unfortunately, the fiscal identifier of sole proprietorships is typically anonymized in the INPS registries, so we lose about 20 thousand applications by micro-enterprises. When estimating the dynamic treatment effects of the subsidy, we drop another 10 thousand applications from firms that first appeared in the INPS data on the year of the call (i.e., start-up firms), as the credibility of our empirical strategy relies on the dynamics of outcomes in the period before the call. We also trimmed the top 1% of firms in terms of size, which employ on average 5 thousand workers (i.e., 100 times the median firm size in our sample). These are the dominant firms in high-returns-to-scale industries (e.g. utilities, automotive, or chemicals), which would be difficult to reliably match to comparable units. We checked that none of these sample restrictions significantly affects our results. Our main analysis of employment effects will rely on a sample of 40,366 projects submitted by 27,074 different firms.

For the vast majority of our sample, we also retrieved detailed balance sheet information from the Firm Register managed by the Cerved group. Cerved is a proprietary database covering all limited liability companies incorporated in Italy, including nearly 70% of firms in the matched L488/92-INPS data described above (18,476 companies, corresponding to 27,856 distinct projects). For this set of firms, we thus observe additional outcomes such as investment, revenues, and value added. Importantly, this final sample matches the initial population of applicants on the main variables included in both datasets.[11]

Table 2 shows the distribution of the main variables in our dataset. In line with the traditional characteristics of Italian firms (see, e.g., Schivardi & Torrini 2008), applicant firms are generally small in size: the average (median) number of employees equals 36.4 (11.5), and one fourth of firms have 3 employees or less. The distribution in terms of assets and revenues is also very skewed, the average of both variables being around €20 million and the median remaining just above €2 million.

Table 2 also shows that the average applicant demands just below €700 thousand and 45% of applications score above the cutoff, meaning that they are in principle eligible for funding; of these, 80% are actually funded, while the remaining ones are not for the reasons explained above.

[11]Online Appendix Figure A4 compares the distribution of requested and awarded subsidy, score obtained by the project, and the score sub-component for the (planned) number of newly created jobs between the original sample including all applications and the final sample of applicants matched with administrative data.

Table 2: *Characteristics of applicant firms*

	Average	P_{10}	P_{25}	P_{50}	P_{75}	P_{90}	**Obs**
Panel A: Administrative data on L488/92							
Funds requested	685	61	130	303	697	1448	40366
Score $\geq$ cutoff	0.45	0	0	0	1	1	40366
Project funded $\mid$ Score $\geq$ cutoff	0.80	0	1	1	1	1	40366
Panel B: Social security data (INPS)							
Firm age	11	0	3	8	17	26	40366
Firm size	36	1	4	12	30	74	40366
Employment growth ($t-1$)	0.20	-0.15	-0.02	0.07	0.25	0.71	36713
Share blue collar	0.69	0.20	0.59	0.76	0.88	1.00	40366
Average firm wage	19.96	11.73	14.56	17.79	22.41	28.94	40366
Industrial	0.72	0	0	1	1	1	40366
South	0.67	0	0	1	1	1	40366
Has apprentices	0.32	0	0	0	1	1	40366
Has managers	0.14	0	0	0	0	1	40366
Employment growth ($t+6$)	0.28	-0.60	-0.10	0.21	0.66	1.37	35156
Survive ($t+6$)	0,87	0	1	1	1	1	40366
Panel C: Balance sheet data (CERVED)							
Total Assets	13153	394	1044	2921	8434	24173	27856
Total Revenues	12784	311	925	2780	8377	24545	27384
Investment rate	0.08	0	0	0.02	0.10	0.22	27856
Cumulated investment ($t+6$)	4189	159	447	1253	3357	8790	19273
Revenues growth ($t+6$)	0.46	-0.36	0.09	0.44	0.85	1.41	18516

Notes: This table shows the mean and some percentiles of the main variables in our dataset, together with the number of non-missing observations. All amounts are expressed in thousand €at constant 2010 prices.

3.1 Evidence on political discretion

Before turning to estimating the treatment effect of subsidies, we examine the implications of political discretion for their allocation across firms. As explained in Section 2.2, the discretionary sub-score SD should in principle reflect "macro" priorities by the regional governments (in terms, e.g., of targeting investment to specific geographical areas and industrial sectors), yet one cannot exclude that local politicians could tailor the allocation of points around specific applicants. For instance, they could attribute more points to municipalities and industries in which some politically connected firms operate. On the other hand, the discretionary sub-score obtained by a given project could not respond directly to the value of the other (objective) indicators because, as explained in Section 2.2, regional governments assigned discretionary points by municipality-industry-type of investment before calls for projects were posted and the allocation of points was not circulated publicly.

In light of these institutional features, it remains unclear whether political discretion was used to favor specific applicants – as opposed to broader policy objectives. To shed light on this issue, we explore the determinants of both the discretionary and objective sub-scores – SD and SR, respectively – using LASSO regression, a machine-learning algorithm for selecting the subset of relevant regressors while estimating at the same time their relationship with the dependent variable of interest. Specifically, LASSO minimizes the following loss function:

$$\min_{\theta} \left\{ \sum_{i=1}^{n} (y_i - z_i'\theta)^2 + \lambda \sum_{j=1}^{k} |\theta_j| \right\}, \tag{3.1}$$

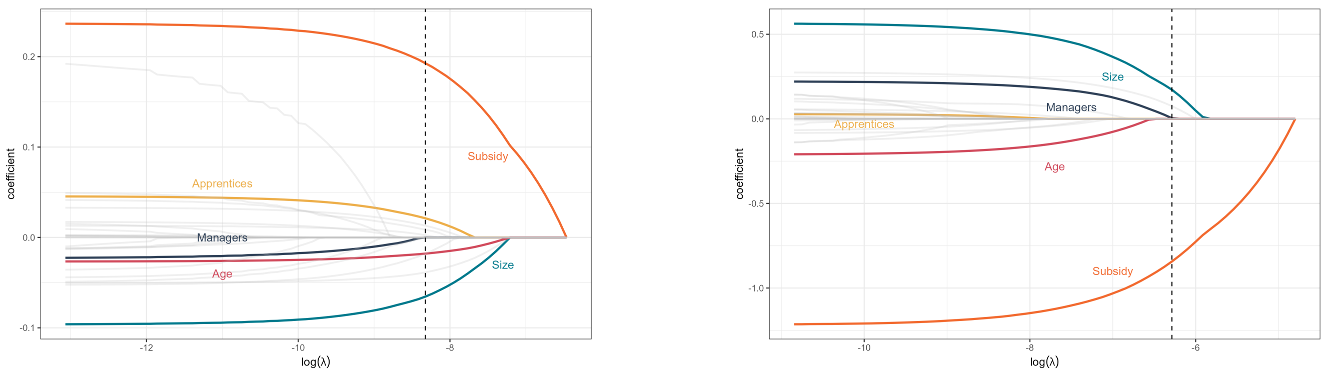
where y_i is the outcome variable of interest (in our case, the sub-scores SR and SD) in the sample $i = 1, 2, ..., N$; z_i is a k-th dimensional vector of predictors, and θ is the vector of associated parameters of interest; finally, $\lambda \geq 0$ is a "shrinkage" parameter. When $\lambda = 0$ the loss function is equivalent to that for OLS, thus $\theta = \theta^{0LS}$, while $\lambda > 0$ introduces an additional penalty for non-zero coefficients. Hence, as λ moves away from 0, the coefficients of variables with lower explanatory power are shrunk to 0. We set the optimal λ using cross-validation and the one-standard-deviation rule (James, Witten, Hastie & Tibshirani 2013).

The vector z_i includes a wide array of applicant characteristics such as age, size, and industrial sector; measures of local economic development, namely unemployment rates and credit constraints at the provincial level;[12] and political proximity between the regional government and the municipality in which the applicant firm is located. Online Appendix Table B1 (second and fourth panels) reports the complete list of variables and their description. Figure 1i and Figure 1ii plot the estimated effect θ of each variable on SD and SR as λ varies, together with the optimal λ (vertical dashed line). The most important predictors are firm size (in terms of number of employees) and the subsidy demanded by the applicant firm. Interestingly, such variables predict SD and SR in opposite directions. In particular, smaller firms are penalized on the objective indicator about job-creation (I2), as they cannot compete with large firms on this dimension, but they receive on average more points on the discretionary score; similarly,

[12] Credit constraints are measured by the spread between loan and deposit rates in local credit markets, from Guiso, Pistaferri & Schivardi (2013).

firms demanding larger subsidies are penalized on the objective indicators I1 and I3, but they receive more points from the regional government.[13] These patters are also shown in Online Appendix Figure A5, which plots either of the sub-scores SD and SR, on the horizontal axis, against average firm size and log of subsidy amount, on the vertical axis, controlling for cell fixed effects.[14]

Figure 1: *Predictors of the discretionary sub-score (SD) and the objective sub-score (SR).*



(i) *SD, main predictors* **(ii)** *SR, main predictors*

Notes: Panel i) and ii) report the LASSO coefficients in the regression of SD and SR on a rich set of covariates for various penalty parameters λ. The black dashed line denotes the optimal value of the penalty parameter according to the one-standard-deviation rule (see James, Witten, Hastie & Tibshirani 2013, for additional details). The full list of included predictors is: (log) amount requested, average firm wage, size of firm, share of blue collars, employment growth, wage growth, industry, age, presence of managers, presence of apprentices, margin of victory at the last local elections, political connection and political alignment at the municipality, province, and regional level. More information on the source and the description of these variables can be found in Online Appendix Table B1.

By contrast, local unemployment rates and credit constraints have very little explanatory power, meaning that neither objective indicators nor political discretion favor firms located in the most disadvantaged areas within each region – though the budget allocation across regions unambiguously favors the poorer. Similarly, measures of political proximity predict neither SR nor SD. Therefore, there is no evidence that local politicians can tailor the points around applicant firms that are politically close, with the caveat that we only observe indirect measures of political proximity (e.g., political alignment between the regional government and the municipality in which the firm is located or the political contestability of such municipality; see Online

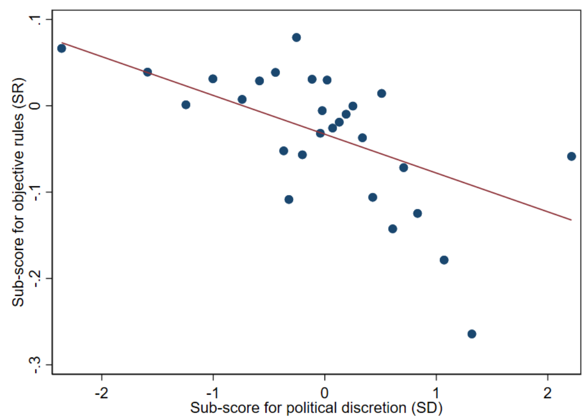
[13] Since both firm size and the amount of subsidy enter the LASSO regression in logs, the latter variable should be interpreted as the log of subsidy-per-worker.

[14] We look at within-cell variation because applicants are ranked within cells.

Appendix Table B1).[15]

Finally, Figure 2 shows that SD and SR are inversely correlated, which is not entirely surprising given that they are correlated in opposite direction with the most important predictors. Indeed, controlling for size and the subsidy amount the relationship between SR and SD flattens out.

Figure 2: *Political discretion and objective rules*



Notes: This graph plots the sub-score for political discretion against that for objective rules, controlling for cell fixed effects, across equally-sized bins.

4 Empirical Strategy

Let Y^1_{ic} and Y^0_{ic} be the potential outcomes of applicant firm i competing in cell c, as defined by the call-region-category of applicant, when obtaining the subsidy – the "treatment" – and not obtaining it, so $Y = DY^1 + (1 - D)Y^0$ is the realized outcome. In addition, let $\tilde{S}_{ic}$ be the score received by firm i and $\bar{S}_c$ be the cutoff score required for obtaining the subsidy in cell c, so $S_{ic} = (\tilde{S}_{ic} - \bar{S}_c)$ is the normalized score for each firm ($S_{ic} = 0$ at the cutoff). Finally, let D_{ic} be a "treatment assignment" variable equal to 1 whenever $S_{ic} \geq 0$, and equal to zero otherwise.

As discussed in the previous section, there is one-sided non-compliance with treatment assignment: about 20% of applicants scoring above the cutoff do not receive the subsidy, for various reasons, while no applicant scoring below the cutoff is subsidized. To the extent that

[15] Unfortunately, precise measures of political connections at the firm-level (as in, e.g., Cingano & Pinotti 2013) are not available for our sample of applicant firms.

D is as-good-as-randomly-assigned across applicants that are arbitrarily close to the cutoff – an assumption that seems very plausible in the present context, and that we validate in the next section – the difference in outcomes between applicants just above and just below the cutoff,

$$\tau = \lim_{s \to 0^+} \mathbb{E}[Y \mid S = s] - \lim_{s \to 0^-} \mathbb{E}[Y \mid S = s],$$

identifies the reduced form (or "intention-to-treat", ITT) effect of scoring above the cutoff. Throughout the paper we focus on estimating this parameter, which provides a lower bound (in magnitude) to the average-treatment-on-the-treated (ATT) effect of receiving the subsidy. Under standard conditions, notably that treatment assignment (i.e., scoring above the cutoff) affects outcomes only through the actual treatment (i.e., receiving the subsidy), the ATT effect equals the ITT effect divided by the share of compliers (roughly 0.8 in our context, see Section 3).

We pool the data across all cells and estimate τ parametrically by regressing firm outcomes on the dummy for receiving the subsidy, D, controlling for a p-th order polynomial in the score S and its interaction with D:

$$Y = \tau D + \sum_p \gamma_p S^p + \sum_p \delta_p D \cdot S^p + FE_c + \varepsilon, \tag{4.1}$$

where FE_c is a fixed effect for cell c and ε is a residual term summarizing the effect of other factors. Following Fort, Ichino, Rettore & Zanella (2022), we include fixed effects at the cell level to control for the fact that the cutoffs are endogenously determined. We restrict the sample to applicants with an application score within the bandwidth $[-5, 5]$ (82% of our sample), and we use linear and quadratic polynomials in S. We also experiment with triangular kernels attaching greater weight to observations closer to the cutoff.[16]

Under the assumption that other determinants of Y vary smoothly at the cutoff (conditional on the polynomial in S), the coefficient τ in equation (4.1) identifies the average effect of the subsidy across firms near the cutoff (Lee 2008). However, treatment effects for inframarginal firms away from the cutoff – and, thus, the overall policy effect – are not identified in general, as

[16] As recommended by Gelman & Imbens (2019), we present baseline results for linear and quadratic specifications of equation (4.1) (i.e., $p \leq 2$), but we show in Figures A7 and A8 of the Online Appendix that all results are virtually identical for $p = 0$ and $p = 3$, and they are even stronger when estimating non-parametric RD regressions for different multiples of the optimal bandwidth around the cutoff (as defined by Calonico, Cattaneo & Titiunik 2014).

high- and low-scoring firms may differ along some unobservable dimension (e.g., managerial ability). However, Angrist & Rokkanen (2015) note that, in RD designs, treatment assignment is fully determined by the running variable – in our case, the application score – which is therefore the only source of selection bias. Hence, if there exists a set of covariates X such that potential outcomes are mean independent of the running variable conditional on X,

$$\mathbb{E}[Y^j \mid S, X] = \mathbb{E}[Y^j \mid X], \quad j \in \{0, 1\}, \tag{4.2}$$

then one can estimate treatment effects for any $S = s'$ by comparing treated and controls conditional on X. The conditional (mean) independence assumption (CIA) in equation (4.2) implies that potential confounders (e.g., high-scoring firms being better-managed) would be either absorbed by X or uncorrelated with the outcome of interest.

To be more specific, following Angrist & Rokkanen (2015) let the running variable S be a function $S = g(X, u)$ of some observable variables X and some unobservable variables u, such as managerial ability. If potential outcomes are mean independent from the score S conditional on X, then controlling for X is sufficient to eliminate selection bias when comparing units away from the cutoff. This is because conditioning on X makes potential outcomes independent from S and, thus, from the treatment status D. Therefore, variables that would potentially bias estimates of treatment effects away from the cutoff are either included in X or in u; in the former case, we control for them, and in the latter we can safely ignore them. In addition to the CIA condition in (4.2), this approach requires common support between treated and controls with respect to X,

$$0 < \mathbb{P}(D = 1 \mid X) < 1. \tag{4.3}$$

The key point made by Angrist & Rokkanen (2015) is that the CIA is partially testable. That is, the RD design provides a test for the usually untestable assumption that conditioning on X removes all confounding differences between treated and controls. In addition, it is staighforward to check whether the common support condition (4.3) holds. If both conditions hold, we can rewrite the treatment effect at $S = s'$ as

$$\mathbb{E}[Y^1 - Y^0 \mid S = s'] = \mathbb{E}[\mathbb{E}[Y \mid X, D = 1] - \mathbb{E}[Y \mid X, D = 0] \mid S = s']. \tag{4.4}$$

Following Angrist & Rokkanen (2015), we estimate (4.4) using the linear reweighting estimator by Kline (2011):

$$\mathbb{E}[Y \mid S, X, D = 1] = \sum_q \alpha_q^1 S^q + X'\beta^1 \tag{4.5}$$

and

$$\mathbb{E}[Y \mid S, X, D = 0] = \sum_q \alpha_q^0 S^q + X'\beta^0. \tag{4.6}$$

Failure to reject the restrictions $\alpha_q^1 = \alpha_q^0 = 0$, $\forall q = 1, \ldots, Q$, provides partial evidence consistent with the CIA in (4.2), the untestable part being that the same restrictions hold for the counterfactuals of treated and untreated units.

If such restriction holds, we can indeed substitute (4.5) and (4.6) into (4.4), to obtain

$$\mathbb{E}[Y^1 - Y^0 \mid S = s'] = (\beta^1 - \beta^0)' \, \mathbb{E}[X \mid S = s']. \tag{4.7}$$

We can estimate equation (4.5) across treated units and (4.6) across non-treated units, retrieve predicted outcome values, and take their difference to estimate (4.7). If common support (4.3) holds, this method allows us to characterize treatment effects all over the support of the running variable S.[17]

Adapting the strategy in Angrist & Rokkanen (2015) to our case, we can characterize the heterogeneity in treatment effects along the distribution of sub-components of the score, SR and SD: provided that the CIA holds,

$$\mathbb{E}[Y^j \mid SR, SD, X] = \mathbb{E}[Y^j \mid X], \quad j \in \{0, 1\}, \tag{4.8}$$

we can estimate conditional average treatment effects as

$$\mathbb{E}[Y^1 - Y^0 \mid SR = sr', SD = sd'] = (\beta^1 - \beta^0)' \, \mathbb{E}[X \mid SR = sr', SD = sd']. \tag{4.9}$$

Equation (4.9) will allow us to assess the contribution of objective rules and political discretion, respectively, to the effectiveness of public subsidies.

[17] In a companion paper, Palomba (2022) introduces a new Stata package, `getaway`, which implements different methods for extrapolating RD estimates away from the cutoff together with several tests and graphical tools. The package is available at https://github.com/filippopalomba/getaway-package.

4.1 Evaluating the impact of counterfactual assignment rules

Here we show how to evaluate the impact of counterfactual assignment rules under a policy invariance condition. The policy invariance condition we need for this exercise requires that neither the distribution of applicant firms with respect to their characteristics relevant for the outcome nor the characteristics of the projects they submit are both not affected by a change of the rules to assign subsidies. Let F_Z^a and F_Z^{cf} be the distributions of the pool of applicants with respect to the characteristics Z relevant for the outcome under, respectively, the actual and the counterfactual assignment rule and let $SR^a(i)$ and $SR^{cf}(i)$ be the objective sub-score obtained by the i-th project under, respectively, the actual and the counterfactual assignment rule. The policy invariance condition we invoke is

$$F_Z^a \sim F_Z^{cf} \text{ and } SR^a(i) = SR^{cf}(i), \quad \forall\, i. \tag{4.10}$$

In principle, this condition could be violated for several reasons. For instance, some applicants may be more or less willing to apply once local politicians can directly intervene in the scoring process, or they may submit projects that are more in line with the priorities of the regional government in terms of location, industrial sector, and type of investment (see Section 2.2). However, the priorities communicated by regional governments to the Ministry of Economic Development were not disclosed publicly, so it is unclear whether firms took those into account when preparing their applications. Although assumption (4.10) is not immediately testable, in Section 6.4 we provide evidence consistent with such assumption by comparing, difference-in-differences, the number and quality of projects (as measured by the sub-score for objective rules, SR) of applicants before and after the introduction of discretion, and between regions in which the regional government decided not to use discretion and other regions.

Holding condition (4.10), we shall use the CIA condition (4.8) to assess how the average impact of subsidies would change under alternative assignment rules, e.g. under an assignment rule based only on the sub-score for objective rules, SR. Condition (4.8) plays a crucial role in this exercise, since it implies that conditioning on X the objective sub-component SR is as good as random, thus allowing us to evaluate what the average impact would be at each point of the support of SR:

$$\mathbb{E}[Y^1 - Y^0 \mid SR = sr'] = (\beta^1 - \beta^0)' \, \mathbb{E}[X \mid SR = sr']. \tag{4.11}$$

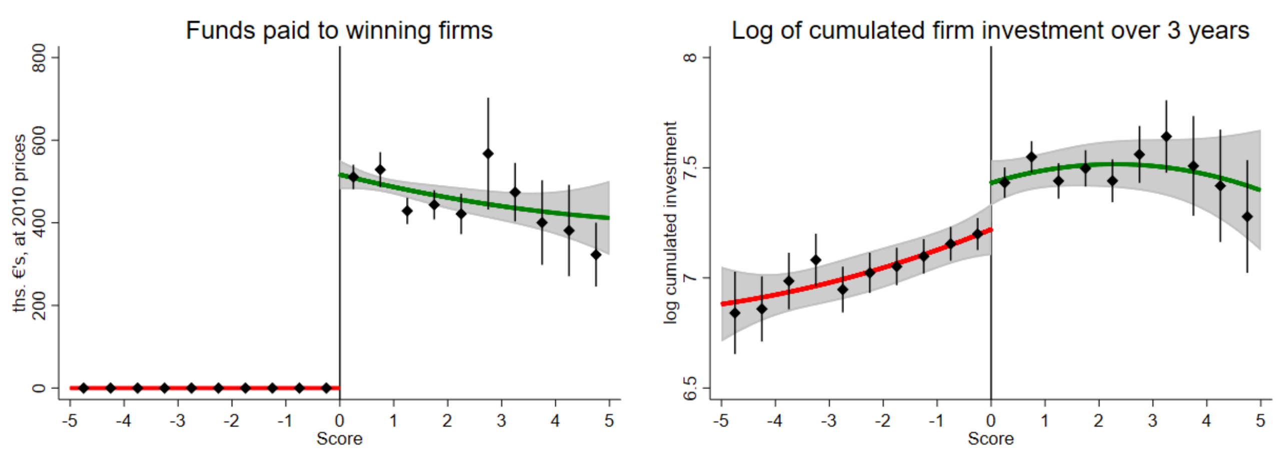
Then, by aggregating over the support of SR, we get the average impact under the counterfactual assignment rule based only on the objective sub-component of the index.

5 Results at the RDD cutoff

Figure 3 plots the relationship between the score obtained by applicant firms and the subsidy they received (left graph) and the log of total, cumulated investment over the three following years (right graph). We show averages and confidence intervals for equally-spaced bins of size 0.5, together with the predicted relationship based on a polynomial quadratic specification.

The left graph confirms that only firms with a score above the cutoff are funded. Treated firms near the cutoff received on average half a million euros (at constant 2010 prices) over three years, and they significantly increased investment compared to other other (control) applicants that ranked just to the left of the cutoff; see the right graph in Figure 3.[18]

Figure 3: *Funds obtained by winning firms and investment over the following 3 years*



Notes: This figure shows the relationship between the amount of funds obtained by firms applying for L488/92 subsidies (left graph) and the log of cumulated firm investment over the following three years (right graph) against the standardized score they obtained (on the horizontal axis). Bins represent averages over equally-spaced intervals, and confidence intervals (at the 90% significance level) are also shown by vertical lines. The predicted relationships between each outcome and the score are estimated using a quadratic polynomial regression. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

[18] In the left graph of Figure 3, the relationship between the subsidy amount and the application score is negative to the right of the cutoff due to indicator I3 (“no waste"), which penalizes applicants requesting higher subsidies; see Section 2. For the same reason, in the right graph firm investment increases with the score to the left of the cutoff, as it seems intuitive, but the relationship becomes flat to the right of the cutoff, and it even turns slightly negative for very high values of the score.

Figure 4 shows that applicants ranking just above and below the cutoff are on average equal on a wide range of other characteristics measured one year before the call; Online Appendix Table A2 presents the results of formal tests. Figure 5 shows that the five components of the score, described in Section 2, also vary smoothly around the cutoff. Finally, Online Appendix Figure A6 shows no evidence of discontinuity in the density of applications.[19]

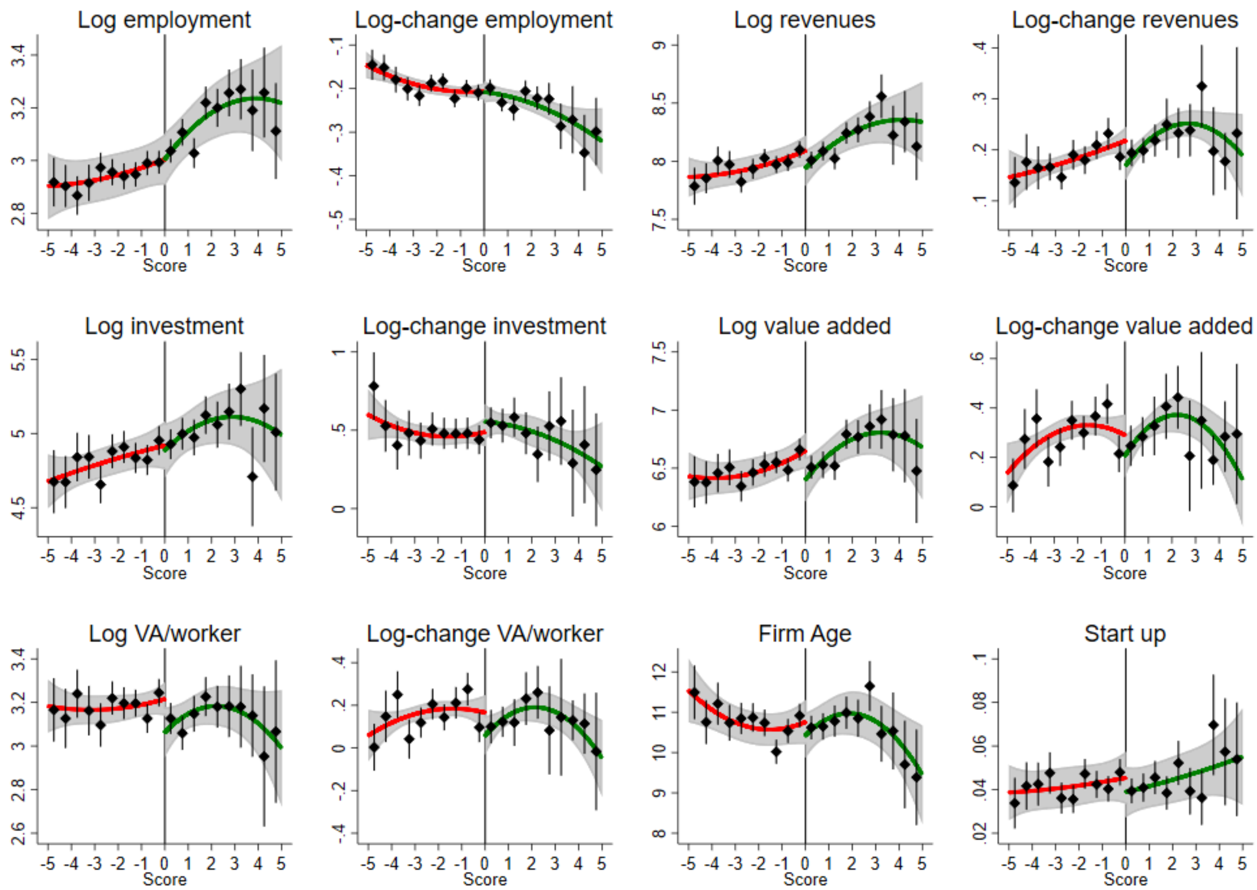
Taken together, Figures 4, 5, and A6 strongly support the main identifying assumption that applicants within an arbitrarily narrow bandwidth of the RD cutoff are unable to precisely determine their assignment to either side of it (see, e.g. Lee & Lemieux 2010). We can thus attribute any difference in outcomes between firms scoring just above and just below the cutoff to the causal effect of the subsidy.

The stated objective of Law 488 was to increase employment in disadvantaged areas, so Figure 6 shows the effect of the subsidy on the log-change of firm employment. In the year before the L488/92 call, firm employment is balanced between treated and control firms near the cutoff (see Figure 4), but the subsidy progressively opens a gap between the two groups during the following years. The gap is already noticeable one year after obtaining the subsidy (first graph); it increases at the end of the subsidy period (second graph) and persists in subsequent years (third graph).

Table 3 shows the effect of obtaining the subsidy on (log) employment and investment, as estimated from different specifications of equation (4.1). Specifically, we experiment with linear and quadratic polynomials in the running variable, uniform and triangular kernels (the latter attach a greater weight to observations near the cutoff), and including a full set of cell fixed effects; the details for each specification are at the top of each column. All results remain virtually identical under all these specifications, so we focus on the simplest linear specification with cell fixed effects throughout the paper. According to this specification, presented in column (2) of Table 3, the subsidy increases firm investment by 39 percent over the following three years (Panel A), and it increases employment by 11 percent over the same period (Panel C), and by 17 percent over a period of six years (Panel D). All these estimates are strongly statistically significant. Figures A7 and A8 in the Online Appendix replicate the analysis for employment and investment, respectively, using non-parametric methods. The results are robust to varying the bandwidth between $0.5B^*$ and $3B^*$, where B^* is the optimal bandwidth according to Calonico, Cattaneo & Titiunik (2014), and to varying the degree of the polynomial in the running variable

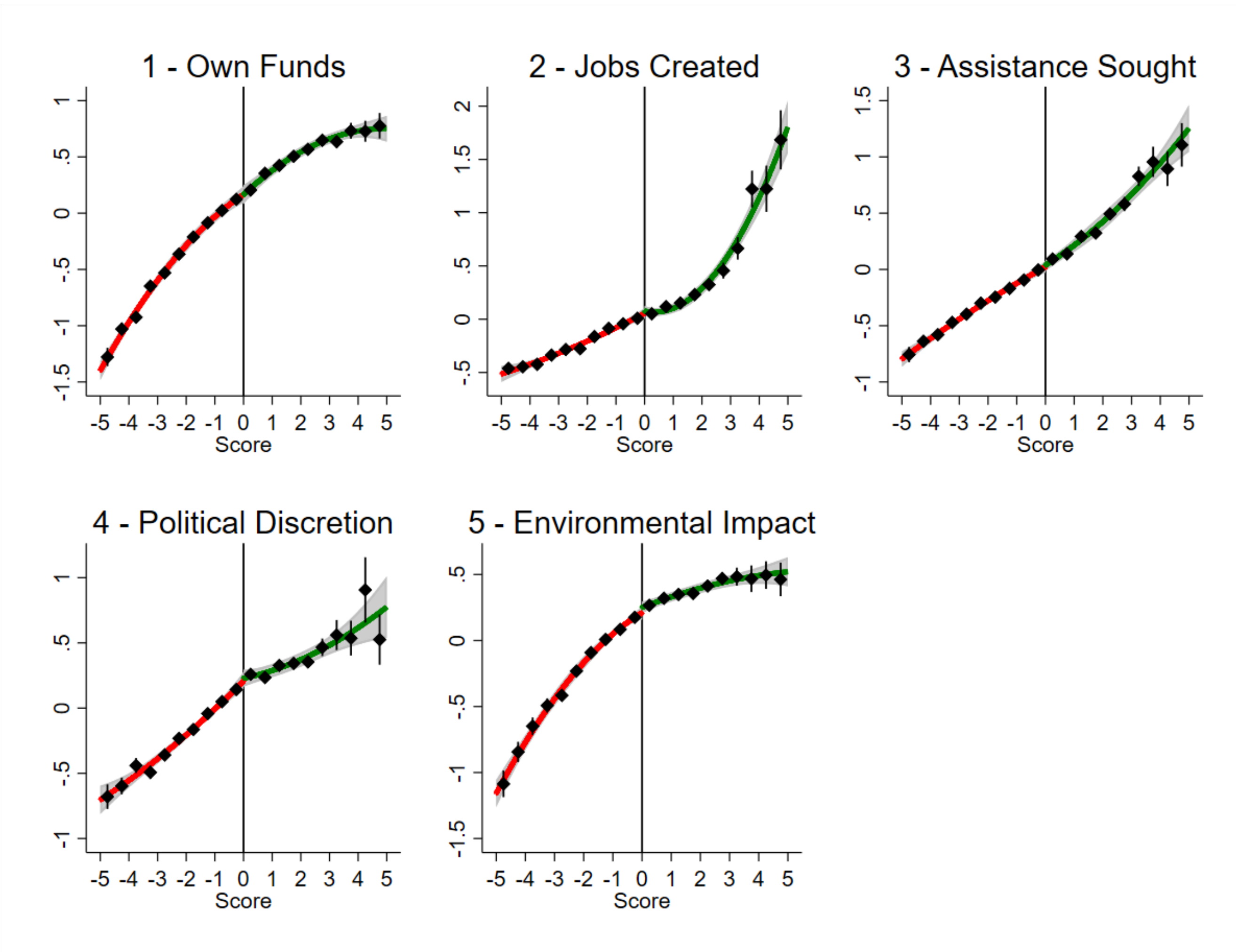
[19] The formal test by McCrary (2008), as implemented by Cattaneo, Jansson & Ma (2020), does not reject the null hypothesis of no discontinuity at the cutoff with a p-value of 0.2.

Figure 4: *Balance of firm characteristics one year before the call*



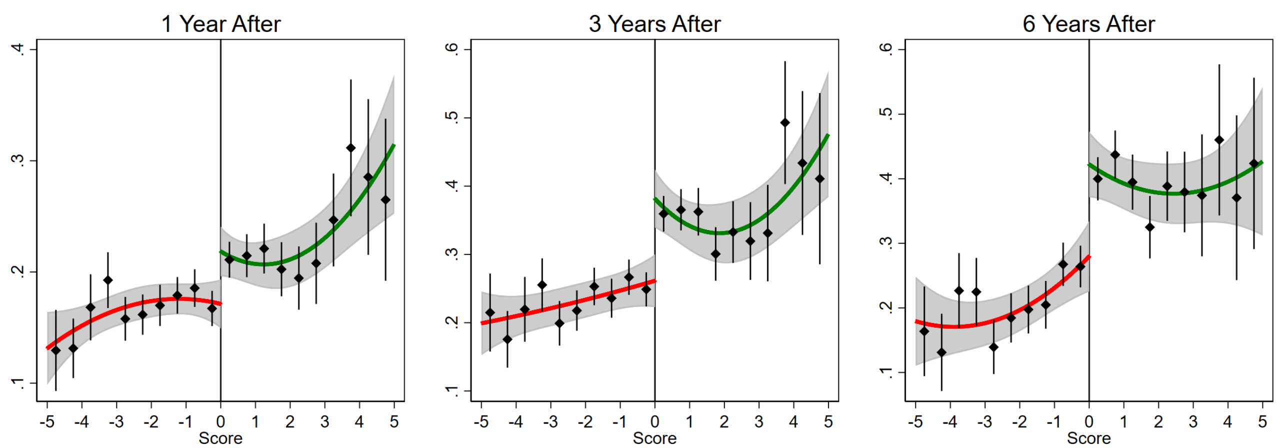
Notes: These graphs show the relationship between the standardized score obtained by firm applications for L488/92 funds, on the horizontal axis, and several firm characteristics measured one year before the call – log and yearly log-change in revenues, value added, value added per worker, investment, firm age and being a start-up. Bins represent averages over equally-spaced intervals, and confidence intervals (at the 90% significance level) are also shown by vertical lines. The predicted relationships between each variable and the score are estimated using a quadratic polynomial regression, controlling for cell-specific fixed effects. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

Figure 5: *Balance of the score components*



Notes: These graphs show the relationship between the standardized score obtained by firm applications for L488/92 funds, on the horizontal axis, and its five components (described in previous Section 2). Bins represent averages over equally-spaced intervals, and confidence intervals (at the 90% significance level) are also shown by vertical lines. The predicted relationships between each variable and the score are estimated using a quadratic polynomial regression, controlling for cell-specific fixed effects. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

Figure 6: *The effect of the L488/92 subsidy on firm employment*



Notes: These graphs show the relationship between the standardized score obtained in firm applications for L488/92 funds, on the horizontal axis, and the (log) employment 1, 3, and 6 years after the award of subsidies. Bins represent averages over equally-spaced intervals, and confidence intervals (at the 90% significance level) are also shown by vertical lines. The predicted relationships between each variable and the score are estimated using a quadratic polynomial regression, controlling for cell-specific fixed effects. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

between 0 and 3.

Figure 7 plots the estimated dynamic treatment effects on firm investment, employment, and other outcomes of interest, as well as (placebo) estimates for the years before obtaining the subsidy. The first two graphs confirm that the subsidy generates a transitory effect on investment, which translates into a long-lasting increase in firm employment; revenues and value added increase by about the same amount as employment (third and fourth graph), implying in turn that firm productivity remains approximately constant (fifth graph).

The last graph in Figure 7 shows that firms receiving the subsidy have higher survival rates than control firms. The difference after 6 years amounts to 3 percentage points, on a baseline survival rate of 87 percent. To the extent that excess mortality hits the lowest-performing firms in the control group (as it seems likely), the estimated effect on the other outcomes of interest – employment, revenues, value added, and productivity – is a lower bound to the average treatment effect when including non-surviving firms as well.

In Online Appendix C, we discuss two issues that could affect the interpretation of our results. First, applicants in a given call may re-apply (and obtain funds) in subsequent calls. In Section C.1 of the Online Appendix we show that (i) applicants obtaining funds are *less* likely to re-apply and to obtain funds in subsequent calls, so our estimates provide a lower bound to the direct effect of subsidies in one-off calls; but (ii) the difference between the direct effect and the total effect (i.e., accounting for the different probability of re-applying and obtaining funds in subsequent calls) remains small. The second issue is that the effects on funded firms may spill over to other, non-funded firms. The sign of potential spillover effects is unclear a priori. In case of positive local spillovers, our baseline estimates would understate the aggregate effect of subsidies; if, on the other hand, subsidized firms eroded the market share of competitors, including firms in the control group, our estimates would be biased upward. We show in Section C.2 of the Online Appendix that, empirically, there are no strong spillovers from subsidized firms to firms in the control group or those operating in the same local labor market and sector.

6 Results away from the RDD cutoff

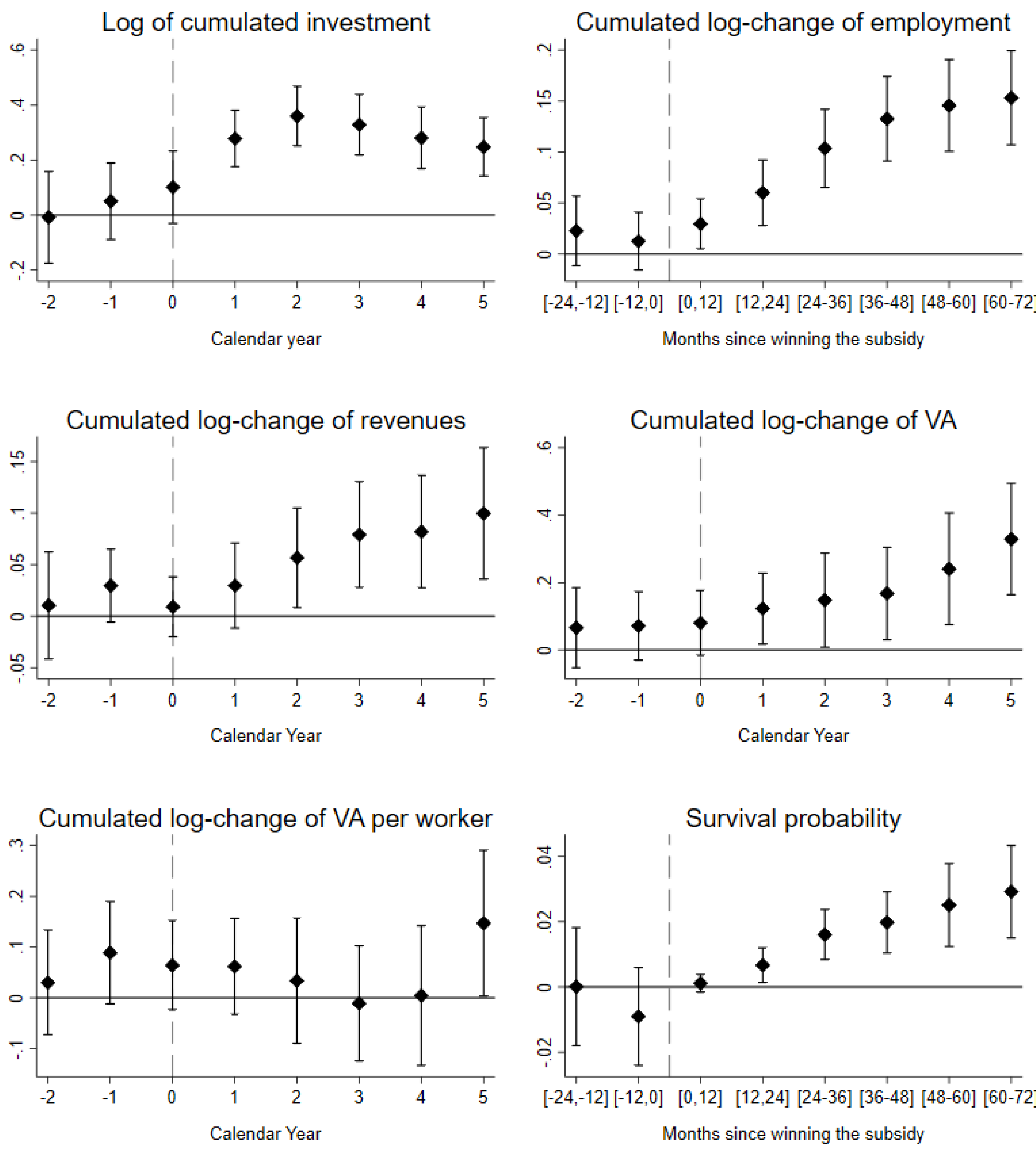
The results in the previous section show that L488/92 subsidies increase employment by 17 percent over a 6-year period across firms near the cutoff. We next estimate the full distribution of treatment effects following the approach of Angrist & Rokkanen (2015). With this analysis

Table 3: *The effect of obtaining the subsidy on firm investment and employment*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification:	linear				quadratic			
Kernel:	uniform		triangular		uniform		triangular	
Group fixed effects:	no	yes	no	yes	no	yes	no	yes
Panel A: Log of cumulated investment over 3 years								
Subsidy	0.267***	0.329***	0.245***	0.291***	0.211***	0.249***	0.218***	0.237***
	(0.062)	(0.056)	(0.062)	(0.059)	(0.077)	(0.075)	(0.077)	(0.074)
Observations	16,768	16,768	16,768	16,768	16,768	16,768	16,768	16,768
R-squared	0.015	0.233	0.012	0.235	0.015	0.233	0.012	0.235
Panel B: Log-change in employment over 1 year								
Subsidy	0.021*	0.030**	0.031***	0.034***	0.047***	0.043***	0.041**	0.039**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.015)	(0.014)	(0.016)	(0.015)
Observations	32,864	32,864	32,864	32,864	32,864	32,864	32,864	32,864
R-squared	0.002	0.043	0.001	0.045	0.002	0.043	0.001	0.045
Panel C: Log-change in employment over 3 years								
Subsidy	0.088***	0.104***	0.101***	0.104***	0.120***	0.107***	0.114***	0.105***
	(0.019)	(0.020)	(0.020)	(0.020)	(0.026)	(0.025)	(0.028)	(0.026)
Observations	31,681	31,681	31,681	31,681	31,681	31,681	31,681	31,681
R-squared	0.004	0.059	0.004	0.063	0.004	0.059	0.004	0.063
Panel D: Log-change in employment over 6 years								
Subsidy	0.147***	0.153***	0.145***	0.139***	0.142***	0.124***	0.131***	0.119***
	(0.023)	(0.024)	(0.023)	(0.023)	(0.030)	(0.029)	(0.032)	(0.030)
Observations	28,759	28,759	28,759	28,759	28,759	28,759	28,759	28,759
R-squared	0.007	0.066	0.007	0.067	0.007	0.066	0.007	0.067

Notes: This table shows the effect of L488/92 subsidies on firm investment and employment growth, as estimated from the parametric RD regression in equation (4.1) across applicant firms in all L488/92 calls. The dependent variable in each regression is indicated on top of each panel: log of cumulated investment in the 3 (calendar) years after the award of subsidies (Panel A); and log change of firm employment in the 12 months, 36 months and 72 months after the award of subsidies (Panels B, C, and D). The main explanatory variable, Subsidy, is a dummy equal to one for firms obtaining a score above the cutoff. The specification in columns (1)-(4) includes the standardized application score, equal to zero at the cutoff, and its interaction with Subsidy, while columns (5)-(8) include, in addition, the squared application score and its interaction with Subsidy; even columns include group fixed effects for firms competing in the same ranking; and columns (3)-(4) and (7)-(8) weight observations by a triangular kernel in distance from the cutoff. Heteroskedasticity-robust standard errors clustered by cell are reported in parenthesis. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% level.

Figure 7: *Dynamic effects of the L488/92 subsidy on several firm outcomes*



Notes: These graphs show the estimated effects of the subsidy on several outcomes of interest at different time horizons, indicated on the horizontal axis, and associated confidence intervals (at the 90% significance level). In particular, each graph shows the effects up to 6 years after obtaining the subsidy as well as the (placebo) estimated effects for up to 2 years before obtaining the subsidy. Point estimates and confidence intervals refer to the baseline specification in column (2) of Table 3, namely a linear regression including cell fixed effects and clustering heteroskedasticity-robust standard errors at the same level.

we can characterize the heterogeneity across different groups of firms; the cost-effectiveness of the policy, as measured by the ratio of public funds over the number of created jobs, and the effectiveness of the policy under alternative allocation criteria.

As discussed in Section 4, Angrist & Rokkanen (2015) invoke mean independence of the outcome on the running variable and common support between treated and control groups, conditional on a set of covariates X. We experiment with alternative predictors of firm growth, and we achieve conditional independence and common support for a vector $X^{\star}$ that includes the following covariates: firm age, which is inversely related to growth (Evans 1987); lagged realizations of a firm's growth and 3-year forward growth of firms in the same market, as defined by the LLM and 3-digit sector; workers' skills, as measured by the average wage of white collar workers and indicators for having managers or apprentices in the payroll, and a measure of the size of the investment project, scaled by initial employment along with cell fixed-effects.[20] Importantly, all results are robust when selecting an alternative set of covariates based on a newly developed data-driven algorithm in the spirit of Imbens & Rubin (2015). This algorithm implements a *greedy approach* that selects, at each step, the variables making the ignorability condition most likely to hold. We describe this alternative approach in more detail in Section S2 of the Supplementary Materials.

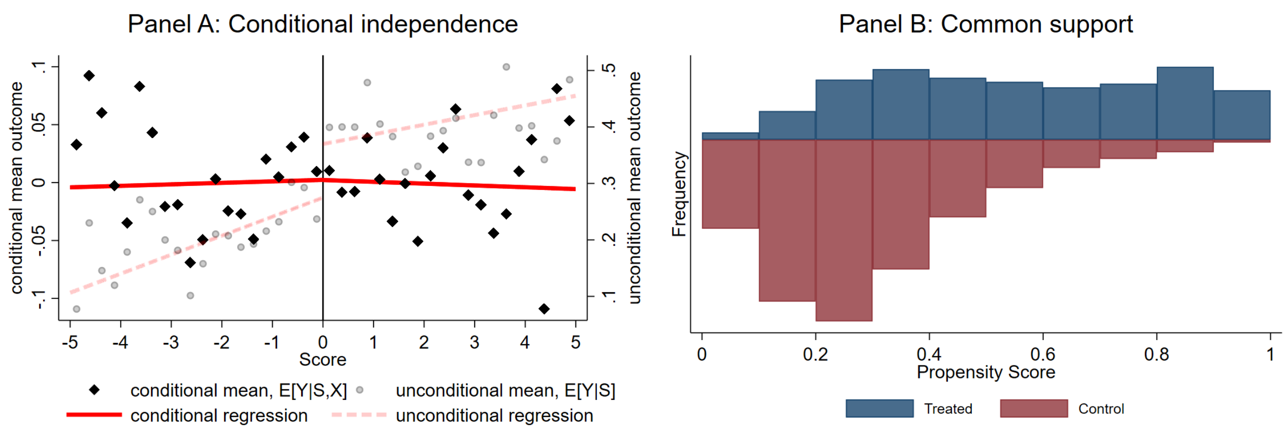
In Figure 8 we visualize the results of the tests for conditional independence - equation (4.2) - and common support - equation (4.3) - for the vector of covariates $X^{\star}$. Starting with the former condition, Panel A plots the binned residuals from a regression of 6-year employment growth on $X^{\star}$ against the applicant's score (black markers) together with the conditional regression line (solid line) separately on each side of the cutoff. The relationship is flat, as confirmed by the estimated coefficients reported in Online Appendix Table A3. The same graph also shows that, in line with the evidence in previous Figure 6, the unconditional relationship between the outcome and the running variable is positive (grey markers and dashed regression line). In other words, while on average higher-ranked applicants experience faster employment growth on both sides of the cutoff, this relationship is broken conditioning on the vector of firm characteristics in $X^{\star}$.[21] Therefore, the variability in the treatment status induced by S after conditioning on $X^{\star}$ is *as if* randomly determined.

[20]In more detail, the specification exploits 5 classes of firm age, deciles of lagged employment growth, and their interaction; deciles of average wages and of 3-year firm employment growth in similar firms, and two dummies for managers or apprentices. All these variables are interacted with project size.

[21]The same result holds at any time-horizon between t+1 and t+6, for both employment growth and investment. These results are available upon request.

Finally, Panel B of Figure 8 displays considerable common support between treated and controls in the distribution of the propensity score $\mathbb{P}(D = 1 \mid X^{\star})$.

Figure 8: *Testing the conditional independence and common support*

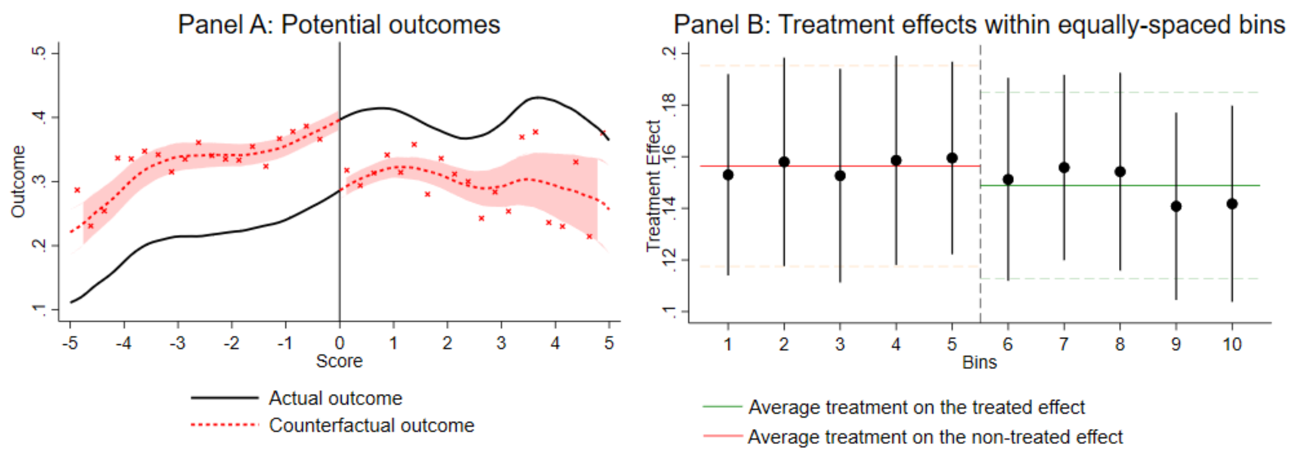


Notes: Panel A shows the test of conditional independence in equation (4.2) for the vector of covariates $X^{\star}$, described at the beginning of Section 6. Black markers represent the mean outcome Y (i.e., firm employment growth in the 6 years after applying for L488/92 funds) by equally-spaced bins of the running variable (i.e., the application score), conditional on the vector $X^{\star}$ of covariates, $E[Y|S, X^{\star}]$; the regression line is also reported (solid line). Grey circles and the dashed line represent the unconditional mean, $E[Y|S]$, and regression line. Panel B shows the density of treated and control firms by decile of the estimated propensity score of receiving the subsidy conditional on $X^{\star}$, $P(D|X^{\star})$.

6.1 The effects of subsidies across inframarginal firms away from the cutoff

Under conditional independence and common support, we can estimate treatment effects across firms away from the cutoff by comparing the outcomes of treated and control firms keeping constant the covariates in $X^{\star}$. Specifically, we use the estimated parameters in equation (4.5) to predict the potential outcomes of control firms were they treated, and the estimated parameters in equation (4.6) to predict the potential outcomes of treated firms were they not treated. Panel A of Figure 9 plots fitted actual and extrapolated counterfactual outcomes along the distribution of the application score, together with bootstrapped confidence intervals. As it should be expected, both potential outcomes increase with the running variable, as higher-ranked applicants exhibit stronger employment growth both when they are treated and when they are untreated. The two lines are approximately parallel, implying that average treatment effects are constant along the application score. This is confirmed in Panel B of Figure 9, which plots treatment effects and confidence intervals across equally-spaced bins. This does not imply that the effects of subsidies can not vary along other dimensions other than the score, as we will see in Section 6.3.

Figure 9: *Potential outcomes and treatment effects in $t + 6$, along the distribution of the applicant score*



Notes: Panel A plots actual and counterfactual potential outcomes six years after obtaining or not obtaining the subsidy, as estimated from kernel-weighted local polynomial smoothers, along the applicant score. Counterfactual outcomes are estimated by equations (4.5) and (4.6), and bootstrapped confidence intervals are reported. Panel B plots average treatment effects within quantile-spaced bins on either side of the cutoff, estimated using the linear reweighting estimator in (4.7). 95% confidence intervals are estimated using 2000 iterations of a non-parametric cluster bootstrap.

6.2 Estimated policy effect and comparison with previous work

Endowed with (i) the distribution of the treatment effects along the application score – as opposed to a limited subset of marginal applicants, as it is typically the case in RDD studies – and (ii) the subsidy paid out to each applicant, we can estimate the total effect of the policy. We focus in particular on the cost, in terms of subsidies paid to the applicant firm, of creating new jobs and additional investment over a period of 6 years since receiving the subsidy. Table 4 shows that the cost per new additional job is just below €180,000, this estimate being very similar when using the baseline set of conditioning covariates (column 1) and the alternative set of covariates selected by the data-driven algorithm (columns 2). Since each job may last several years, we also compute the cost per job-year through year 6, which stands at €54,000-58,000 (columns 3 and 4). Since job duration may extend beyond the sixth year, these estimates are an upper bound to the actual cost of the policy.

The estimates in column (1) are substantially higher than previous estimates by Cerqua & Pellegrini (2014), which stand at €60,000-€100,000 per job. Restricting the analysis to marginal firms close to the cutoff, as they do, closes part of the gap; the remaining part reflects differences in data coverage, research design, and estimation methodology. For instance, our administrative data cover almost all applicants, including very small firms that typically exhibit higher cost-per-job and investment compared to large firms (we discuss this cost-size gradient in more detail in the next Section 6.3).

We next compare our results with previous estimates of the cost per job of different incentive policies (tax breaks or cash transfers), all converted to 2010 prices. Bartik (2020) finds that the typical drawn-out incentive package generates a job at a discounted cost of $180,000 dollars. The figure factors in a local multiplier effect of 1.5, hence the cost per job at subsidized firms amounts to $270,000, which should be compared with our average estimate in discounted US$ of $236,000. Slattery & Zidar (2020) find a lower average figure ($96,000), which nonetheless varies substantially across states and reaches $310,000 in disadvantaged areas – an estimate comparable to ours for disadvantaged, Southern Italian regions ($320,000). Chodorow-Reich (2019) reviewed estimates of local effects of the American Recovery and Investment Act (ARIA), in terms of cost per job-year. The estimates vary between $25,000-$125,000, depending on the components of the program and the estimation approach. The preferred figure is about $50,000 per job-year, which we compare to $71,000 in our case (column 3 of Table 4, after conversion to US$).

Overall, the cost-effectiveness of L488/92 subsidies is not too different from that estimated for similar programs in other countries. At the same time, cost-effectiveness varies dramatically between regions in Italy. The second and third row of Table 4 show that the cost per new job is 3.5 times higher – and the cost per job-year four times higher – in Southern regions than in Northern regions. These wide gaps in job creation per € of subsidy reflect analogous differences in (inverse) investment multipliers, as measured by the amount of the subsidy over new investment. New investment in the south equals the amount of the public subsidy, while each € of public subsidy generates more than two additional euros of investment in center-northern regions (columns 5 and 6).

Therefore, the cost-effectiveness of L488/92 subsidies was much lower in Southern regions, which received the largest share of funds; see also Figure A12 in the Online Appendix . This

Table 4: *Cost of new jobs and investment generated by L488/92 subsidies*

	(1)	(2)	(3)	(4)	(5)	(6)
Cost measure:	cost per new job (thousands of €'s)		cost per worker-year (thousands of €'s)		cost of new investment (cost per € of investment)	
$X^{\star}$:	manual	data-driven	manual	data-driven	manual	data-driven
all regions	178	159	54	56	0.81	0.63
	[133; 299]	[118; 260]	[47; 62]	[51; 62]	[0.59; 1.25]	[0.48; 0.87]
south	241	201	77	72	1.05	0.87
	[195; 332]	[163; 270]	[70; 88]	[67; 79]	[0.79; 1.51]	[0.67; 1.17]
north-center	68	70	19	25	0.35	0.25
	[41; 211]	[44; 214]	[17; 24]	[22; 29]	[0.24; 0.59]	[0.18; 0.34]

Notes: This table shows the cost of new jobs and investment generated by the L488/92 subsidies over a six-year period. All amounts are expressed in euros at constant 2010 prices. The estimates in columns labelled as "manual" employ the set of covariates listed at the beginning of Section 6, while the estimates in columns labelled as "data-driven" employ the set of covariates selected by the algorithm described in detail in Section S2 of the Supplementary Materials. 90% confidence intervals are reported in brackets and are computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level.

relationship is consistent with decreasing returns to the mobilization of new public subsidies, particularly in disadvantaged areas characterized by a scarcity of profitable investment opportunities. We next ask whether an alternative allocation mechanism could have improved on cost-effectiveness, especially in Southern regions.

6.3 Rules vs. discretion

As explained in Section 2, the application score initially summarized only *objective rules*, namely own resources invested by the applicant (indicator I1), number of newly created jobs (indicator I2), and proportion of funds requested in relation to a benchmark by type of project (indicator I3). Starting with the third call for applicants, in 1998, a fourth criterion reflecting only the *political discretion* of the regional government was added.

We next compare the effectiveness of projects selected on the basis of objective rules and political discretion, respectively, using the result in equation (4.9). To this purpose, we first show that the CIA holds for both scores jointly. In Table 5, we regress the log change in employment over 6 years (i.e., our main outcome of interest) on both subscores SR and SD separately for projects on either side of the cutoff, and include the additional set of covariates $X^{\star}$ (columns 2 and 4). In line with the CIA, the coefficients of SR and SD are no longer significantly different from zero after controlling for $X^{\star}$. In addition, Online Appendix Figure A9 plots the residuals of the estimated regression over the support of (SR, SD) – 25 bins, corresponding to the 5×5 quintiles of SR and SD – separately to the left and right of the cutoff, and Online Appendix Figure A10 shows their 95% confidence intervals. There is no systematic relationship between the residuals and either of the two sub-scores, and confidence intervals do not cross the zero line only in 5 cases out of 100, which is what we should expect (given the level of statistical significance) under the null hypothesis that the CIA holds.

Turning to the estimation of treatment effects, Panel A of Figure 10 plots the effect on 6-year employment growth by quintiles of the sub-scores for objective rules (SR) and political discretion (SD).[22] Both the firms preferred by regional politicians and those scoring high on objective criteria generate larger employment growth compared to other applicants. In particular, the effect ranges between 10% for applicants scoring low on both SR and SD to almost 20% for applicants scoring high on both dimensions. Therefore, the (approximately) constant effect along the distribution of the overall application score S, shown in Figure 9,

[22] Online Appendix Table A7 reports point estimates and confidence intervals for all entries of Figure 10.

Table 5: *Conditional independence tests*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment Growth				Investment			
	Left		Right		Left		Right	
SR	0.054***	0.004	0.114***	0.024	0.020	-0.039	-0.011	-0.004
	[0.016]	[0.012]	[0.030]	[0.016]	[0.030]	[0.024]	[0.040]	[0.030]
SD	0.038***	0.004	0.032**	0.003	0.049**	0.003	-0.056**	0.001
	[0.008]	[0.007]	[0.013]	[0.011]	[0.019]	[0.014]	[0.024]	[0.020]
Obs	14,646	14,646	8,020	8,020	11,013	11,013	6,013	6,013
Adj R^2	0.035	0.343	0.054	0.431	0.164	0.364	0.168	0.385
F-stat	13.96	0.17	8.81	1.19	3.13	1.47	2.84	0.02
p-value	0.000	0.845	0.000	0.304	0.045	0.231	0.060	0.981
$X^{\star}$	N	Y	N	Y	N	Y	N	Y

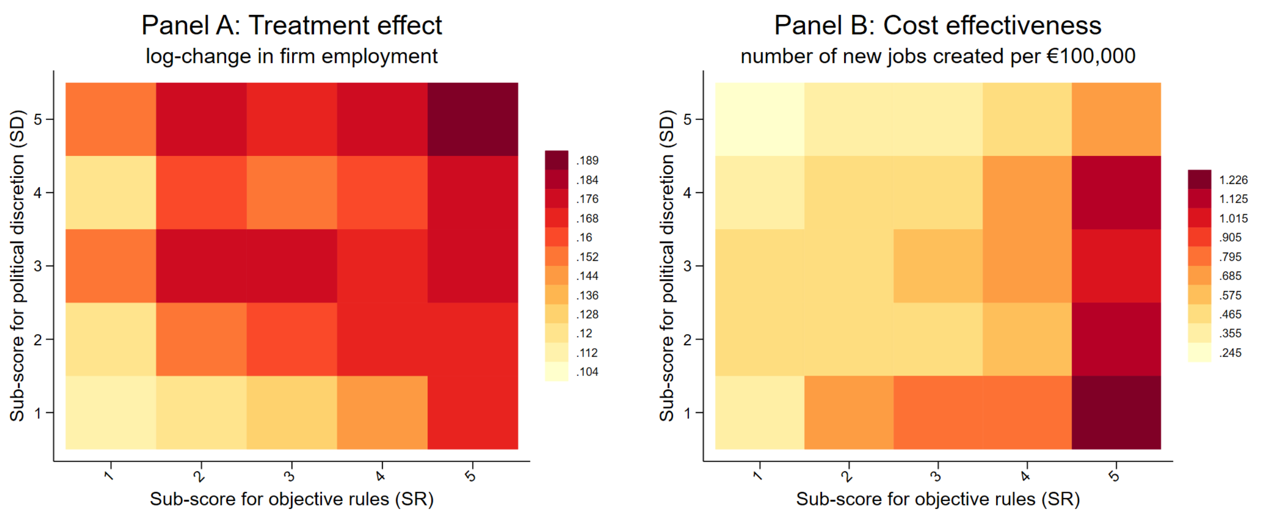
Notes: The table reports regression-based tests of the conditional independence assumption in equation (4.8). We regressed employment growth and investment in the six years after the award of L488/92 subsidies on the two sub-scores for objective rules and political discretion (i.e., SR and SD). Regressions in even columns also includes all covariates in $X^{\star}$, listed at the beginning of Section 6. All regressions include cell fixed effects. The sample includes only applicant firms from the 3^{rd} call for projects onward, as the sub-score for political discretion was not present in the first two calls (see Section 2). Standard errors clustered by cell are reported in parenthesis.

results from the fact that SR and SD are inversely correlated with each other (Figure 2), thus masking a positive relationship between job creation and both of the two sub-scores.

At the same time, the cost of creating new jobs, in terms of subsidies, varies dramatically between applicants ranking high on SR and applicants ranking high on SD. Panel B of Figure 10 shows that the number of new jobs created per €100,000 of subsidies received by the firm is highest in the south-east quadrant (high on rules and low on discretion) and it is lowest in the north-west of the graph (low on rules and high on discretion). On average, it takes just over €80,000 for high-on-rules, low-on-discretion applicant firms to create a new job, while the cost is five times as large for low-on-rules, high-on-discretion applicant firms.

Therefore, the larger subsidies-per-worker demanded by applicants ranking high in terms of the political score, compared to applicants ranking high in terms of objective rules (Figure A5, bottom panels), translate into a higher cost-per-newly-created job. Figure A5 also made clear that applicant firms favored by political discretion are on average smaller than firms ranking high in terms of objective rules. Additional estimates reported in Online Appendix Table A6 reveal that smaller firms generate higher *percent* changes in employment but lower increases in

Figure 10: *Treatment effect and new jobs created per €100,000, rules vs. discretion*



Notes: This figure shows the heterogeneity in treatment effects on firm employment growth (Panel A) and the cost effectiveness of subsidies (Panel B), by quintiles of the sub-scores for objective rules (SR) and political discretion (SD). In Panel A, the treatment effect for each bin ($SR = r, SD = d$) is estimated as $\mathbb{E}[Y^1 - Y^0 \mid SR = r, SD = d] = (\beta_1 - \beta_0) \cdot \mathbb{E}[X^\star \mid SR = r, SD = d]$. The covariates included in $X^\star$ are listed at the beginning of Section 6. In Panel B, cost effectiveness is measured by the number of newly created per €100,000 of subsidies received by the firm. The number of newly created jobs in each bin is computed by multiplying the size of each firm by the treatment effect for its respective bin, as reported in Panel A, and aggregating across all firms in that bin. Online Appendix Table A7 reports the estimate for each bin together with 90% confidence intervals computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level.

the *number* of new jobs than large firms (250+ employees), as even smaller percent increases in employment in large firms translate into a high number of newly-created jobs. As a consequence, the cost per new job is more than three times larger for small firms than for large firms – €253 thousand and €78 thousand, respectively, the two estimates being statistically different from each other.

These facts reconcile the findings in the two panels of Figure 10. Specifically, both applicants favored by political discretion and those favored by objective rules generate large percent increases in employment, but the former are smaller in size and demand larger subsidies per worker. Hence, the same percent increase translates into a lower number of new jobs and a higher cost-per-new-job. Online Appendix Figure A13 show that such differences are statistically significant.

6.4 Counterfactual scenarios

To further assess the implications of allowing for political discretion in the selection of projects, we simulate the cost of new jobs under counterfactual policies. Specifically, we consider alternative criteria for ranking applications and compute the cutoffs obtained within each cell under the counterfactual ranking. Some of the applicants funded under the actual policy would not be funded under the counterfactual policy, and vice-versa. We then compute the counterfactual cost of new jobs and investment by integrating the treatment effects over the subset of applicants funded under the counterfactual ranking.

This exercise maintains the policy invariance assumption (4.10): the distribution of applicants' characteristics as well as the average "objective quality" of the projects they submit, as measured by the objective scores, are not affected by a change of the selection rule. We provide evidence consistent with such assumption by comparing applicants' characteristics and projects' objective scores between the last call for projects before the policy change and the first call for projects after the policy change. Online Appendix Figure A11 shows a large degree of overlap in the distribution of such variables before and after the policy change, and Online Appendix Table A4 confirms that means are not significantly different between the two groups. We also implement a difference-in-differences version of this balance test by leveraging the fact that a few regions did not specify discretional priorities in all or part of the period after the policy change. In particular, the regional government of Lombardy always relied only on objective scores, while the government of Puglia did the same in the first two calls after the policy change but attributed discretionary points in the following calls. We thus compare average objective scores and applicants' characteristics between regions that used and did not use discretion, before and after the policy change, controlling for region and period fixed effects. These regressions, reported in Online Appendix Table A5, suggest that neither the average characteristics of applicant firms nor the average quality of submitted projects changed significantly after the policy change, providing strong support for the policy invariance assumption.

We consider three main counterfactual policies: eliminating political discretion; relying only on political discretion, and an "optimal" policy prioritizing categories of firms generating jobs at the lowest cost, based on the treatment effect distribution estimated for the actual policy.

The costs of new jobs and new investment under these counterfactual policies are presented in Table 6, along with the costs under the actual policy (column 1).[23] Column (2), Panel A,

[23] The costs reported in column (1) of Table 6 are slightly different from those in Table 4 because the latter

shows that that eliminating political discretion would reduce the cost of creating new jobs by 11 percent. Interestingly, the cost reduction would be more marked in southern than in northern regions (12 percent and 9 percent). The cost of investment, in Panel B, exhibits a similar reduction (13 percent) but no clear gradient along the north-south dimension. The following column (3) shows the effect of an opposite policy, namely relying exclusively on political discretion for allocating subsidies. Such a policy would greatly increase the cost of new jobs and investment by 42 and 22 percent, respectively.[24]

Overall, the evidence in columns (1)-(3) of Table 6 suggests that politicians' influence on the allocation of subsidies results in fewer new jobs generated for the same budget, particularly in disadvantaged (southern) regions commanding the largest share of the budget. Based on our previous results in Section 3.1, we exclude that lower cost effectiveness of subsidies allocated by political discretion reflects better targeting of more disadvantaged areas, as observable measures of economic disadvantage such as local unemployment rates and credit constraints are not significantly correlated with the discretionary sub-score.

We also consider the possibility that the different effectiveness of projects selected on objective rules vs. political discretion may be biased by a different compliance with treatment assignment (i.e., a different probability of actually receiving the subsidy among applicants scoring above the cutoff). In Online Appendix Figure A14, we plot the relationship between non-compliance with treatment assignment and the sub-scores for objective rules and political discretion, respectively. While there is no relationship between non-compliance and the sub-score for political discretion, eligible applicants scoring higher on objective rules are less likely to receive the subsidy. A potential reason for this pattern is that such applicants may tend to over-promise in the application stage (e.g., in terms of new jobs to be created) and are eventually unable to deliver. In any event, the results in Online Appendix Figure A14 suggest that, if anything, we are under-estimating the cost-effectiveness of projects selected on objective rules relative to projects selected on political discretion.

Finally, we consider a counterfactual policy assigning priority to firms generating new jobs at the lowest cost. Column (4) of Table 6 shows that the cost of creating new jobs and investment would decrease by 54 and 57 percent. Even in this case, the reduction in the cost of new jobs would be larger in southern than in northern regions.

is based on application in all calls for projects, while the former includes only applicant firms from the 3^{rd} call onward (the sub-score for political discretion was not present in the first two calls).

[24] For this second counterfactual scenario, we cannot validate the policy invariance assumption in the same way as we did for the no-discretion counterfactual scenario.

Table 6: *Cost of new jobs and investment under different counterfactual policies*

	(1)	(2)		(3)		(4)	
	Actual policy	**Counterfactual policies**					
		No discretion		Only discretion		Cost minimizing	
	cost	cost	%Δ	cost	%Δ	cost	%Δ
Panel A: Cost per new job (thousands)							
all regions	179	159	-11.13	253	41.68	83	-53.73
			[-14.76; -8.04]		[17.67; 64.30]		[-60.25; -52.41]
south	225	198	-12.14	310	37.83	97	-57.07
			[-16.03; -8.49]		[23.52; 54.57]		[-61.36; -54.55]
north-center	83	76	-8.68	115	37.67	45	-45.62
			[-14.92; -3.66]		[-32.96; 47.69]		[-62.45; -43.47]
Panel B: Cost per 1€ of investment							
all regions	0.76	0.67	-12.80	0.93	22.45	0.33	-56.19
			[-16.89; -9.25]		[12.01; 32.75]		[-59.94; -53.37]
south	0.94	0.82	-12.38	1.12	19.98	0.38	-59.21
			[-17.00; -8.54]		[12.02; 30.90]		[-63.35; -55.56]
north-center	0.39	0.33	-13.80	0.47	22.64	0.2	-48.87
			[-21.58; -6.43]		[1.25; 27.33]		[-55.69; -44.49]

Notes: This table shows the cost per new job (Panel A) and the cost of new investment (Panel B) under the actual policy (column 1) and under different counterfactual policies: eliminating politicians' discretion, i.e. $SD(i) = 0$ for any i applicant (column 2); rank applicants exclusively on discretion, i.e. $SR(i) = 0$ for any i applicant (column 3); and giving priority to applicants with lower cost of generating jobs (column 4). 90% confidence intervals are reported in brackets and are computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level. All results are based on data from the 3^{rd} call for projects onward, as the sub-score for political discretion was not present in the first two calls for projects. All amounts are expressed in euros at constant 2010 prices.

7 Conclusions

Governments around the world are investing trillions of dollars to help private business in the wake of the Covid-19 pandemic (Romer & Romer 2021). However, the effects of these policies may vary widely depending on the criteria used to allocate funds: policies effectively targeting high-return firms may accelerate economic recovery and reduce economic disparities between regions, while other policies may entail significant deadweight losses, distort the allocation of productive inputs, and even encourage rent seeking behaviour (Krueger 1990, Restuccia & Rogerson 2008, Kline & Moretti 2014, Ehrlich & Overman 2020, Lane 2020).

It is thus extremely important to estimate the economic effects of public subsidies. To this purpose, we exploit quasi-experimental variation in investment subsidies across Italian firms. We address treatment effect heterogeneity and the cost-effectiveness of actual and counterfactual allocation schemes along the rules vs. discretion trade-off. Both firms ranking high on objective criteria and firms preferred by local politicians generate larger employment growth on average, but the latter do so at a higher cost per job. Under somewhat stronger assumptions, we can integrate such effects across different subsets of potential beneficiaries to compare policy effects under different allocation criteria. We conclude that, for the case of this specific policy, eliminating political discretion – thus relying only on ex ante, objective criteria – would improve cost effectiveness by 11 percent, while relying only on political discretion would increase the cost by as much as 42 percent.

A thorough assessment of welfare effects – of the type conducted, e.g., by Busso, Gregory & Kline (2013) for the US Empowerment Zones – would require detailed data on housing values and rents. We leave this analysis for future research.

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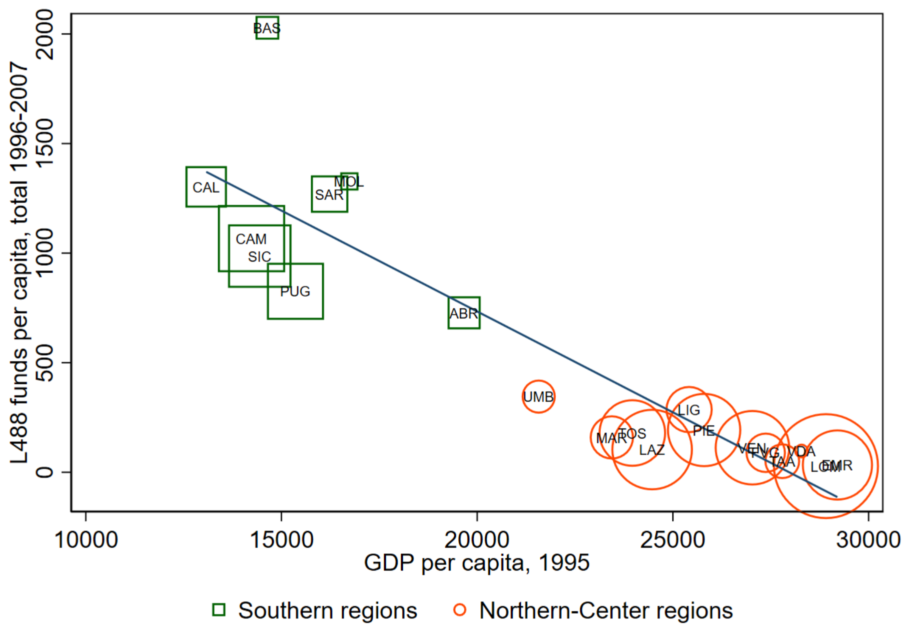
Online Appendix

A Additional figures and tables

Figure A1: *L488/92 funds and GDP per capita across regions*



Notes: This figure plots the total amount of L488/92 per capita received over the period 1997-2007 (vertical axis) against the GDP per capita in 1995 (horizontal axis), across Italian regions. Both variables are expressed in euros at constant 2010 prices. The size of markers is proportional to region population.

Figure A2: *Total L488/92 funds by year and geographical area*

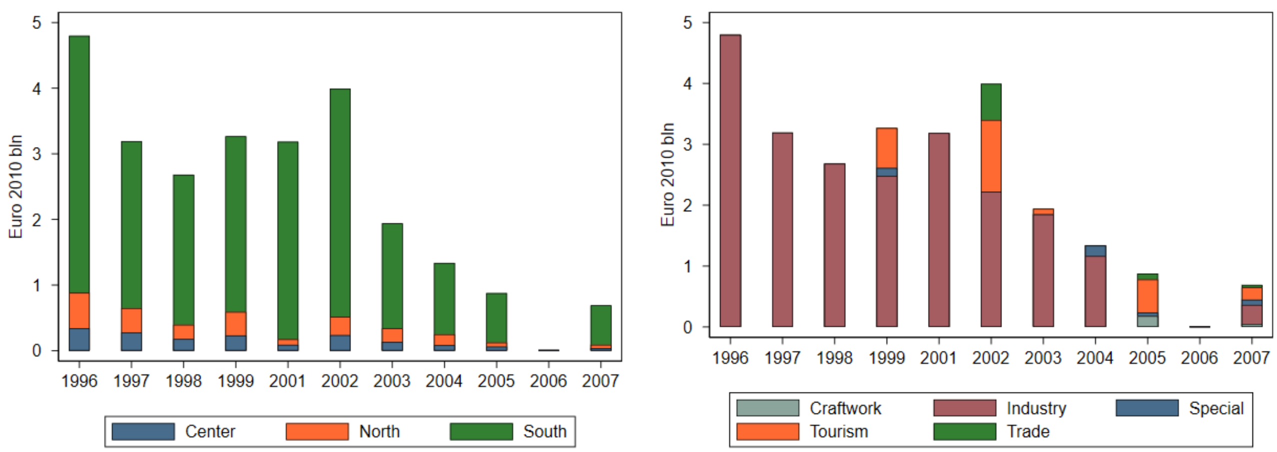


Figure A3: *Total L488/92 funds by region, source, and economic sector*

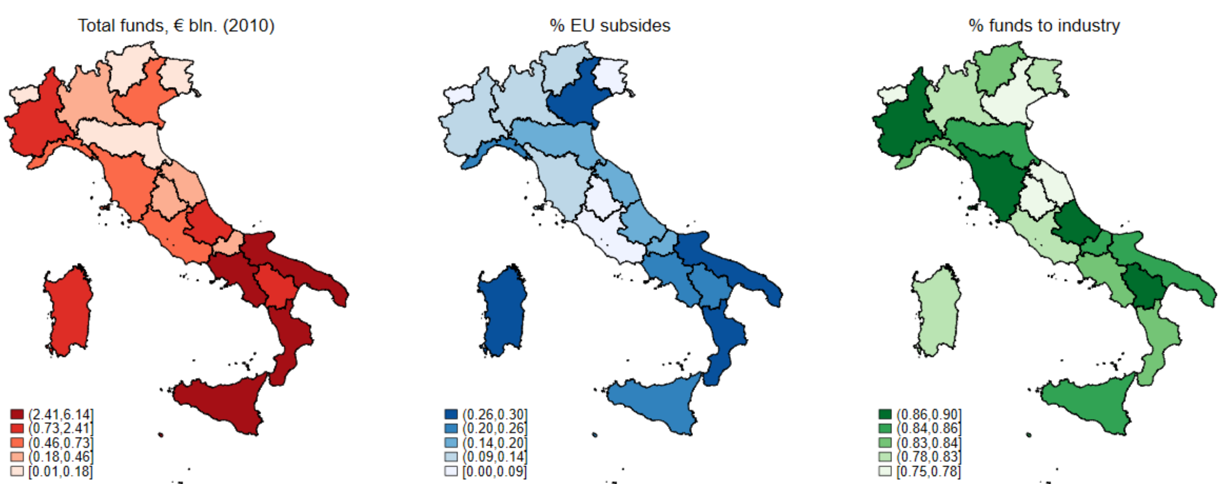
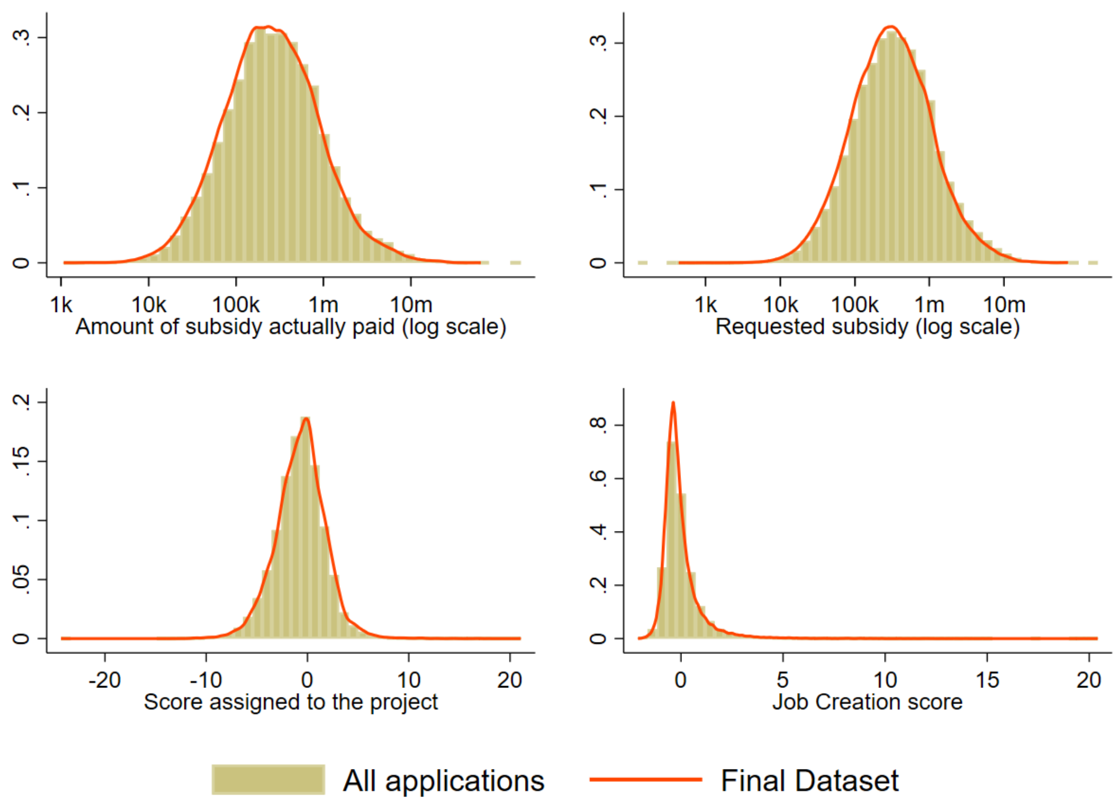
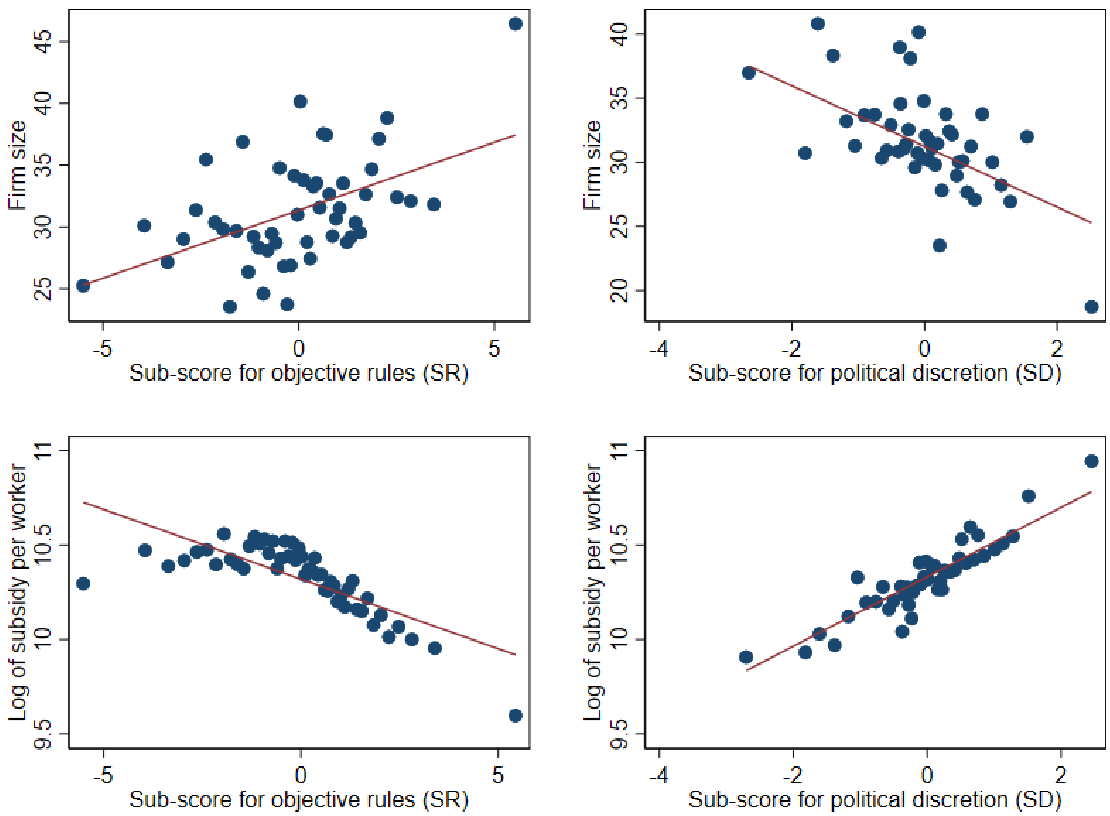


Figure A4: *Distribution of selected variables across all applications and within the sub-sample of matched applications*



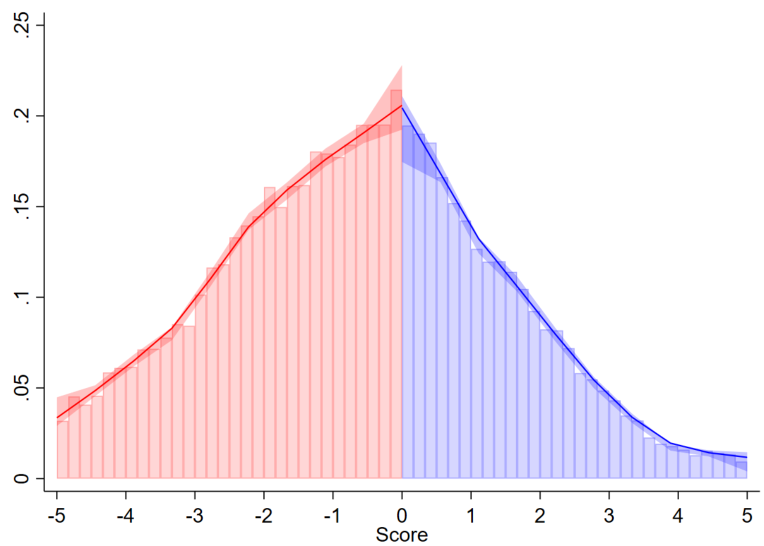
Notes: This figure shows the distribution of some variables across the entire sample of applicants and across the final sample of applicants for which we have complete information on employees and balance sheet data.

Figure A5: *Political discretion, objective rules, and applicant characteristics*



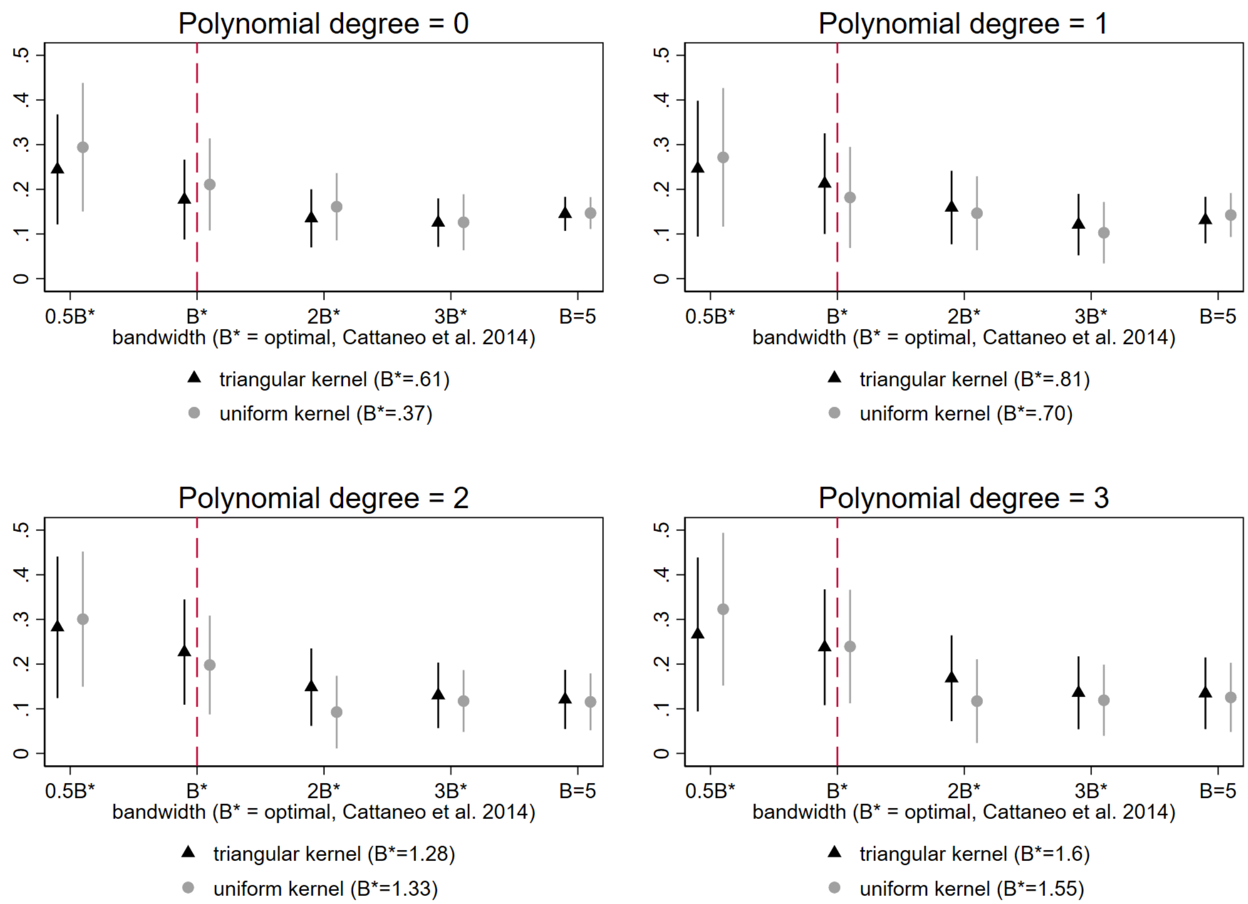
Notes: These graphs plot the sub-scores for political discretion and objective rules (on the horizontal axis), against the size of applicant firms and the amount of subsidies they applied for (on the vertical axis), controlling for cell fixed effects, across equally-sized bins.

Figure A6: *Density of applicant scores*



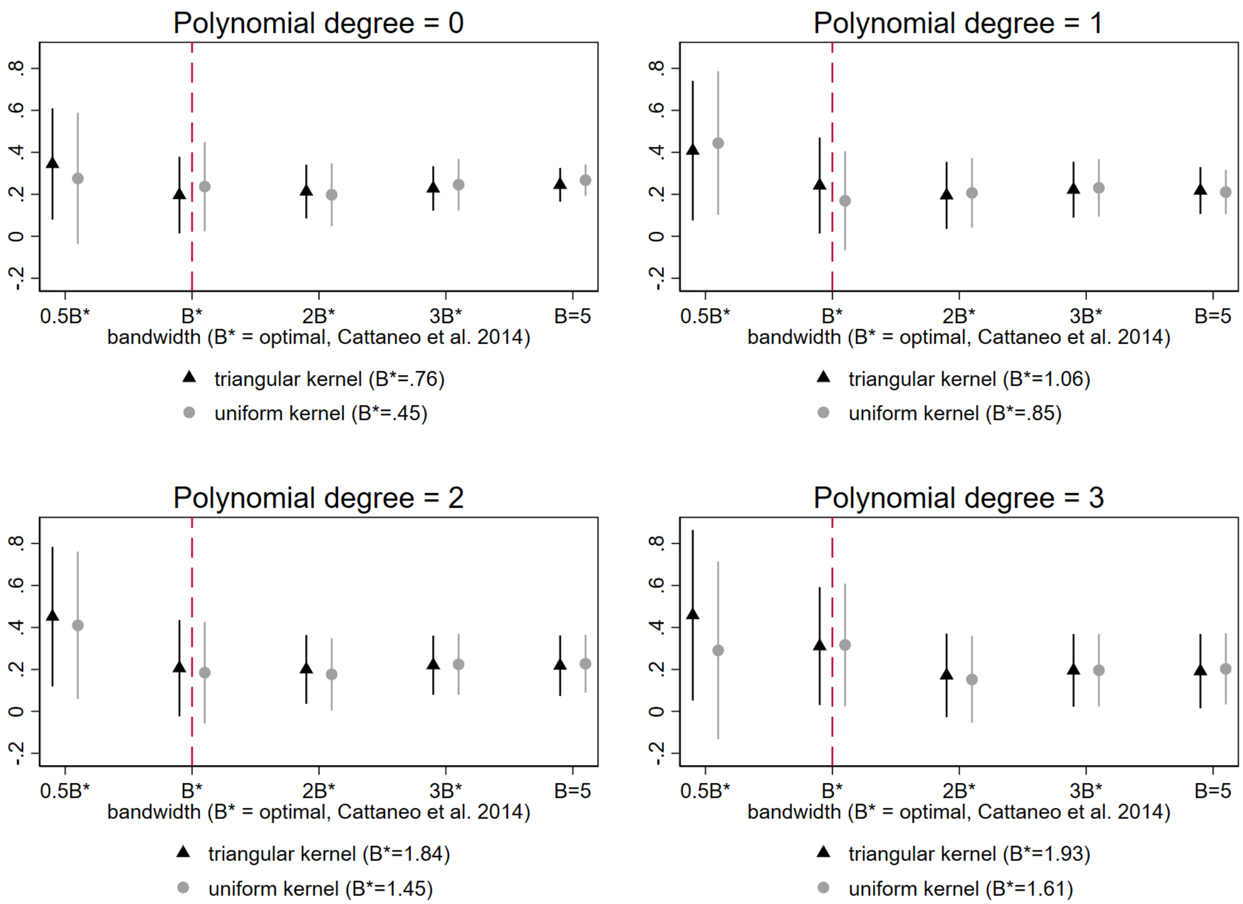
Notes: The histogram shows the distribution of applicant scores. Local polynomial density estimates (solid lines) and robust bias-corrected 95% confidence intervals (shaded areas), computed according to Cattaneo, Jansson & Ma (2020), are also reported in the figure.

Figure A7: *The effect of obtaining the subsidy on firm employment, non-parametric RDD estimates*



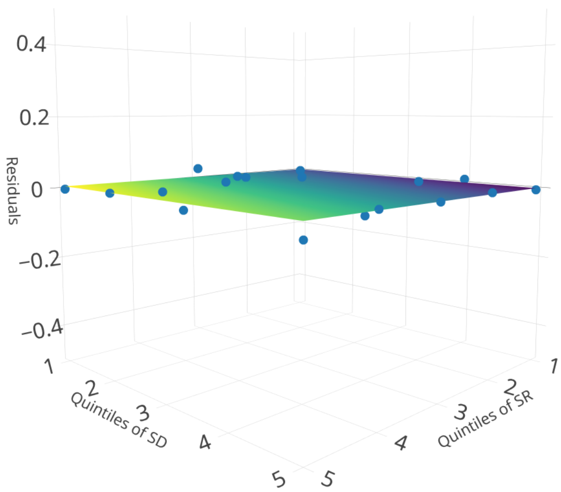
Notes: This figure plots the estimated effect of being eligible for the subsidy (i.e., scoring above the cutoff) on the log-change of firm employment 6 years after the award of subsidies, for different specifications of non-parametric RDD. In particular, each graph shows point estimates and confidence intervals when using triangular and uniform kernels, for different degrees of the polynomial in the running variable (reported on top of each graph) and different bandwidths around the cutoff (on the horizontal axis). The optimal bandwidth B^* as well as point estimates and confidence intervals are computed following the robust approach by Calonico, Cattaneo & Titiunik (2014).

Figure A8: *The effect of obtaining the subsidy on firm investment, non-parametric RDD estimates*

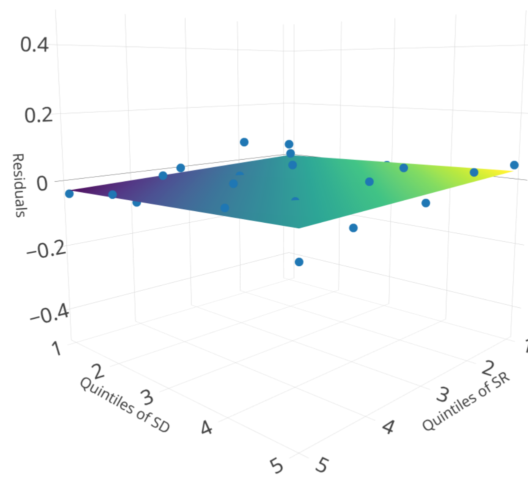


Notes: This figure plots the estimated effect of being eligible for the subsidy (i.e., scoring above the cutoff) on the log of cumulated investment over the 3 years after the award of subsidies, for different specifications of non-parametric RDD. In particular, each graph shows point estimates and confidence intervals when using triangular and uniform kernels, for different degrees of the polynomial in the running variable (reported on top of each graph) and different bandwidths around the cutoff (on the horizontal axis). The optimal bandwidth B^* as well as point estimates and confidence intervals are computed following the robust approach by Calonico, Cattaneo & Titiunik (2014).

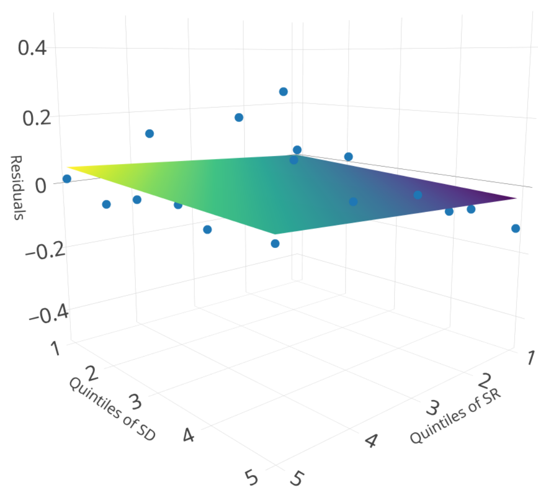
Figure A9: *Testing the CIA: plotting residuals on SR and SD.*



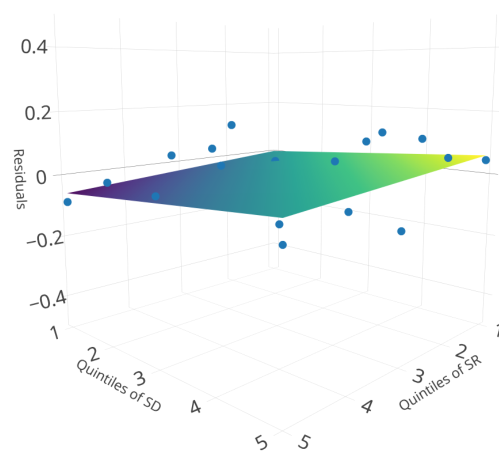
(i) *Employment Growth - Left*



(ii) *Employment Growth - Right*



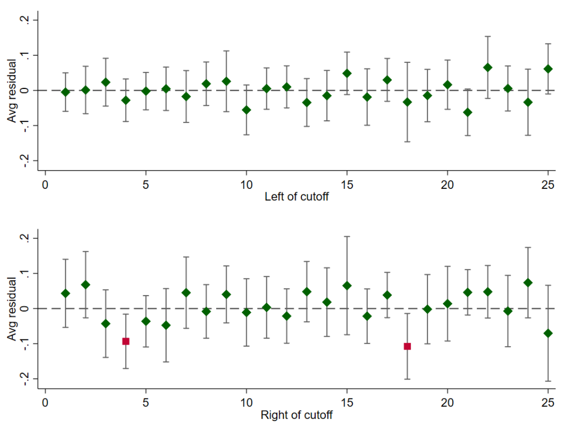
(iii) *Investment - Left*



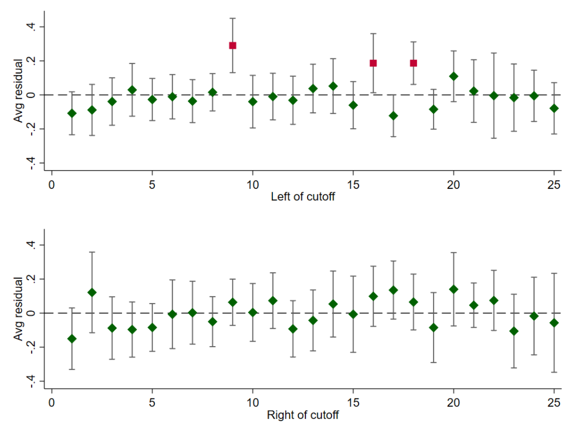
(iv) *Investment - Right*

Notes: This figure reports the best interpolating hyperplane of the residuals of the regression of the outcome (either employment growth or investment) on $X^{\star}$ on the quintiles of the sub-scores for objective rules (SR) and political discretion (SD).

Figure A10: *Testing the CIA: zero-mean residuals test.*



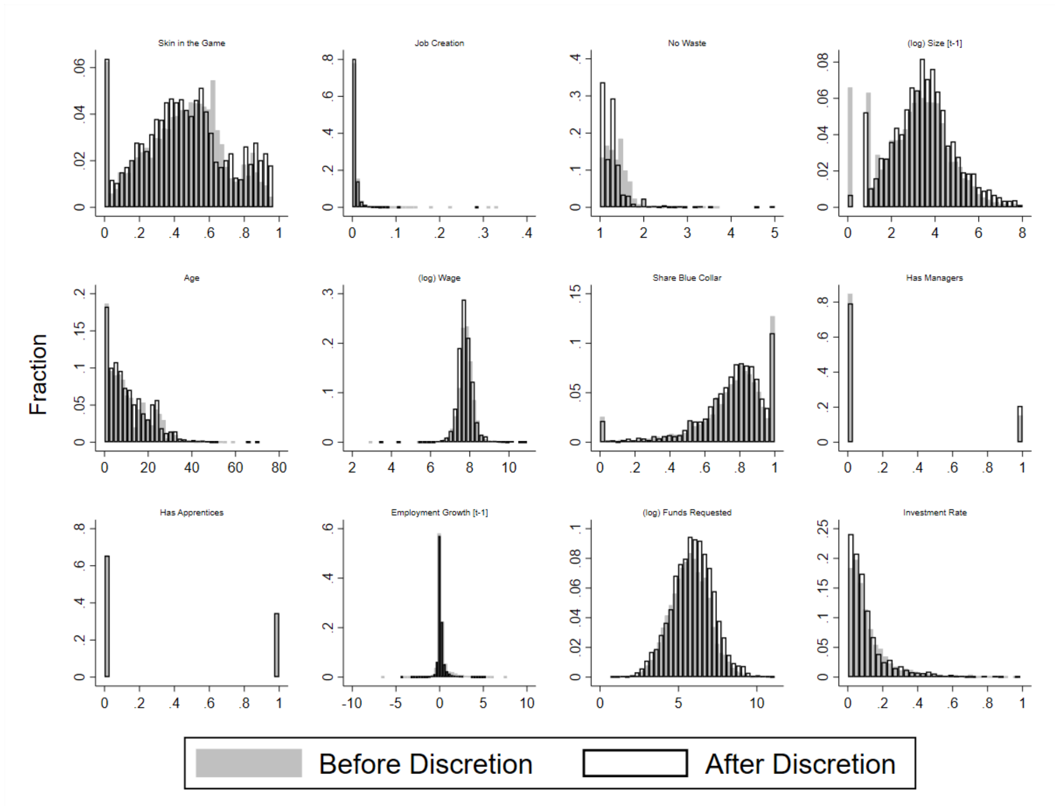
(i) *Employment Growth*



(ii) *Investment*

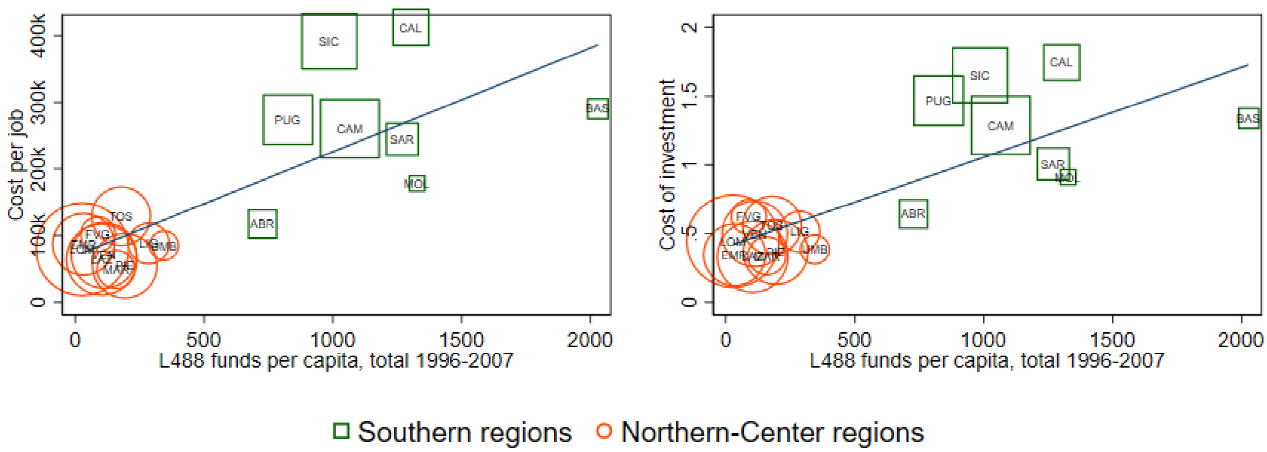
Notes: This figure reports the within-cell average of the regression of the outcome (either employment growth or investment) on $X^{\star}$ with 95% confidence intervals. The 25 cells are defined as the intersection of 5 quintiles of SR and 5 quintiles of SD.

Figure A11: *Balancing of applicants' characteristics before and after the change in the selection rule*



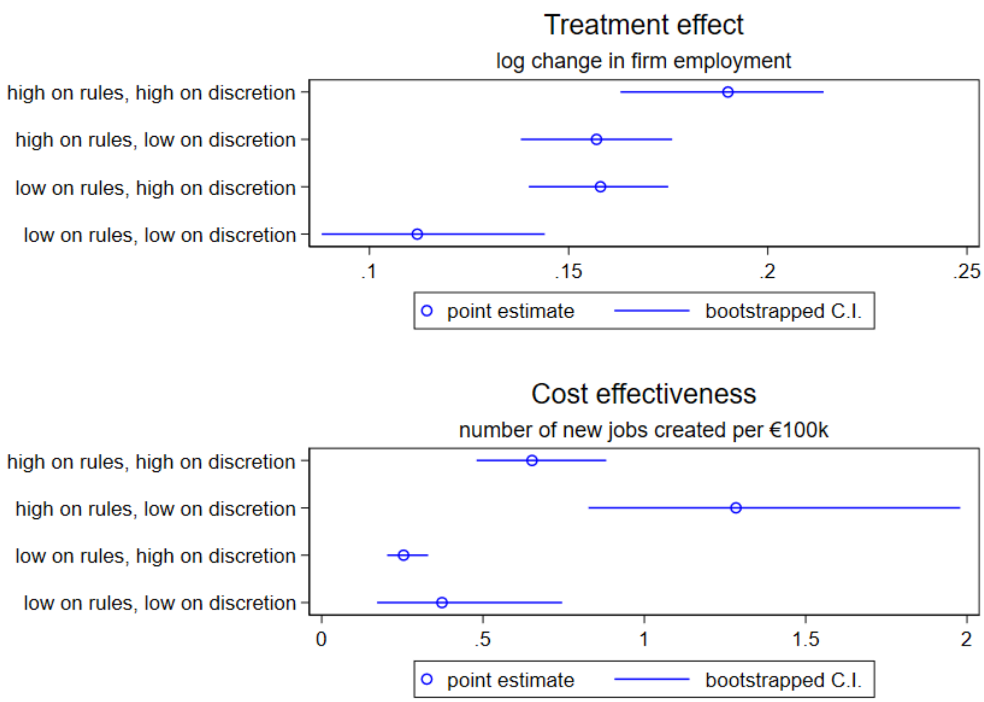
Notes: This figure reports the distribution of the applicants' characteristics in the auctions right before (gray bars) and after (transparent bars) the introduction of political discretion. Only auctions concerning industry are included.

Figure A12: *Cost per job and cost of investment across regions*



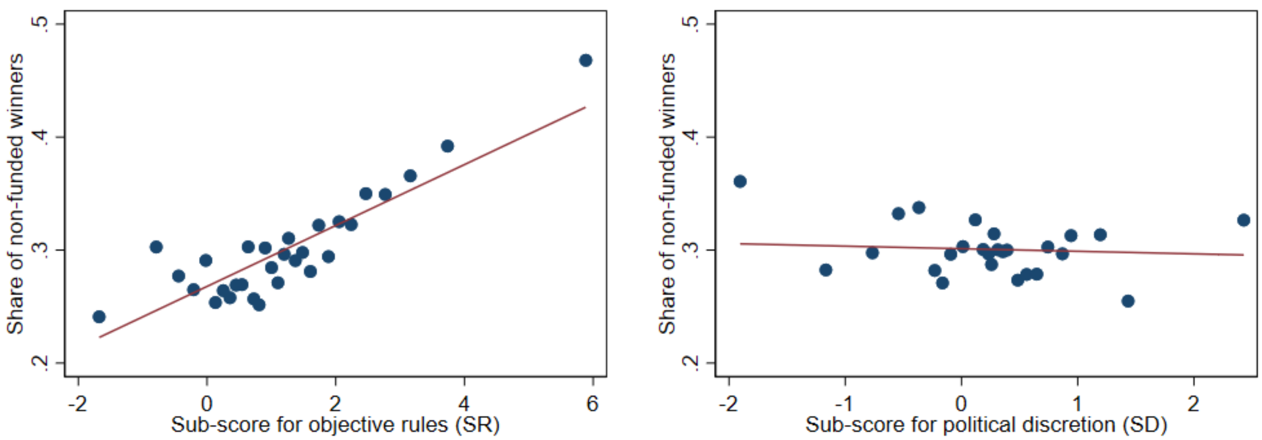
Notes: These are graphs of the estimated cost per job (left graph) and the cost of additional investment (right graph) against the total amount of L488/92 per capita across Italian regions. The size of markers is proportional to regional population.

Figure A13: *Treatment effect and new jobs created per €100,000, rules vs. discretion*



Notes: This figure plots point estimates and bootstrapped confidence intervals for treatment effects on firm employment growth (top graph) and the cost effectiveness of subsidies (bottom graph), for four groups of applicants. Applicants "high on rules" ("low on rules") are those in the top (bottom) quintile of the objective sub-score SR; similarly, applicants "high on discretion" ("low on discretion") are those in the top (bottom) quintile of the discretionary sub-score SD. In practice, the four estimates in each graph refer to the four "corners" of the heatmaps in Figure 10, and 90% confidence intervals are bootstrapped as in Online Appendix Table A7.

Figure A14: *Share of projects scoring above the cutoff that are not funded*



Notes: This figure shows the relationship between the share of eligible projects scoring above the cutoff that are not funded eventually and, respectively, the sub-score for objective rules (left graph) and the sub-score for political discretion (right graph).

Table A1: *List of calls in the L488/92 data*

Call	Type	Ministerial Decree	Official Journal	Projects	€ 2010 bln
1°	Industry I	M.D. 20.11.1996	SG 288 of 09.12.1996, SO 215	7459	4.55
2°	Industry II	M.D. 30.06.1997	SG 174 of 28.07.1997, SO 151	5988	3.06
3°	Industry III	M.D. 14.08.1998	SG 207 of 05.09.1998, SO 149	12364	2.54
⋆	Correction	M.D. 11.09.1998	SG 219 of 19.09.1998, SO 161		
4°	Industry IV	M.D. 18.02.1999	SG 54 of 06.03.1999 54, SO 47	8766	2.46
5°	Special	M.D. 16.07.1999	SG 174 of 27.07.1999	528	-
6°	Tourism I	M.D. 07.12.1999	SG 297 of 20.12.1999, SO 223	2575	0.63
7°	Special	M.D. 29.10.1999	SG 276 of 24.11.1999	791	0.13
8°	Industry V	M.D. 09.04.2001	SG 121 of 26.05.2001, SO 129	8716	2.14
⋆	Correction	M.D. 10.07.2001	SG 186 of 11.08.2001, SO 208		
9°	Tourism II	M.D. 30.11.2001	SG 2 of 03.01.2002, SO 4	2290	0.40
10°	Trade I	M.D. 10.12.2001	SG 12 of 15.02.2002, SO 9	658	0.17
11°	Industry VI	M.D. 12.02.2002	SG 65 of 18.03.2002, SO 47	3870	1.44
12°	Tourism III	M.D. 12.07.2002	SG 185 of 08.08.2002, SO 165	1695	0.40
13°	Trade II	M.D. 10.07.2002	SG 186 of 09.08.2002, SO 167	485	0.15
14°	Industry VIII	M.D. 27.05.2003	SG 157 of 09.07.2003, SO 105	2936	1.00
15°	Tourism IV	M.D. 14.10.2003	SG 278 of 29.11.2003, SO 186	1127	0.32
16°	Trade III	M.D. 14.10.2003	SG 278 of 29.11.2003, SO 186	492	0.05
17°	Industry VIII	M.D. 15.11.2004	SG 281 of 30.11.2004, SO 172	5845	0.72
⋆	Correction	M.D. 14.01.2005	SG 43 of 22.02.2005, SO 23		
18°	Special	M.D. 07.07.2004	SG 170 of 22.07.2004	117	-
19°	Tourism V	M.D. 05.07.2005	SG 185 of 10.08.2005, SO 141	3097	0.27
20°	Trade V	M.D. 05.07.2005	SG 186 of 11.08.2005, SO 142	2103	0.05
22°	Special	M.D. 16.03.2005	SG 110 of 13.05.2005, SO 89	292	0.06
23°	Craftwork	M.D. 23.12.2004	SG 24 of 31.01.2005, SO 13	2036	-
27°	Special	M.D. 09.04.2004	SG 95 of 12.04.2004	12	0.04
28°	Tourism	M.D. 15.11.2005	SG 276 of 26.11.2005	15	0.04
29°	Industry-Tourism	M.D. 04.08.2006	SG 190 of 17.08.2006	15	0.01
31°	Industry	M.D. 30.12.2006	SG 35 of 12.02.2007, SO 34	1957	0.72
32°	Tourism	M.D. 30.12.2006	SG 42 of 20.02.2007, SO 44	685	0.41
33°	Trade	M.D. 30.12.2006	SG 42 of 20.02.2007, SO 45	332	0.08
34°	Craftwork	M.D. 30.12.2006	SG 37 of 14.02.2007, SO 37	549	-
35°	Special	M.D. 29.12.2006	SG 31 of 07.02.2007	19	0.02
Tot				77286	21.82

Notes: This is a list of the calls included in the L488/92 data supplied by the Ministry of Economic Development. The original data did not include 5 of the 35 calls (21, 24, 25, 26, 30), while for 4 other calls we cannot retrieve the total amount of subsidy (5, 18, 23, 34). The rows denoted with a ⋆ indicate corrections to the final official rankings published on the Official Journal. In our analysis we consider the rankings published in the corrections. The 5th, 7th, 18th, 22nd, and 35th calls do not fall within the usual characterization of L488/92, as they were issued to intervene quickly against natural disasters, or tackle particular issues. For example, call 5 targeted projects in the regions of Umbria and Marche hit by the September 1997 earthquake. Call 18 targeted environmentally sustainable projects. The 22nd call was restricted to firms in minor islands, whilst call 7 was limited to Veneto, Marche, Emilia-Romagna, Liguria, and Umbria. Finally, Call 35 was limited to a subset of firms in the province of Salerno.

Table A2: *Balance of firm characteristics one year before the call*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification:	linear				quadratic			
Kernel:	uniform		triangular		uniform		triangular	
Group fixed effects	no	yes	no	yes	no	yes	no	yes
Log employment	0.044	0.002	0.027	0.006	0.035	0.017	0.02	0.026
	(0.043)	(0.034)	(0.04)	(0.034)	(0.048)	(0.04)	(0.048)	(0.04)
Log-change employment	0.016	0.013	0.009	0.011	-0.001	0.005	0.01	0.016
	(0.013)	(0.014)	(0.014)	(0.015)	(0.018)	(0.018)	(0.019)	(0.019)
Log revenues	-0.071	-0.004	-0.102	-0.041	-0.151	-0.094	-0.12	-0.076
	(0.06)	(0.049)	(0.061)	(0.051)	(0.078)	(0.063)	(0.079)	(0.064)
Log-change revenues	-0.021	-0.03	-0.032	-0.038	-0.048	-0.051	-0.036	-0.037
	(0.017)	(0.018)	(0.018)	(0.019)	(0.023)	(0.025)	(0.025)	(0.026)
Log investment	0.022	0.049	0.001	0.022	-0.034	-0.009	-0.027	-0.009
	(0.079)	(0.071)	(0.083)	(0.077)	(0.107)	(0.098)	(0.108)	(0.098)
Log-change investment	0.124	0.088	0.102	0.065	0.066	0.045	0.109	0.088
	(0.065)	(0.067)	(0.064)	(0.066)	(0.078)	(0.081)	(0.084)	(0.086)
Log value added	-0.112	-0.088	-0.165	-0.133	-0.249	-0.208	-0.214	-0.188
	(0.079)	(0.07)	(0.08)	(0.073)	(0.103)	(0.093)	(0.103)	(0.094)
Log-change value added	-0.065	-0.071	-0.073	-0.077	-0.084	-0.088	-0.084	-0.078
	(0.05)	(0.052)	(0.055)	(0.057)	(0.073)	(0.076)	(0.075)	(0.077)
Log VA/worker	-0.081	-0.05	-0.109	-0.083	-0.153	-0.143	-0.15	-0.144
	(0.047)	(0.048)	(0.05)	(0.051)	(0.067)	(0.065)	(0.068)	(0.067)
Log-change VA/worker	-0.077	-0.089	-0.089	-0.099	-0.108	-0.116	-0.097	-0.099
	(0.05)	(0.051)	(0.057)	(0.058)	(0.077)	(0.079)	(0.078)	(0.08)
Firm age	0.261	0.177	0.029	0.029	-0.335	-0.224	-0.333	-0.249
	(0.245)	(0.216)	(0.249)	(0.22)	(0.313)	(0.287)	(0.31)	(0.282)
Start-up	-0.006	-0.001	-0.006	-0.004	-0.007	-0.006	-0.01	-0.009
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)

Notes: This table presents the results from a comparison of firm characteristics one year before the call between applicants scoring just above and just below the cutoff. All variables are described in Online Appendix Table B1. Start-up identifies firms in the age class (0-1). The numbers are the estimated coefficients from RD regressions analogous to (4.1) in which the dependent variable is the firm characteristic indicated in each row, and the main explanatory variable is a dummy equal to one for firms scoring just above the cutoff. The specification in columns (1)-(4) includes the standardized application score, equal to zero at the cutoff, and its interaction with the dummy for applicants above the cutoff, while columns (5)-(8) also include the squared application score and its interaction with the dummy; odd columns include group fixed effects for firms competing in the same ranking; and columns (3)-(4) and (7)-(8) weight observations by a triangular kernel in distance from the cutoff. Standard errors clustered by cell are reported in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Table A3: *Conditional independence tests*

Variable	Left of cutoff	Right of cutoff
Conditional on $X^\star$:		
Running variable	0.0012	-0.0029
t-statistic	0.313	0.334
p-value	0.754	0.734
Unconditional:		
Running variable	0.0388	0.0145
t-statistic	5.155	1.265
p-value	0.000	0.206
Obs	16,007	11,045

Notes: The table reports regression-based tests of the conditional independence assumption in equation (4.2). We regressed employment growth in the six years after the award of L488/92 subsidies on the running variable (i.e., the application score) separately for the sub-samples of applicants above and below the cutoff. The top panel shows the estimated coefficients when controlling for cell fixed effects and for the vector of covariates $X^\star$, while the bottom panel reports the estimated coefficients when controlling only for cell fixed effects. Results are robust to including a quadratic polynomial in the running variable. The covariates included in $X^\star$ are listed at the beginning of Section 6.

Table A4: *Applicants' characteristics before and after the change in the selection rule*

Variable Name	Before discretion	After discretion	Normalized Difference
Skin in the Game	0.458	0.453	-0.019
Job Creation	0.006	0.007	0.098
No Waste	1.288	1.359	0.233
(log) Size [t-1]	3.463	3.095	-0.247
Age	11.297	10.84	-0.048
(log) Wage	7.765	7.803	0.094
Share Blue Collar	0.749	0.751	0.005
Has Managers	0.207	0.153	-0.143
Has Apprentices	0.345	0.344	-0.004
Employment Growth [t-1]	0.136	0.245	0.183
(log) Funds Requested	5.903	5.7	-0.158
Investment Rate	0.113	0.12	0.059

Notes: This table reports averages of applicants' characteristics in the auctions right before and right after the introduction of political discretion, and their normalized difference. Only auctions concerning industry are included.

Table A5: *Policy invariance test, difference-in-differences*

Variable Name	Skin in the Game	Job Creation	No Waste	(log) Size $[t-1]$	Age	(log) Wage
$POST1998 \times DISCRETION$	-0.032	-0.000	0.082	-0.108	0.138	0.052
	[0.027]	[0.001]	[0.086]	[0.073]	[0.280]	[0.014]
Obs	38,367	38,367	38,367	38,367	38,367	34,747
Adjusted R^2	0.109	0.100	0.685	0.121	0.045	0.092
WY p-value	0.826	0.932	0.870	0.746	0.932	0.142

	Share of Blue Collar	Has Managers	Has Apprentices	Employment Growth $[t-1]$	(log) Funds Requested	Investment Rate
$POST1998 \times DISCRETION$	-0.004	-0.005	-0.065	-0.018	-0.030	0.008
	[0.011]	[0.016]	[0.012]	[0.012]	[0.062]	[0.007]
Obs	34,747	38,367	38,367	34,819	38,367	15,104
Adjusted R^2	0.020	0.070	0.053	0.006	0.230	0.009
WY p-value	0.932	0.932	0.104	0.736	0.932	0.826

Notes: This table shows the results of difference-in-differences regressions comparing project and applicant characteristics between regions attributing and not attributing discretionary points, before and after the introduction of discretion. In particular, we estimate the specification $Y_{irt} = \phi(POST1998_t \times DISCRETION_r) + FE_r + FE_t + virt$, where $POST1998_t = 1$ for the period after 1998 and $POST1998_t = 0$ otherwise, $DISCRETION_r = 1$ in regions attributing discretionary points and $DISCRETION_r = 0$ otherwise, and FE_r and FE_t are region and year fixed effects, respectively. Robust standard errors clustered at the region-year level and reported in brackets. The last row reports Westfall & Young (1993) p-values corrected for multiple-hypothesis tests.

Table A6: *Cost of new jobs and investment generated by L488 subsidies*

	(1)	(2)	(3)	(4)	(5)	(6)
Cost Measure	cost per new job (thousand of €'s)		cost per worker-year (thousand of €'s)		cost of new investment (thousand of €'s)	
$X^{\star}$	manual	data-driven	manual	data-driven	manual	data-driven
all firms	178	159	54	56	0.81	0.63
	[133; 299]	[118; 260]	[47; 62]	[51; 62]	[0.59; 1.25]	[0.48; 0.87]
large	78	78	24	29	0.4	0.29
	[47; 222]	[52; 174]	[19; 30]	[25; 35]	[0.27; 0.74]	[0.21; 0.41]
small	253	209	80	74	1.08	0.87
	[203; 349]	[162; 296]	[73; 90]	[68; 81]	[0.83; 1.49]	[0.68; 1.15]

Notes: This table shows the cost of new jobs and investment generated by the L488 subsidies over a six-year period. All amounts are expressed in euros at constant 2010 prices. The estimates in columns labelled as "manual" employ the set of covariates listed at the beginning of Section 6, while the estimates in columns labelled as "data-driven" employ the set of covariates selected by the algorithm described in detail in Section S2 of the Supplementary Materials. 90% confidence intervals are reported in brackets and are computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level.

Table A7: *Point estimates and confidence intervals of treatment effect and average cost per new job, rules vs. discretion*

Panel A: Treatment Effect

Quintiles of sub-score SD		Quintiles of sub-score SR: 1	2	3	4	5
	5	0.158	0.188	0.172	0.180	0.190
		[.14; .175]	[.163; .217]	[.151; .195]	[.159; .201]	[.163; .214]
	4	0.126	0.155	0.163	0.167	0.149
		[.099; .156]	[.131; .179]	[.141; .191]	[.137; .199]	[.129; .172]
	3	0.164	0.163	0.171	0.181	0.176
		[.126; .193]	[.139; .185]	[.148; .189]	[.154; .203]	[.152; .197]
	2	0.122	0.151	0.148	0.166	0.171
		[.09; .154]	[.127; .171]	[.122; .168]	[.133; .194]	[.142; .192]
	1	0.112	0.107	0.132	0.135	0.157
		[.088; .144]	[.084; .136]	[.107; .156]	[.114; .155]	[.138; .176]

Panel B: Cost Effectiveness

Quintiles of sub-score SD		Quintiles of sub-score SR: 1	2	3	4	5
	5	0.254	0.340	0.385	0.465	0.652
		[.203; .33]	[.269; .468]	[.286; .519]	[.291; .695]	[.48; .882]
	4	0.359	0.491	0.505	0.682	1.157
		[.261; .502]	[.306; .852]	[.348; .754]	[.466; .984]	[.751; 1.739]
	3	0.487	0.428	0.526	0.681	1.054
		[.388; .677]	[.337; .577]	[.39; .772]	[.493; .991]	[.611; 1.648]
	2	0.466	0.434	0.444	0.610	1.149
		[.324; .741]	[.343; .589]	[.32; .754]	[.472; .837]	[.588; 1.935]
	1	0.373	0.703	0.753	0.816	1.284
		[.172; .745]	[.505; .987]	[.595; .999]	[.621; 1.124]	[.827; 1.979]

Notes: This table reports the heterogeneity in treatment effects on firm employment growth (Panel A) and the cost effectiveness of subsidies (Panel B), by quintiles of the sub-scores for objective rules (SR) and political discretion (SD). In Panel A, the treatment effect for each bin ($SR = r, SD = d$) is estimated as $\mathbb{E}[Y^1 - Y^0 \mid SR = r, SD = d] = (\beta_1 - \beta_0) \cdot \mathbb{E}[X^\star \mid SR = r, SD = d]$. The covariates included in $X^\star$ are listed at the beginning of Section 6. In Panel B, cost effectiveness is measured by the number of newly created per €100,000 of subsidies received by the firm. The number of newly created jobs in each bin is computed by multiplying the size of each firm by the treatment effect for its respective bin, as reported in Panel A, and aggregating across all firms in that bin. 90% confidence intervals are reported in brackets and computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level.

B Data Description

The analysis leverages on three main sources of micro data: 1. The administrative data on all applications for L488/92 subsidies (1996-2007), sourced from the Italian Ministry of Economic Development, DG Firm subsidies; 2. The National Social Security Institute (INPS) firm archive (called DM10M) covering the universe of Italian firms with at least one dependent worker, available starting from 1986; and 3. The Cerved database containing balance sheet information on Italian limited liability companies, available starting from 1993.

The L488/92 archive contains administrative data on 74,584 applications for L488/92 subsidies, submitted by 49,082 firms. It cover nearly the universe of rankings (only some smaller auctions are excluded) for which it contains all submitted applications.

The data contain:

(i) information on the project: a unique project identifier; the three (five) sub-indexes measuring project quality; the score (the forcing variable) obtained aggregating the sub-indexes standardized at the auction and region level; the position in the ranking; an indicator for winning projects; the amount of funds requested in the application and that of funds actually transferred - separate by each of three instalments; whether financed on EU or Italian sources.

(ii) information on the auction (number, region and sector of destination of funds, date of issuance, date of closure, dates of each of the three instalments).

(iii) information on the firm (fiscal identifier or individual fiscal code (in case of sole proprietorships), legal form, address, municipality.

Additional information on the auction, recovered from the Official Journal of the Italian Republic (*Gazzetta Ufficiale*), associate each project to "cells" identified by the following dimensions: firm size (Large/Medium/mall), sector (Industry, energy, Tourism, Trade, Services), eligibility for co-financing (Yes/No), and geographical area (Region). This additional information allowed to exactly allocate projects to the several sub-rankings within within the same call, region, and (possibly) special category of applicants (see Section S1 of the Supplementary Materials).

The firm archive is assembled by INPS sourcing on a master dataset collecting all social security payments made every month by legal entities for any employee with open-ended, fixed-term and apprenticeship contract. The archive covers therefore the universe of firms with at least one employee at some point during a given calendar year. The data is available between 1986 and 2015. For each firm it reports the fiscal code; monthly information on the number of employees; and yearly information on the number employees and their total wage bill by qualification (manager, blue collar; white collar; apprentices; others); date of birth and cessation of activity; detailed geographical (municipality) and industry (3-digit) data; and an identified for firms belonging to groups.

Information on firms' balance sheet and income statement comes from a proprietary database assembled by the Cerved Group S.p.a. The Cerved Firm Registry, which is the Italian source of the Orbis database, covers the universe of limited liability firms in the private non-financial sector and is available since 1993.

Further data used in the paper include: (*i*) the administrative registries of local politicians and local elections, available from the Italian Ministry of Interior, https://dati.interno.gov.it/; and (*ii*) a classification of local governments' ideologies, sourced from the Local Opportunities Lab, https://www.localopportunitieslab.it/.

Table B1: *Description and source of all the variables used in the analysis.*

Variable	Description	Source
	Main variables from L488/92 data	
Info on Auctions	Date, region, and result of the auction. Complementary information from the Official Journal includes, for each project, all the details required to recover the rankings within each auction-region cell, as explained in Section S1 of the Supplementary Materials	MinEcDev
Score	Project quality obtained combining the 3(5) indicators below, once standardized within each call-region	MinEcDev
Skin in the Game	Ratio of the applicant's own investment in the project relative to the amount requested	MinEcDev
Job Creation	Number of jobs created by the project	MinEcDev
No Waste	Proportion of funds requested in relation to an ad-hoc benchmark set by the EU Commission	MinEcDev
Political Discretion	Score attributed on the basis of priorities indicated by the regional government	MinEcDev
Environmental Responsibility	Compliance with the requirements of an environmental management system, e.g. ISO 14001 or EMAS	MinEcDev
Funds Requested	Amount of subsidies requested in application	MinEcDev
Funds Paid	Amount of subsidies disbursed to winners, in three instalments	MinEcDev
	Main variables from INPS	
Size	Number of employees, available at monthly frequency	INPS
Growth	Employment growth rate between two dates. Computed over different horizons starting and ending in the month of the auction	INPS
Age	Firm age at any given year	INPS
Wage	Average wage of employees. Obtained aggregating yearly data on wage bill and employees by qualification (managers, blue collar; white collar; apprentices; others)	INPS
Share of blue collars	Ratio between blue collar employees and total employees, computed from the same data	INPS
Manager	Dummy for presence of (middle) managers in workforce	INPS
Apprentices	Dummy for presence of apprentices in workforce	INPS
Survival	Dummy for whether firm is alive at any given future horizon	INPS
Area	Headquarter municipality	INPS
Industry	3-digits NACE Rev. 2 industry codes	INPS
	Main variables from CERVED	
Revenues	Firm revenues (sales) (thousands of €)	CERVED
VA	Firm value added(thousands of €)	CERVED
Total Assets	Total assets (thousands of €)	CERVED
Investment	Investments in tangible and intangible fixed assets (thousands of €)	CERVED

Description and source of all the variables used in the analysis (CONT.).

Variable	Description	Source
Political proximity and other predictors of the discretionary score (SD)		
Political alignment	Dummy for the same party (right, centre, left, civic) ruling both the Region and the municipality the firm is located	Ministry of Interior and Local Opportunities Lab
Margin of victory	Dummy for the margin of victory in the last elections municipality the firm is located	Ministry of Interior
Birthplace of Regional president	Dummy for the president of the Regional government being born in the municipality the firm is located	Ministry of Interior
Birthplace of Regional counsellor	Dummy for at least one counsellor in the Regional government being born in the municipality the firm is located	Ministry of Interior
Birthplace of Regional alderman	Dummy for at least one alderman in the Regional government being born in the municipality the firm is located	Ministry of Interior
Birthplace of mayor	Dummy for the mayor of the municipality being born in the municipality the firm is located	Ministry of Interior
Human capital of Regional president / municipality mayor	Dummy for level of schooling of Regional president / municipality mayor (primary, lower sec., high school, university degree)	Ministry of Interior
Local unemployment	Unemployment rate at province level (ISTAT)	ISTAT
Credit constraints	Spread between loan and deposit rates in provinces	Guiso et al. (2013)

C RD estimates at the cutoff: Additional results

Two important issues could affect the interpretation of our RD estimates in section 5. First, applicants in a given call may re-apply (and obtain funds) in subsequent calls. Second, the effects on funded firms may spill over to other, non-funded firms.

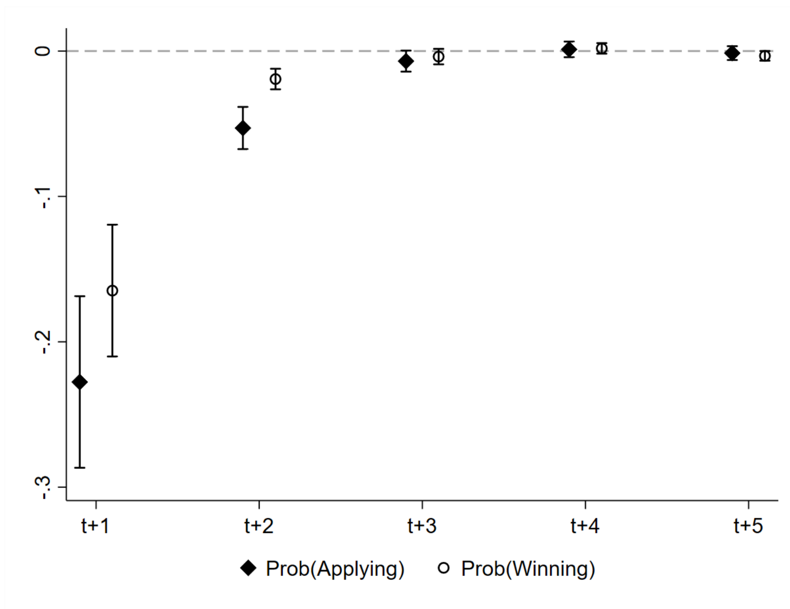
C.1 Total and direct effects when applicants can re-apply

The outcome of applications submitted in year t may affect the probability of re-applying for funds – and, therefore, obtaining the subsidy – in later years, say at $t + \Delta$. In this case, the dynamic treatment effects on outcomes from $t + \Delta$ onwards would reflect both the direct effect of the subsidy obtained at time t, and the indirect effect through a different probability of obtaining subsidies in subsequent years. The sign of the indirect effect is a priori unclear. On the one hand, firms obtaining funds in year t may not have additional (promising) projects to submit in year $t + \Delta$, or they may be constrained in the amount of own resources that could be invested. In this case our estimates provide a lower bound for the direct effect of obtaining the subsidy at time t. On the other hand, obtaining funds in year t may improve the chances of succeeding in year $t + \Delta$, due for example to increased availability of resources or reputation effects, in which case we would be over-estimating the direct effects of the subsidy.

In practice, we sign the (indirect) effect of obtaining a subsidy on the probability of obtaining additional funds in the following years using our baseline RDD specification (4.1). Figure C1 shows that applicants scoring just above the cutoff in year t have a 23 percentage point *lower* probability of re-applying for funds in year $t + 1$, and a 16 percentage point lower probability of actually obtaining such funds. These differences decrease markedly in year $t + 2$ to eventually disappear from $t + 3$ onward. Therefore, the estimated coefficients in Table 3 and Figure 7 under-estimate the direct, dynamic treatment effects of the subsidy.

This is not an issue for the internal validity of our estimates, as receiving less subsidies between t and $t + \Delta$ is itself a causal effect of the subsidy received at time t. In terms of external validity, however, we may want to distinguish between direct and indirect effects, as the latter would not apply in the context of one-off interventions. We thus extend the estimating equation (4.1) to allow for dependence of firm outcomes on subsidies received in *all* previous calls. We illustrate our procedure with reference to a two-period case. Let the model for the call in period $t = 1$ be the standard one:

Figure C1: *Direct and indirect effects for re-applicants*



Notes: The graph shows the estimated effect of obtaining the L488/92 subsidy in year t on the probability of re-applying for the same subsidy (black markers) and obtaining it (grey marker) in subsequent years, as estimated from the RD regression 4.1. 95% confidence intervals are also shown in the graph.

$$Y_1 = \tau_1 D_1 + \gamma_1 S_1 + \delta_1 D_1 \cdot S_1 + \varepsilon_1 \tag{C.1}$$

where all variables are defined as in equation (4.1), and the sub-index "1" denotes the period.[25] With repeated interventions, the causal effect of the subsidy received in period $t = 1$ on the outcome in period $t = 2$ would read as

$$Y_2 = \tau_2 D_2 + \gamma_2 S_1 + \delta_2 D_1 \cdot S_1 + \tilde{\tau}_2 D_1 + \varepsilon_2,$$

where we explicitly take into account that in period 2 some units among those applying for the subsidy in $t = 1$ might apply to the new call and possibly receive the subsidy in $t = 2$, which would have an effect on Y_2 as large as τ_2. If we knew τ_2, the following regression would be

[25] We consider the case of a linear regression in S to simplify notation (i.e., $k = 1$ in equation 4.1), but it is immediate to allow for higher-order polynomials in S.

suitable to properly estimate $\tilde{\tau}_2$ (i.e., the causal effect of D_1 on the outcome in $t = 2$):

$$Y_2 - \tau_2 D_2 = \gamma_2 S_1 + \delta_2 D_1 \cdot S_1 + \tilde{\tau}_2 D_1 + \varepsilon_2. \tag{C.2}$$

An estimate of τ_2 could be recovered from a regression analogous to (C.1), run on firms participating in the call issued in period $t = 2$ but not in the previous call.

In practice, with calls issued across several subsequent years, we estimate (C.1) allowing for year-specific coefficients τ_1^t ($t = 1996, ..., 2006$) in a sample including only firms applying for the first time. Year-specific contemporaneous coefficients are then used to "net" outcomes of firms applying in two consecutive years: $\tilde{Y}_2 = Y_2 - \tau_1^t D_2$.[26] Finally, the one-year-ahead direct effect of the subsidy $\tilde{\tau}_2$ is obtained by RDD using $\tilde{Y}_2$ on the left-hand-side of equation (C.2). The procedure is then iterated to estimate the direct effects of the policy at further horizons.

Figure C2 compares the total effect of the subsidy received at time t on employment growth at different time horizons, as reported also in Table 3 and Figure 7, with the direct effect obtained by subtracting the effect of subsequent subsides, estimated following the procedure described above. As expected, in light of the evidence in Figure C1, the direct effect is larger than the total effect, as the latter also includes the indirect, negative effect going through a lower probability of re-applying for subsidies after obtaining it. However, the difference between direct and total effects remains small.

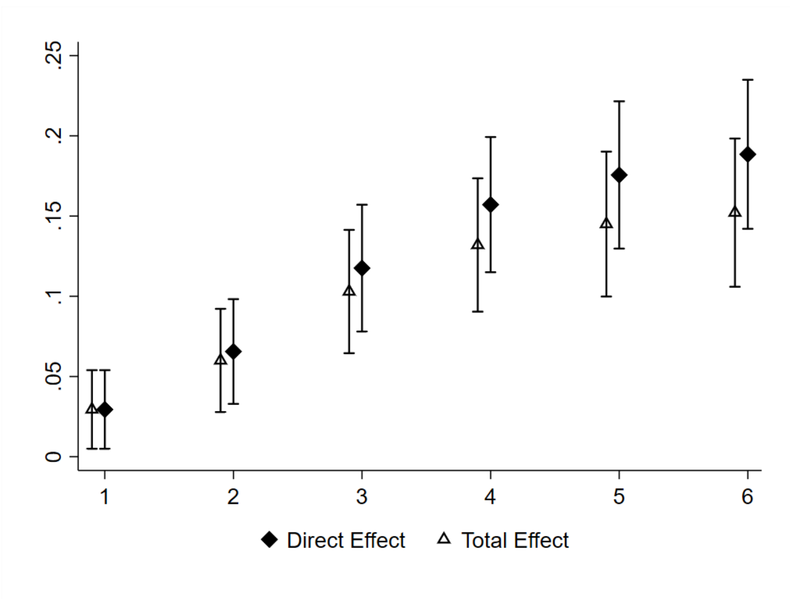
C.2 Spillovers

Employment increases by subsidized firms may affect other, non-subsidized firms. The sign of these effects is also unclear a priori. The growth of subsidized firms may benefit upstream and downstream producers in the same market, or it may erode the market share of competitors – possibly including firms in the control group. In the latter case, estimates in section 5 would overstate the effects of the policy.

To address this possibility, we look across Italian Local Labor Markets (LLM) comparing the employment dynamics of non-subsidized firms in subsidized LLMs to those of firms in non-subsidized LLMs; spillover effects should affect more (or exclusively) employment in the former group. We focus on the following specification:

[26] For example, the outcomes of a firm applying for the first time in 2001 and then also in 2002 would be Y_{2001} and $\tilde{Y}_{2002} = Y_{2002} - \tau_1^{2002} D_2$

Figure C2: *Total and direct effects for re-applicants*



Notes: The graph compares the total effect of obtaining a subsidy, as estimated in Table 3 and Figure 7 (second graph), with the direct effect obtained by subtracting the contemporaneous effect of any subsidy obtained in subsequent calls, as detailed in equations (C.1) and (C.2).

$$\ln L_{m,t+k} - \ln L_{m,t} = \theta_k D_{m,t} + \alpha \ln L_{m,t} + FE_m + FE_t + \varepsilon_{m,t} \quad \text{(C.3)}$$

where $L_{m,t+k}$ and $L_{m,t}$ are the total employment of non-subsidized firms in the m-th LLM in year $t+k$ and t, taken from the INPS administrative data on the universe of workers in (non-agricultural) firms; $D_{m,t}$ is a dummy equal to 1 when at least one firm in LLM m received funds in year t; FE_m and FE_t are LLM- and year-specific fixed effects; and $\varepsilon_{m,t}$ is a residual summarizing the effect of other factors. The coefficient of main interest, θ_k, captures the differential employment response, after k years, of non-subsidized firms within the same LLM as a subsidized firm relative to non-subsidized firms in other LLMs.

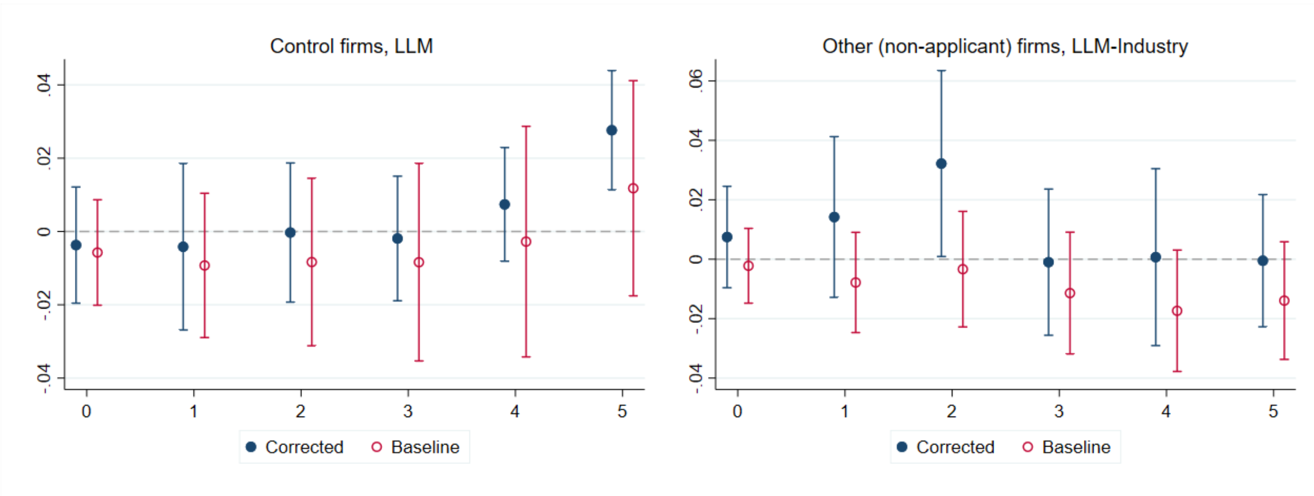
Figure C3i plots the estimated coefficients θ_k's for two different subsets of non-subsidized firms – respectively, applicant firms not obtaining the subsidy (left graph) and non-applicant firms in the same LLM-industry cell as subsidized firms.[27] Both graphs present baseline difference-in-differences estimates as well as "corrected" estimates accounting for the staggered research design, using the approach suggested by de Chaisemartin & D'Haultfœuille (2020). Overall, there is no evidence of significant spillover effects; the same is true when replacing the binary indicator $D_{m,t}$ in equation C.3 with the (log of) funds actually paid to subsidized firms in each LLM or LLM-industry, see Figure C3ii.

These results imply that higher employment among subsidized firms reflects a net increase in aggregate employment, rather than a mere reallocation of jobs from non-subsidized to subsidized firms. Cerqua & Pellegrini (2022) reach the same conclusion by decomposing worker flows towards subsidized firms. Using worker-level data, they show that the majority of recruits come from new entrants in the labor market, and conclude that L488/92 subsidies generate few displacement effects across firms, if at all.

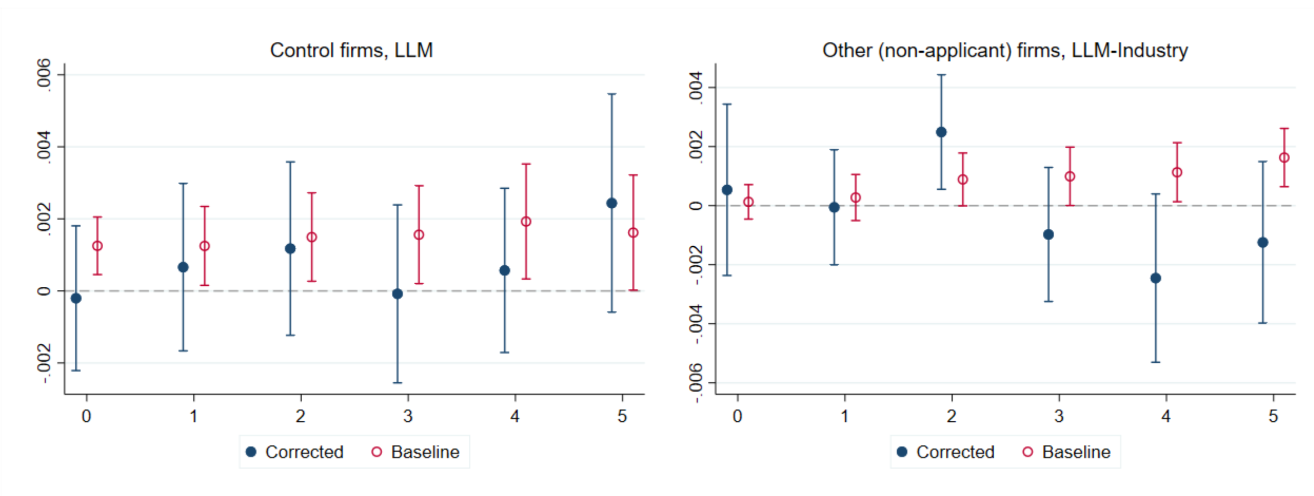
[27] Industry is defined at the 3-digit level.

Figure C3: *Spillover effects on other firms in the same labor market*

(i) *Using a binary indicator for presence of subsidized firms in LLM*



(ii) *Using the log of total subsidies in LLM*



Notes: The graphs show the estimated spillover effects of the subsidy on local employment at different time horizons, indicated on the horizontal axis, and associated confidence intervals (at the 90% significance level). The left panel plots the aggregate employment response of control firms located in the same LLM as treated firms. The left panel focuses on non-participating firms in the LLM and (3-digit) industry as treated firms. The treatment variable is the log of funds received by treated firm in a LLM (or LLM-industry cell). "Baseline point" estimates and confidence intervals are obtained from specification C.3 in the main text, clustering heteroskedasticity-robust standard errors by LMM. "Corrected" coefficients are obtained using the estimator proposed by de Chaisemartin & D'Haultfœuille (2020) to account for biases arising if group-time treatment effects are averaged with negative weights.

Supplementary Materials

S1 Construction of sub-rankings of L488/92 applications

As explained in Section 2, the final ranking of L488/92 applicants mainly depends on three criteria in the first two calls for projects (*skin in the game*, *job creation*, *no waste*), plus two additional criteria in subsequent calls (*political discretion* and *environmental responsibility*). In addition, separate rankings were formed by (i) firm size, (ii) activity in the service sector, (iii) eligibility to receive EU funds, and (iv) EU objective area in which a firm operates. These four additional criteria entered the formation of the final ranking by either reserving part of the total budget for specific categories of firms (i-ii) or by making additional EU funds available for specific types of projects (iii-iv).

Firm size. Each region had to commit 50% of its L488/92 budget to small and medium enterprises (i.e., fewer than 250 employees, turnover under €50 million, or balance sheets below €43 million).

Figure S1 provides one example from the second call, as published in the Official Journal. The projects are sorted in decreasing order according to the final score (in column L). Looking at funds allocation (column T) reveals that the projects ranked 90th and 92nd (ID 75995 and 7939) were declared eligible, while those ranked 88th (ex-aequo, ID 90634 and 38259) were not, despite their higher score. This is because the first two were submitted by a medium and a small firm, while the other two were submitted by large firms (see column N: "G" stands for large, "M" for Medium and "P" for small).

Had these projects been selected for funding, the 50% quota reserved for small and medium-sized firms would have been violated.

Activity in the service sector. Firms operating in the service sector could receive at most 5% of the regional budget. Therefore, a project could be selected to receive funds even if it had a lower score than another project submitted by a company operating in the services sector. This case is illustrated in Figure S2.

Figure S1: *Extract of the ranking published in the Official Journal.*

A Posiz. in grad.	B Numero di prog.	C RAGIONE SOCIALE	D I1 Capitale proprio	E I2 Occupazione attività	F I3 Agevolazione richiesta	G I1N Capitale proprio	H I2N Occupazione attività	I I3N Agevolazione richiesta	L Somma indicatori normalizz.	M Sett. serv.	N Dim	O Ob	Q Cof	Q Esito finale	R Cod esc.	S Risor	T Agevolaz concessa L mil.
80	75299		07300000	00103263	11111111	07150427	04611723	-02334272	094278780		M	1	S	A		N	994 62
81	90303		07300000	00101891	11111111	0,7150427	04451254	-02334272	0,92674090		M	1	S	A		N	326,55
82	15165		07356807	00068273	11764706	0,7428728	00519287	01297118	0,92451330		M	1	S	A		N	2.545,50
83	8219		06347110	0,0103466	1,1904762	02482163	04635466	0,2075272	091929010		P	1	S	A		N	200,79
84	38337		07450000	0,0062719	1,1764706	07885286	-00130310	0,1297118	0,90520940		P	1	S	A		N	670,38
85	45619		07480000	0,0053331	1,1904762	08032257	-01228332	0,2075272	0,88791970		P	1	S	A		N	1 358,73
86	64729		07037122	0,0041324	1,2500000	05862572	-02632673	0,5382429	0,86123280		P	1	S	A		N	8 191,05
87	75998		0,5503532	0,0105457	1,2500000	0,1650575	0,4868334	0,5382429	0,86001880		M	1	S	A		N	754,89
88	90634		0,8000000	0,0000000	1,2500000	1,0579768	-0,7465935	0,5382429	0,84962620		G	1	S		4		0,00
88	38259		0,8000000	00000000	1,2500000	1,0579768	-0,7465935	0,5382429	0,84962620		G	1	S		4		0,00
90	75995		0,5698718	00213675	1,0000000	-0,0694347	1,7525530	-0,8507631	0,83235520		M	1	S	A		N	833,16
91	50826		0,5441235	00063659	1,3333333	-0,1955771	-0,0020367	1,0012447	0,80363090		G	1	S		4		0,00
92	7939		0,7540000	00029062	1,2195122	0,8326201	-0,4066839	0,3688519	0,79478810		P	1	S	P	1	N	2.390,31
93	1707		0,5388774	00095278	1,2658228	-0,2212781	0,3677796	0,6261547	0,77265620		M	1	S		1		0,00
94	1681		0,5152374	0,0140829	1,1904762	-0,3370918	0,9005449	0,2075272	0,77098030		P	1	S		1		0,00

Notes: This is a snapshot from a ranking of the second call published in the Official Journal. The first column (A) shows the position in the ranking, the second (B) the ID of the project, and the third (C) the company name, which we omit. Then there are 7 columns (D-L) that contain data on the raw sub-indexes, normalized sub-indexes and aggregated index presented in Section 2. The last columns indicate: whether the firm is active in the service sector (M), the size of the firm (N), the EU Objective area where the firm operates (O), the firm's eligibility to receive EU funding (P), the outcome of the application (Q), the reason for non-selection (R), the source of funding received (S), the amount of funding (T). Source: *Gazzetta Ufficiale, SG 174 of 28.07.1997, SO 151, p.68*.

Figure S2: *Extract of the ranking published in the Official Journal.*

LEGGE 488/92 - BANDO DEL 2000 (8°) DEL SETTORE INDUSTRIA - GRADUATORIA ORDINARIA DELLA REGIONE LIGURIA — **Allegato 2/10**

NUMERO INIZIATIVE IN GRADUATORIA 113

	Indicatore 1	Indicatore 2	Indicatore 3	Indicatore 4	Indicatore 5
MEDIE	0,5582615451	0,0035209584	1,1729751788	19,8230088496	6,8716814159
DEVIAZIONI STANDARD	0,2193934539	0,0055085503	0,2925841006	5,3838881471	3,6245558601

A Posiz. in grad.	B Numero di progetto	C Ragione Sociale	D Prov.	E 1 Capitale proprio	F 2 Occupazione attivata	G 3 Agevolazione richiesta	H 4 Indicatore Regionale	I 5 Indicatore Ambientale	L Somma indicatori normalizzati	M Sett. Serv.	N Dimen-sione	O Ob.	P Cofin	Q Esito con-clusivo	R Cod. escl.	S Agevolaz. Concedibile (LM)	T Agevolaz. Concedibile (Euro)
1	52111 - 11		GE	36,4458900	0,045	1,0526 %	20	10,00000	07,0763729	S	P	2	SI	A		85,52	44.167
2	66443 - 11		GE	51,6017900	0,001	2,9412 %	20	10,00000	06,3481123		M	2	SI	A		1.299,06	670.908
3	68960 - 11		GE	40,6230200	0,001	2,8571 %	20	7,0000000	04,6858531	S	G	2		P	2	638,88	329.954
4	67097 - 11		SP	89,6628000	0,024	1,5385 %	20	0,0000000	04,5825445		G	2		A		99,74	51.511
5	40226 - 11		GE	70,4517800	0,010	1,1111 %	30	10,00000	04,4082812	S	P	2	SI	N	2	-	-
6	20788 - 11		GE	30,7919600	0,002	2,0000 %	30	10,00000	04,1099450	S	P	2	SI	N	2	-	-
7	67085 - 11		GE	85,7800000	0,002	1,2658 %	30	10,00000	04,1029045		P	2	SI	A		871,86	450.278
8	20903 - 11		GE	84,7000000	0,002	1,1765 %	30	10,00000	03,8635997	S	P	2	SI	N	2	-	-
9	20709 - 11		GE	83,5347900	0,003	1,1364 %	30	10,00000	03,8450711		P	2	SI	A		254,94	131.665
10	20649 - 11		SV	67,7305500	0,007	1,1111 %	30	10,00000	03,6940373		P	2	SI	A		477,15	246.427

Notes: This is a snapshot from a ranking of the eighth call published in the Official Journal. The first column (A) shows the position in the ranking, the second (B) the ID of the project, the third (C) the company name, which we omit, and the fourth (D) the province where the company was located. Then there are 6 columns (E-L) that contain data on the five normalized sub-indexes presented in Section 2, as well as the overall index. The last columns indicate whether the firm is active in the service sector (M), the size of the firm (N), the EU Objective area where the firm operates (O), the firm's eligibility to receive EU funding (P), the outcome of the application (Q), the reason for non-selection (R), the amount of funding received in millions Italian Lire (S), the same amount in euros (T). Source: *Gazzetta Ufficiale, SG 186 of 11.08.2001, SO 208, p.29*.

As before, projects are sorted by the score received (column L). However, the project in 7th place with ID 67085-11 was funded even though it had a lower score than the project in 6th place with ID 20788-11. This is because the latter was submitted by a service provider and the 5% upper bound had been reached (see column M, where "S" stands for service provider).

Eligibility for EU funds. Projects meeting certain criteria – in terms of location and type of activities, duration of investment, and the amount of eligible expenses – were eligible for co-funding from the European Regional Development Funds (ERDF). These projects might be selected over higher-ranked projects that were eligible for national funds only.

Figure S3: *Extract of the ranking published in the Official Journal.*

A Posiz. in grad.	B Numero di prog.	C Ragione Sociale	D 1 Capitale proprio	E 2 Occupazione attivata	F 3 Agevolazione richiesta	G 4 Ind. reg.	H 5 Ind. amb.	I Somma indicatori normalizz.	L Sett. serv.	M Dim.	N Ob.	O Cof.	P Esito finale	Q Cod. Escl.	R Risorse	S Agevolaz. concedibile L. mil.	T Agevolaz. concedibile Euro
163	12380		0,6586178	0,0000000	2,0000000	0	4	0,52555960		G	2		N	1		0,00	0
164	15042		0,5190234	0,0026975	1,1111111	1	6	0,51754010		P	2	S	A		C	283,53	146.734
165	3955		0,4065680	0,0020112	1,2500000	1	6	0,48736070		P	5		N	1		0,00	0
166	5814		0,3706947	0,0061005	1,2500000	1	5	0,48524370		P	2	S	A		C	146,60	75.869
167	15338		0,9300000	0,0000000	1,4285714	0	6	0,44496170		G	2		N	1		0,00	0
168	40967		0,1657733	0,0033984	1,1764706	1	8	0,44400580		M	2	S	A		C	835,28	432.279
169	16944		0,3165459	0,0022000	1,0526316	1	8	0,39952000		P	5	S	A		C	325,35	168.377
170	40418		0,2326934	0,0058173	1,2500000	1	6	0,38718090		P	2	S	A		C	94,71	49.015
171	40416		0,1642957	0,0005917	1,6666667	1	5	0,38703570		P	2		N	1		0,00	0
172	12997		0,2303593	0,0012955	1,1764706	1	8	0,38296540		P	2	S	A		C	233,42	120.801

Notes: This is a snapshot taken from one ranking of the eight calls published in the Official Journal. The first column (A) shows the position in the ranking, the second one (B) the ID of the project, and the third one (C) the company name, which we omit. Then, there are 6 columns (D-I) containing data on the five normalized sub-indexes presented in Section 2, and the aggregate index. The last columns report: whether the firm operates in the services sector (L), the dimension of the firm (M), the EU Objective area the firm operates in (N), the firm's eligibility for EU funding (O), the outcome of the application (P), the reason for not being selected (Q), the source of funds received (R), the amount of funds received expressed in millions of Italian Lire (S), the same amount expressed in Euro (T). Source: *Gazzetta Ufficiale, SG 54 of 06.03.1999 54, SO 47, p.28*.

This case is portrayed in Figure S3. The projects ranked 171st and 172nd (IDs 40416 and 12997) were both presented by small firms. However, only the second, lower scoring project received funding. This is because it had access to EU funds while the first one did not, and the national funds were already exhausted (eligible projects are marked with an "S" in column O; the "C" in column R indicates that the funds received were co-financed, whilst "N" denotes national funding).

EU Objective Area. Even projects eligible for EU funding could be subject to constraints on the type of ERDF program. In particular, firms in Northern and Central regions could tap either Objective 2 funds (if located in areas in industrial decline) or Objective 5b funds (if in disadvantaged rural areas), and the budget available for either source of funds would typically be different. Figure S4 shows an example in which all projects submitted by firms operating in an Objective 5b area were not selected due to exhaustion of the corresponding funds, while all Objective 2 projects were selected, even if such projects received a lower score.

Figure S4: *Extract of the ranking published in the Official Journal.*

			INDICATORI NON NORMALIZZATI				INDICATORI NORMALIZZATI										
A Posiz in grad	B Numero di prog	C RAGIONE SOCIALE	D I1 Capitale proprio	E I2 Occupazione attività	F I3 Agevolazione richiesta	G I1N Capitale proprio	H I2N Occupazione attività	I I3N Agevolazione richiesta	L Somma indicatori normalizz	M Sett serv	N Dim	O Ob	Q Cof	Q Esito finale	R Cod esc	S Risor	T Agevolaz concessa L. mil
129	25360		0,8505263	0,0000000	1,0000000	0,7008209	-0,4351545	-0,7296810	-0,46401460		P	5B	S		1		0,00
130	65825		0,7917975	0,0010292	1,0526316	0,5186830	-0,3709085	-0,6155161	-0,46774160		M	2	S	A		C	363,81
131	42605		0,7877161	0,0029590	1,0000000	0,5060252	-0,2504442	-0,7296810	-0,47410000		P	2	S	A		C	691,05
132	22406		0,8000000	0,0022222	1,0000000	0,5441217	-0,2964376	-0,7296810	-0,48199690		P	2	S	A		C	295,77
133	34725		0,3582400	0,0000000	1,6666667	-0,8259257	-0,4351545	0,7164069	-0,54467330		P	2	S	A		C	123,24
134	35710		0,3737235	0,0031519	1,5384615	-0,7779062	-0,2384027	0,4383129	-0,57799600		M	2	S	A		C	1270,80
135	8717		0,1890212	0,0155360	1,4285714	-1,3507306	0,5346528	0,1999468	-0,61613100		P	2	S	A		C	88,83
136	8686		0,8000000	0,0000000	1,0000000	0,5441217	-0,4351545	-0,7296810	-0,62071380		P	2	S	A		C	381,00
137	45047		0,7065861	0,0000000	1,1235955	0,2544136	-0,4351545	-0,4615861	-0,64232700		M	2	S	A		C	167,85
138	4708		0,5756229	0,0043018	1,1764706	-0,1517477	-0,1666222	-0,3468930	-0,66526290		M	2	S	A		C	584,43
139	31930		0,4007181	0,0036424	1,4285714	-0,6941867	-0,2077841	0,1999468	-0,70202400		P	5B	S		1		0,00
139	31931		0,4007181	0,0036424	1,4285714	-0,6941867	-0,2077841	0,1999468	-0,70202400		P	5B	S		1		0,00
141	41867		0,5150862	0,0121228	1,0000000	-0,3394925	0,3215899	-0,7296810	-0,74758360		P	5B	S		1		0,00
141	41870		0,5150862	0,0121228	1,0000000	-0,3394925	0,3215899	-0,7296810	-0,74758360		P	5B	S		1		0,00

Notes: This is a snapshot from a ranking of the first call published in the Official Journal. The first column (A) shows the position in the ranking, the second (B) the ID of the project, and the third (C) the company name, which we omit. Then there are 7 columns (D-L) that contain data on the raw sub-indexes, normalized sub-indexes and aggregated index presented in Section 2. The last columns indicate: whether the firm is active in the service sector (M), the size of the firm (N), the EU Objective area where the firm operates (O), the firm's eligibility to receive EU funding (P), the outcome of the application (Q), the reason for non-selection (R), the source of funding received (S), the amount of funding (T). Source: *Gazzetta Ufficiale, SG 288 of 09.12.1996, SO 215, p.34*.

Cell construction. A ranking is defined by six elements:

(1) *call* – in our final sample, we consider the following calls: $1, 2, 3, 4, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 19, 20, 31, 32, 33$

(2) *region* – Italy has 20 regions

(3) *firm size* – we create two different rankings along this dimension, one for small-medium enterprises and one for large firms

(4) *service sector* – there is one ranking for service providers and another one for firms that are not active in this sector

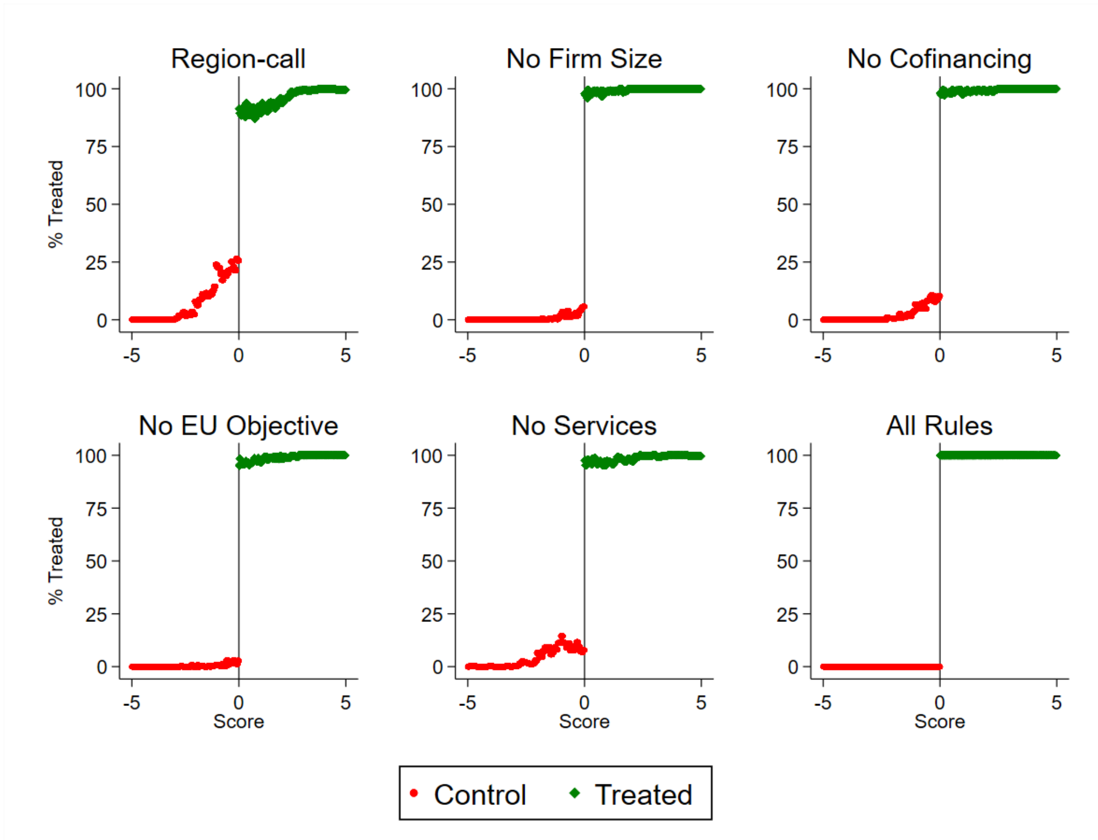
(5) *eligibility for EU funding* – there is one ranking for eligible firms and another for those not eligible

(6) *EU Objective* – there are four ranking types: one for Objective 1, one for Objective 2, one for Objective 5b, and one for the areas that are not part of the program and are considered ”Out of Objective”

We define a *cell* as the interaction of elements (1) to (6). For example, a cell in our specification could be: projects submitted during the 2nd call in the Tuscany region by small and medium-sized enterprises not active in the service sector, eligible for EU funds, and operating in an Objective 2 area.

Considering only elements (1) and (2), as in previous evaluations of L488/92, introduces significant measurement error in treatment assignment near the cutoff (top-left panel in Figure S5). When we consider the additional rules that determine assignment to treatment, we retrieve a sharp discontinuity at the pooled cutoff (lower right panel in Figure S5). The other panels in Figure S5 show that each and any of the four dimensions described above (in addition to call and region) is necessary to recover the sharp discontinuity in treatment assignment.

Figure S5: *Measurement error in treatment assignment due to errors in the construction of rankings*



S2 Data-driven selection of covariates

We implement a data-driven algorithm that searches for a vector of covariates satisfying the CIA condition in the spirit of Imbens & Rubin (2015). Formally, assume that we have a set of k covariates C, which is the union of two disjoint sets:

- a set $C_1 \subset C$ made up of $k_1 < k$ variables which must be included in the CIA regressions (4.5)-(4.6), but are not sufficient to make the running variable ignorable. These variables may be justified by some economic theory and, in principle, it could be that $C_1 = \varnothing$.
- a set $C_2 \subseteq C$ made up of $k_2 \leq k$ *candidate* variables which could be included in the CIA regressions (4.5)-(4.6) with the only purpose of making the running variable ignorable.

The algorithm searches for a set $\tilde{C} \subseteq C_2$ such that $\tilde{C} \cup C_1$ makes the running variable ignorable.

Algorithm

1. Run the following set of regressions for $j = 1, \ldots, k_2$,

$$Y = \sum_{\ell=1}^{p} \gamma_\ell^0 S^\ell + \mathbf{z}'\tau^0 + w_j \mu_j^0 + FE_c^0 + \nu^0, \quad \text{if} \quad -h \leq S < 0,$$
$$Y = \sum_{\ell=1}^{q} \gamma_\ell^1 S^\ell + \mathbf{z}'\tau^1 + w_j \mu_j^1 + FE_c^1 + \nu^1, \quad \text{if} \quad 0 \leq S \leq h, \tag{S2.1}$$

   where $\mathbf{z}$ is the vector of k_1 covariates that are always included; w_j is the j-th candidate covariate; and the other terms are defined as in equations 4.1 and (4.5)-(4.6), but allowing for different parameters on the two sides of the cutoff.

2. For each regression run the F-test for the null hypothesis that the CIA holds (separately) on each side of the cutoff

$$H_0^{(L)} : \gamma_1^0 = \cdots = \gamma_p^0 = 0 \quad \text{and} \quad H_0^{(R)} : \gamma_1^1 = \cdots = \gamma_q^1 = 0.$$

   and store the F-tests $F^{j,L}$ and $F^{j,R}$.

3. Select the two variables associated with the smallest F-statistics in the two sets $\mathcal{F}^L = \{F^{1,L}, F^{2,L}, \ldots, F^{k_2,L}\}$ and $\mathcal{F}^R = \{F^{1,R}, F^{2,R}, \ldots, F^{k_2,R}\}$. Notice that nothing prevents the variable with the smallest F-statistic on the left of the cutoff to differ from one on the right of the cutoff.

4. Add these two variables to the regressions in (S2.1) and repeat steps 1-3 for the other candidate covariates.

5. Repeat step 4 until one of the following stopping criteria is reached:

   - the null hypothesis that the running variable is not significantly different from 0 cannot be rejected at the α% level
   - all the covariates in $\tilde{C}$ have been included in the (S2.1)

The basic idea behind the algorithm is to implement a *greedy approach*. An approach is greedy when it is myopic, in the sense that the best variable is selected at each particular step, rather than looking ahead and picking a variable that will lead to a larger reduction in the loss function in some future step. This is done to avoid testing all the possible combinations of the elements of C_2.[28]

[28] This exercise would soon become intractable from a computational point of view as it involves estimating $\sum_{i=1}^{k_2} \binom{k_2}{i}$ different regressions. To quantify this issue, with 10 covariates, the number of different combinations to be tested for is 1023. This case is still tractable. However, adding just 10 other covariates drives the number of combinations over 1 million.