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Making Subsidies Work: Rules vs. Discretion*

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Abstract

We estimate the employment effects of a large program of public investment subsidies that ranked applications on a score reflecting both objective criteria and local politicians' preferences. 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 criteria 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 47%.

JEL Classification: H25, J08

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

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1 Introduction

Public subsidies to firms located in disadvantaged areas are an important component of public expenditure. 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 et al. 2017, Criscuolo et al. 2019, Siemer 2019), but market frictions may also force larger, more mature producers to forego investment opportunities (see e.g. Hsieh & Olken 2014, Akcigit et al. 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. However it is vulnerable to abuse, and 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. political discretion trade-off – for the effectiveness of public subsidies to private firms. Specifically, we investigate the impact of Law 488/92 (L488 henceforth), the largest program of investment subsidies ever implemented in Italy, and one of the largest in Europe (Giavazzi et al. 2012). Between 1996 and 2007, L488 financed 77,000 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.

Most policy evaluation studies face three main challenges. First, they must compare subsidized and non-subsidized firms that are similar in all dimensions except for the subsidy to achieve *internal validity*. Second, the effect of the subsidy may be very *heterogeneous* across firms. Third, and related to the first two, 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; on the other hand, average treatment effects across compliers 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 et al. 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 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 subsidies were allocated to projects through open calls. First, budgets were allocated to the 20 Italian regions, giving preference to economically under-developed regions in the South. Then, bids within each call-region were ranked according to a numerical score of project quality, and financed on a first-ranked, first-served basis until the funds were fully allocated. Importantly, the selection criteria weighted both objective indicators ("rules") and subjective priorities indicated by local politicians ("discretion").

This allocation mechanism is an ideal RD design. We find that firms just above the cutoff increased investment by almost 40 percent over the three-year funding period, compared to applicants that scored just below the cutoff. This transitory shock to investment generated, on average, a 10 percent increase in 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 show that if (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 control groups, then we can extrapolate treatment effects for any value of the running variable by matching treated and control groups on X. Both conditions (i) and (ii) are testable and they hold in our case conditional on a parsimonious set of firm characteristics. Using this approach, we characterize the heterogeneity of treatment effects and cost effectiveness of the policy across different types of applicants, and we compute total policy effects under actual and counterfactual allocation criteria.

The estimated cost per job observed at $t+6$ is just below €200,000 per job, or €55,000 per job-year. However, there is a stark divide between Northern and Southern regions – €70,000 and €270,000 per job, respectively. The cost of investment shows a similar gradient, as each Euro of subsidy generates three Euros of investment in the North, but only one Euro in the South. Turning to heterogeneity, firms ranking high on objective indicators and firms preferred by local politicians both generate larger treatment effects than other firms, on average, but the firms preferred by politicians do so at a higher cost-per-job. To isolate the role of political discretion, we estimate employment effects when employing two alternative, hypothetical criteria for distributing the subsidies: the first criterion completely ignores the subjective preferences of local politicians, thus eliminating them from the score used to rank applicants; the second criterion 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 funds are invariant to the criteria used to award the subsidies. While admittedly strong, this assumption is supported by evidence that applicants' observable characteristics 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 9% and 12%, compared to the actual policy. Relying exclusively on political discretion increases the cost of job creation and additional investment by 55% and 42%. 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 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 third. 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 et al. 2010, Bloom et al. 2019) or stimulating recovery after recessions (e.g., Wilson 2012, Chodorow-Reich et al. 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 et al. (2010) and Becker et al. (2013) evaluate the impact of the ERDF (which also contributed to the budget of L488) across European

[1] A related strand of literature estimates the local effects of enterprise zones, both in the United States (Bondonio & Greenbaum 2007, Ham et al. 2011, Busso et al. 2013) and in Europe (Gobillon et al. 2012, Mayer et al. 2017, Ehrlich & Seidel 2018).

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

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 et al. (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. 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 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 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 et al. (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 et al. 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 et al. 2018) and firms owned or run by individuals with a criminal record (Decarolis et al. 2020).[4] All of these papers study the effect of bureaucratic discretion, while we contribute novel evidence on politicians' discretion. Most importantly, the institutional features of L488 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, 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.

[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 Appendix B.

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

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] The last few decades also 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 rates across countries (Molloy et al. 2011).

Large territorial divides and low worker mobility provide a strong rationale for spatially-targeted subsidies (Kline & Moretti 2014, Bartik 2020). During the post-war 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 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 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 (although this was limited to 5% of the program total budget).

Funds were allocated through open calls for tenders, each one targeting a specific

[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.

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 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. Figure 1 shows a clear, negative relationship between L488 funds and regional GDP per capita.[7]

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

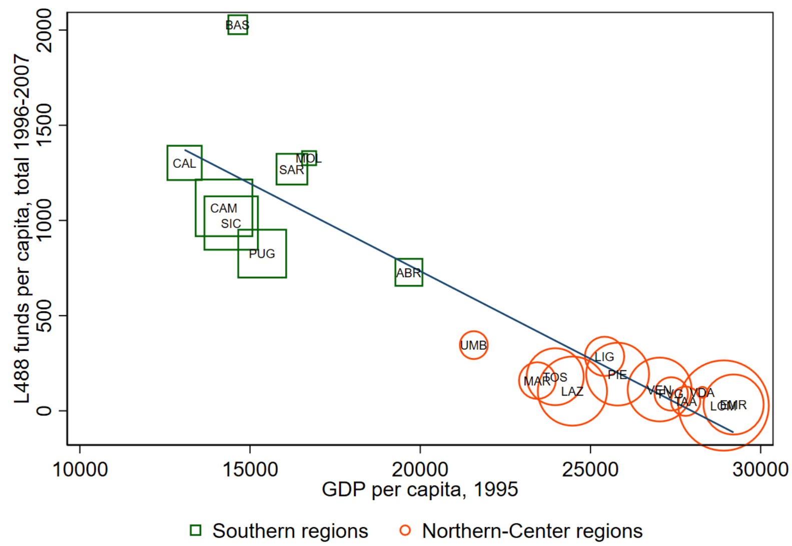
	All Italy	**North**	**Center**	**South**
Total funds	25.98	2.34	1.68	21.95
Allocation across economic sectors				
Industry	21.89	1.97	1.37	18.55
Tourism	2.68	0.21	0.19	2.28
Trade	0.73	0.06	0.06	0.61
Special	0.45	0.09	0.05	0.31
Craftwork	0.23	0.02	0.01	0.20
Source of funds				
National	19.77	1.95	1.52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

Notes: This table shows the allocation of L488 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 on quantitative indicators of project quality, combined with rules regarding minimum quotas of L488 funds reserved for specific categories of applicants (e.g., small-medium firms) or eligibility for co-financing with EU funds. Applicants had to provide the information required to construct

[7] Appendix Figures A1 and A2 provide additional descriptive evidence on the evolution and composition of funding over time and across geographical areas.

Figure 1: *L488 funds and GDP per capita across regions*



Notes: This figure plots the total amount of L488 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.

the quantitative indicators during the application process. In the first two calls for projects, there were three such indicators:

C1) the ratio of the companies' own investment to the amount requested ("*skin in the game*")

C2) the number of jobs created by the project ("*job creation*")

C3) the proportion of funds requested in relation to an ad-hoc benchmark set by the EU Commission ("*no waste*")

Criteria 1 and 3 captured the entrepreneurial stake in a project, privileging projects with a higher level of involvement, while criterion 2 follows naturally from the main goal of L488, namely stimulating employment. Starting from the third call, two additional criteria were introduced:

C4) a firm-specific score attributed by the regional government ("*political discretion*")

C5) compliance with the requirements of an environmental management system, e.g. ISO 14001 or EMAS ("*environmental responsibility*")

The score for criterion 4 was completely at the discretion of local politicians, who only needed to base their choice on loosely-defined "regional priorities". Clearly, including this criterion greatly increased the scope for political discretion. In Section 6, we examine the implications of political discretion for the effectiveness of investment subsidies.[8]

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

$$S_i = \sum_j (I_{ic}^j - \mu_c^j), \tag{2.1}$$

where I_{ic}^j is the value achieved by project i in call-region c on the j-th indicator, and μ_c^j is the mean of the same indicator across all projects presented in the same call-region.[9]

To determine the allocation of projects, the ranking of applicants by the score S within each call-region was combined with additional rules, mentioned above, prioritizing specific categories of applicants and projects. There were three rules:

1. at least 50% of the budget within each region was reserved for small-medium

[8] In Appendix C, we investigate the relationship between the political discretion index and different measures of political proximity between the regional government and the municipality in which the firm is located (e.g., partisan alignment between the two). Appendix Table C6 shows that none of these variables appears to be significantly related to the political discretion index. However, this null result may be because our measures of political proximity between regional and municipal governments do not adequately capture connections between applicant firms and the local government. Unfortunately, precise measures of political connections at the firm-level (as in, e.g., Cingano & Pinotti 2013), are not available for our sample of firms.

[9] In some calls, the fifth sub-index (C5) was not added to all others to form the final score of a project. Rather, if the project was compliant with the environmental certifications, C5 increased by 5% all other sub-indexes. In such cases the correct formula for the score is

$$S_i = \sum_{j=1}^{4} (I_{ic}^j \times I_{ic}^5 - \mu_c^j)$$

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

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 meeting certain requirements – in terms, e.g., of location, type, and duration of investments – were eligible for additional co-funding from EU structural funds, so they could be financed ahead of higher-ranked projects eligible only for national funding.

These rules, which are explained in more detail in Appendix B, 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 the "*cells*" of firms competing for L488 funds within the same call, region, and (possibly) special category of applicants.[10]

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.

3 Data

Our analysis is based on a unique dataset combining administrative data on applications for L488 subsidies, registry data on applicant firms and their employees,

[10]Previous evaluations of L488 constructed the RD design only by call and region. In Appendix B, 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.

and a proprietary database of balance sheet data. The Italian Ministry of Economic Development provided detailed information on all applications from 26 calls for L488 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), submitted by 49,082 firms; see Appendix Table A1.[11] 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 sub-components; the amount awarded to the applicant firm, if any. 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 were eventually eligible for funding.

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 the employment levels of each firm at monthly frequencies as well as business foundation and cessation dates. Using these data, we can track very precisely, at monthly frequencies, job numbers at applicant firms both before and after applying for (and possibly obtaining) the subsidy, as well as firms' survival rates over long periods of time.

Unfortunately, the fiscal identifier of sole proprietorships is typically anonymized in the INPS registries, so we lose about 20,000 applications by micro-enterprises. When estimating the dynamic treatment effects of the subsidy, we drop another 10,000 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 5000 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 40,344 projects submitted by 27,074 different firms.

[11] 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.

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 80% of firms in the matched L488-INPS data described above (21,459 companies, corresponding to 33,511 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.[12]

In Figure 2, we explore the allocation of subsidies across geographical areas, particularly whether budgets assigned to each region effectively target provinces within each region that are most in need of public subsidies.[13] Panel A shows that both the (log of) subsidies requested and the actual disbursements to winning projects are higher in provinces where firms face tighter credit constraints, as measured by the spread between loan and credit rates in local credit markets (Guiso et al. 2013). On the other hand, there is no relationship with local unemployment rates – if anything, the regressions in Panel B of Figure 2 are slightly negative, though not significantly different from zero.

4 Empirical Strategy

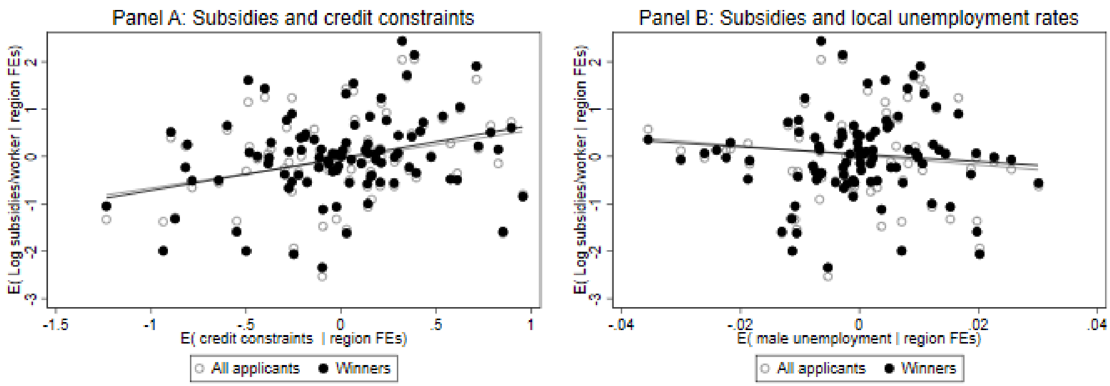
Let Y_i^1 and Y_i^0 be the potential outcomes of firm i competing in cell c (as defined by the call-region-category of applicant) when obtaining and not obtaining the subsidy, and D_i be a "treatment" dummy equal to 1 for firms receiving the subsidy. In addition, let $\tilde{S}_i$ be the score received by firm i and $\bar{S}_c$ be the cutoff score required for obtaining the subsidy in cell c. Then, $S_i = (\tilde{S}_i - \bar{S}_c)$ is the standardized score for each firm, with $S_i = 0$ at the cutoff, $D_i = 1$ whenever $S_i \geq 0$.

In principle, there could be imperfect compliance if some applicants scoring above the cutoff did not receive the subsidy or, conversely, some applicants below the cutoff did receive the subsidy. In practice, no applicant scoring below the cutoff

[12] Appendix Figure A3 shows evidence for requested and awarded subsidy, score obtained by the project, and the score sub-component for the (planned) number of newly created jobs.

[13] Provinces are the administrative units immediately below regions – they correspond to the NUTS-3 level. During our sample period, Italy comprised between 95 and 103 provinces.

Figure 2: *credit constraints, unemployment rates, and allocation of subsidies across provinces*



Notes: These graphs plot the relationship between log of subsidies per worker demanded by all applicants (grey plots) and by winning applicants (black plots) against credit constraints (Panel A) and male unemployment rates (Panel B) across provinces, controlling for region fixed effects. OLS regressions weighted by province populations are shown in the graphs. Credit constraints are measured by the spread between loan and credit rates in local credit markets, from Guiso et al. (2013).

received the subsidy and virtually all applicants scoring above the cutoff received at least one (or more) instalments. Put differently, the intention-to-treat and treatment effects coincide, so we comment on the latter throughout the paper.

Since the score completely determines treatment assignment, we can isolate the average treatment effect of the subsidy at the cutoff, τ, by comparing firms just above and just below the normalized cutoff. Formally,

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

where $Y = DY^1 + (1 - D)Y^0$ is the realized outcome. In practice, 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 k-th order polynomial in the score S and its interaction with D:

$$Y = \tau D + \sum_k \gamma_k S^k + \sum_k \delta_k D \cdot S^k + FE_c + \varepsilon, \tag{4.1}$$

where FE_c is a fixed effect for cell c and ε is a residual term summarizing the effect of other factors. Following Fort et al. (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.[14] We also experiment with triangular kernels attaching greater weight to observations closer to the cutoff.

Under the testable 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 high- and low-scoring firms may differ along some unobservable dimension (e.g., managerial ability). Angrist & Rokkanen (2015) address this issue by leveraging on the fact 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 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, let the running variable S be a function $S = g(X, u)$ of some (observable) variables X and some (potentially 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

[14] Results are qualitatively unaltered when we progressively lower the bandwidth from $[-5, 5]$ to $[-1, 1]$. As recommended by Gelman & Imbens (2019), we avoid specifying polynomials in the running variable of order higher than 2.

potential outcomes independent from S and, thus, from u. 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 (4.2), Angrist & Rokkanen (2015) require common support between treated and controls on X,

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

Both the CIA and common support are partially testable, so the RD design provides a test for the (usually untestable) assumption that conditioning on X removes all confounding differences between treated and controls. 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):

$$\mathbb{E}[Y \mid S, X, D = 1] = \sum_{k=1}^{p} \gamma_k^1 S^k + X'\beta^1 \tag{4.5}$$

and

$$\mathbb{E}[Y \mid S, X, D = 0] = \sum_{k=1}^{p} \gamma_k^0 S^k + X'\beta^0. \tag{4.6}$$

Failure to reject the restrictions $\gamma_k^1 = \gamma_k^0 = 0$, $\forall k = 1, \ldots, p$, 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) by OLS 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.[15]

In the same way, we can characterize the heterogeneity in treatment effects along the distribution of sub-components of the score. In particular, let S_r and S_d denote the scores obtained on objective "rules" and political "discretion": provided that the CIA holds,

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

we can estimate conditional average treatment effects as

$$\mathbb{E}[Y^1 - Y^0 \mid S_r = s_r', S_d = s_d'] = (\beta^1 - \beta^0)' \,\mathbb{E}[X \mid S_r = s_r', S_d = s_d']. \tag{4.9}$$

Equation (4.9) will allow us to assess the contribution of objective rules and political discretion, respectively, to the effectiveness of public subsidies. Importantly, such analysis requires that a firm's decision to apply for funds is not affected by the change of criteria. In the next section, we provide indirect evidence consistent with such an assumption, based on the comparison of applicant firms before and after the major change in allocation criteria that occurred starting with the 3^{rd} call for projects.

5 Results at the RDD cutoff

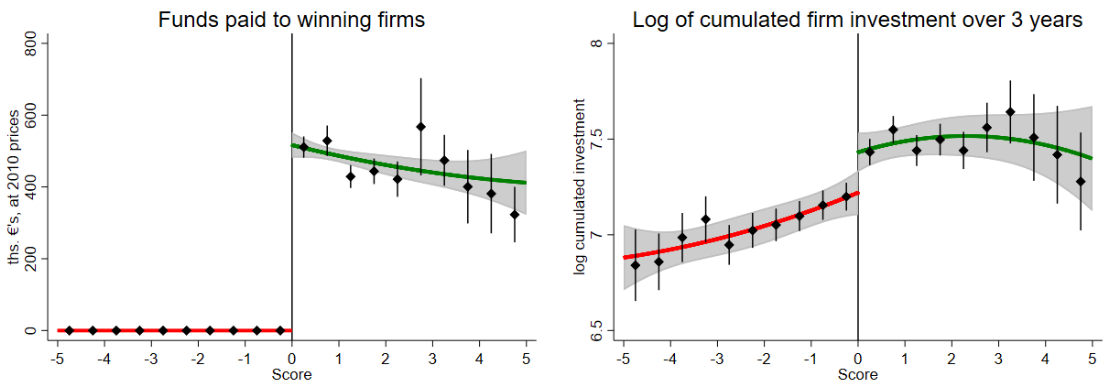
Figure 3 plots the relationship between the score obtained by applicant firms and the subsidy they received (left graph) and the log of total, cumulated investment over the three following years (right graph). We show averages and confidence intervals for equally-spaced bins of size 0.5, together with 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.[16]

Figure 4 shows that applicants ranking just above and below the cutoff are on

[15] In a companion paper, Palomba (2022) introduces a new Stata package, `getaway.ado`, which implements different methods for extrapolating RD estimates away from the cut-off together with several tests and graphical tools. The package is available at

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 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, as defined by groups of firms competing for L488 funds within the same call, region, and (possibly) special category of applicants.

average equal on a wide range of other characteristics measured one year before the call; 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, Figure 6 shows no evidence of discontinuity in the density of applications.[17]

Taken together, Figures 4, 5, and 6 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.

5.1 Baseline results

The stated objective of Law 488 was to increase employment in disadvantaged areas. Figure 7 shows the effect of the subsidy on the log-change of firm employment. In the year before the L488 call, firm employment is balanced between treated and control firms near the cutoff (first graph), but the subsidy progressively opens a gap between the two groups of firms during the following years. The gap is already noticeable one year after obtaining the subsidy (second graph); it increases at the end of the subsidy period (third graph) and persists in subsequent years (last graph).

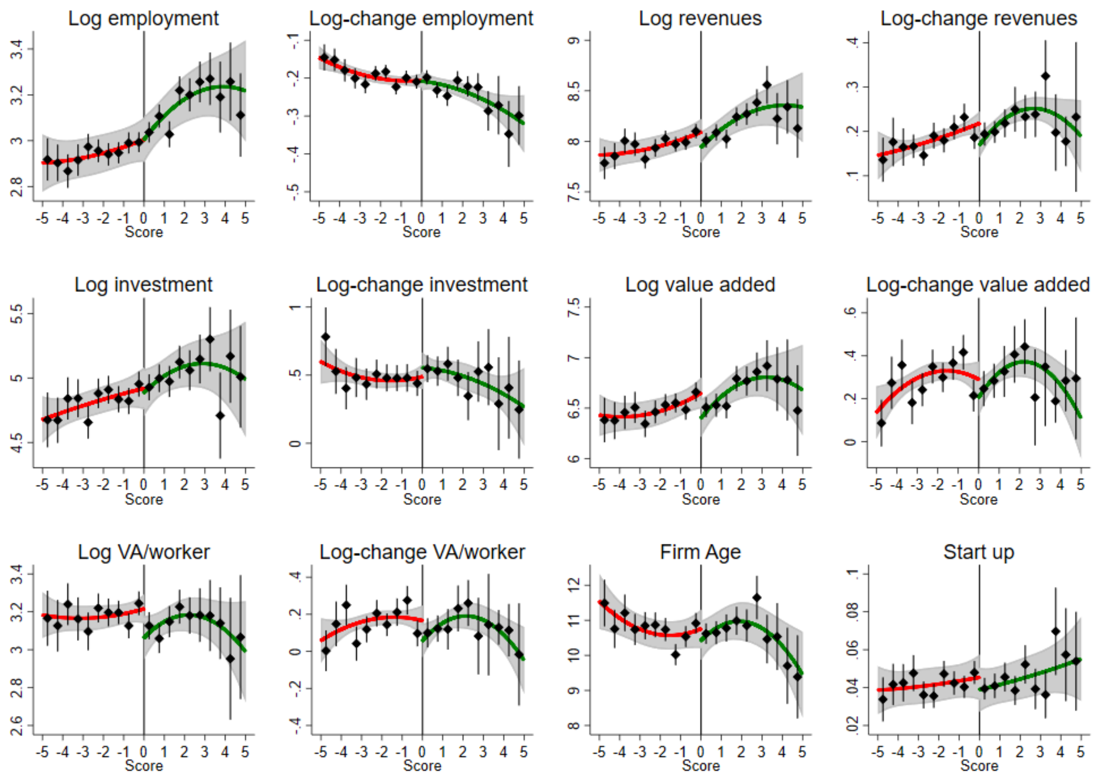
Table 2 presents the evidence in Figures 3 and 7, showing estimates of equation (4.1) under a variety of specifications. Specifically, we experiment with linear and quadratic polynomials in the running variable, uniform and triangular kernels (the latter attach a greater weight to observations near the 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

https://github.com/filippopalomba/getaway-package.

[16]In the right graph of Figure 3, 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. This depends on the fact that sub-component 3 of the score (i.e., the "no waste criterion", see Section 2) penalizes applicants requesting higher subsidies. Therefore, other things equal high-scoring firms obtained lower subsidies, as shown in the left graph, and they generate as a consequence lower additional investment.

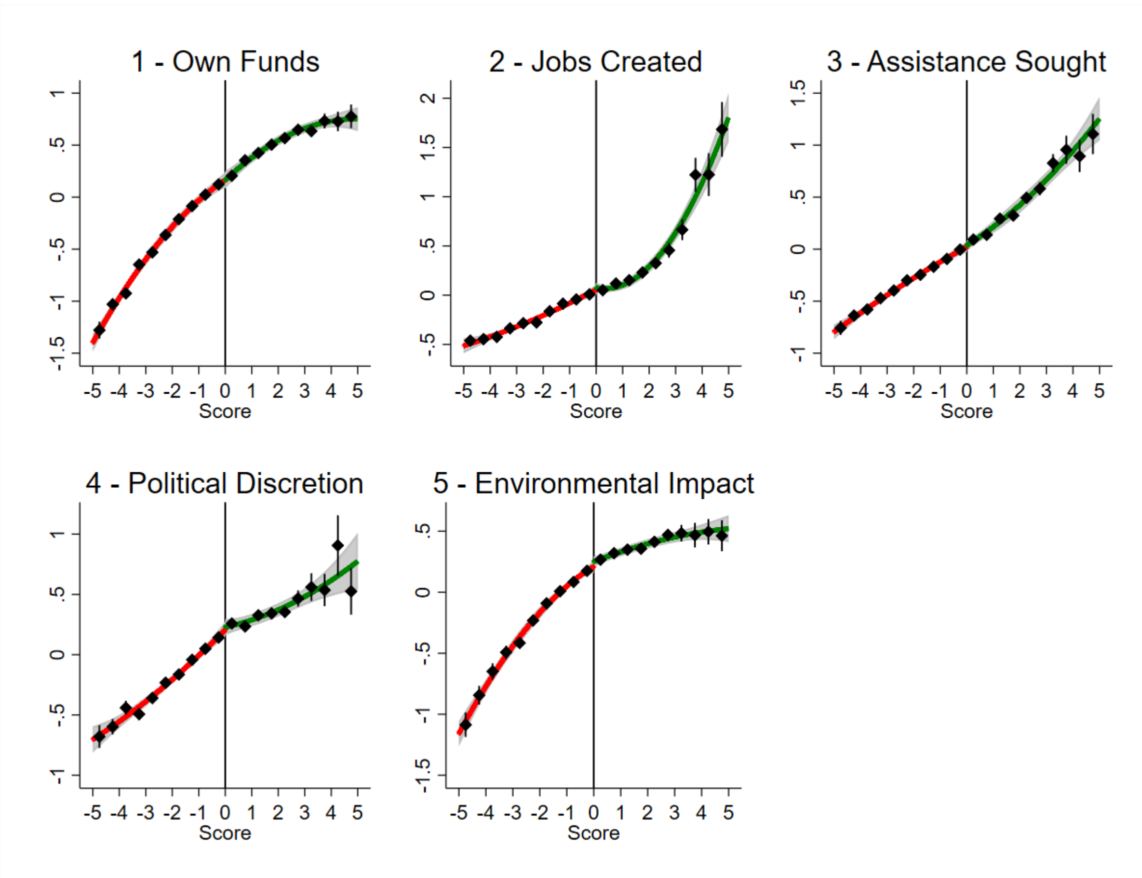
[17]The formal test by McCrary (2008), as implemented by Cattaneo et al. (2020), does not reject the null hypothesis of no discontinuity at the cutoff with a p-value of 0.2.

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



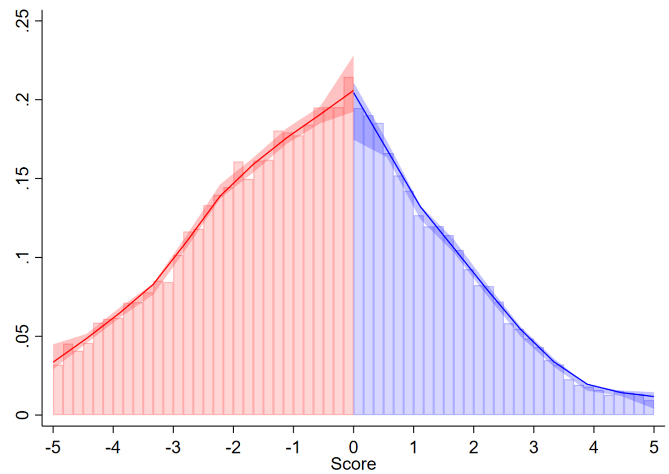
Notes: These graphs show the relationship between the standardized score obtained by firm applications for L488 funds, on the horizontal axis, and several firm characteristics measured one year before the call – log and yearly log-change in revenues, value added, value added per worker, investment, firm 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. Cells comprise defined by groups of firms competing for L488 funds within the same call, region, and (possibly) special category of applicants. 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 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. Cells comprise defined by groups of firms competing for L488 funds within the same call, region, and (possibly) special category of applicants. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

Figure 6: *Density of applicant scores*



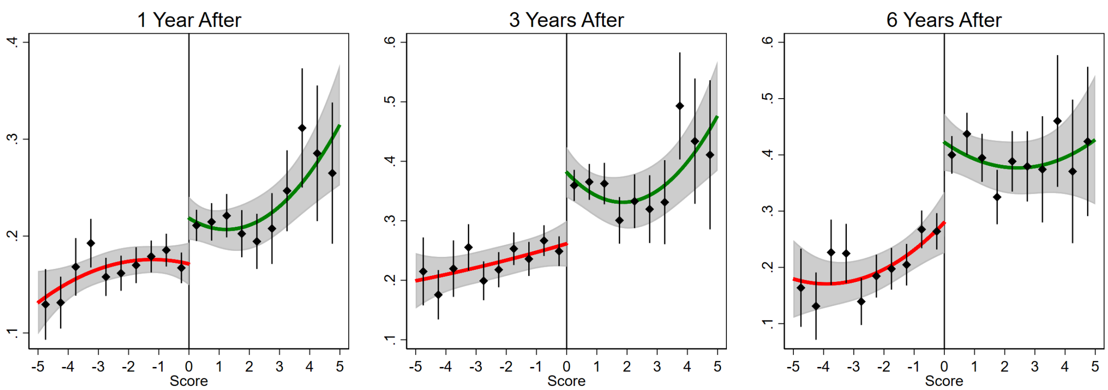
Notes: The histogram shows the distribution of applicant scores. Local polynomial density estimates (solid lines) and robust bias-corrected 95% confidence intervals (shaded areas), computed according to Cattaneo et al. (2020), are also reported in the figure.

in this paper we focus on the simplest linear specification with cell fixed effects. According to this specification, presented in column (2) of Table 2, 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.

Figure 8 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 permanent 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 8 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 86 percent. To the extent that excess mortal-

Figure 7: *The effect of the L488 subsidy on firm employment*



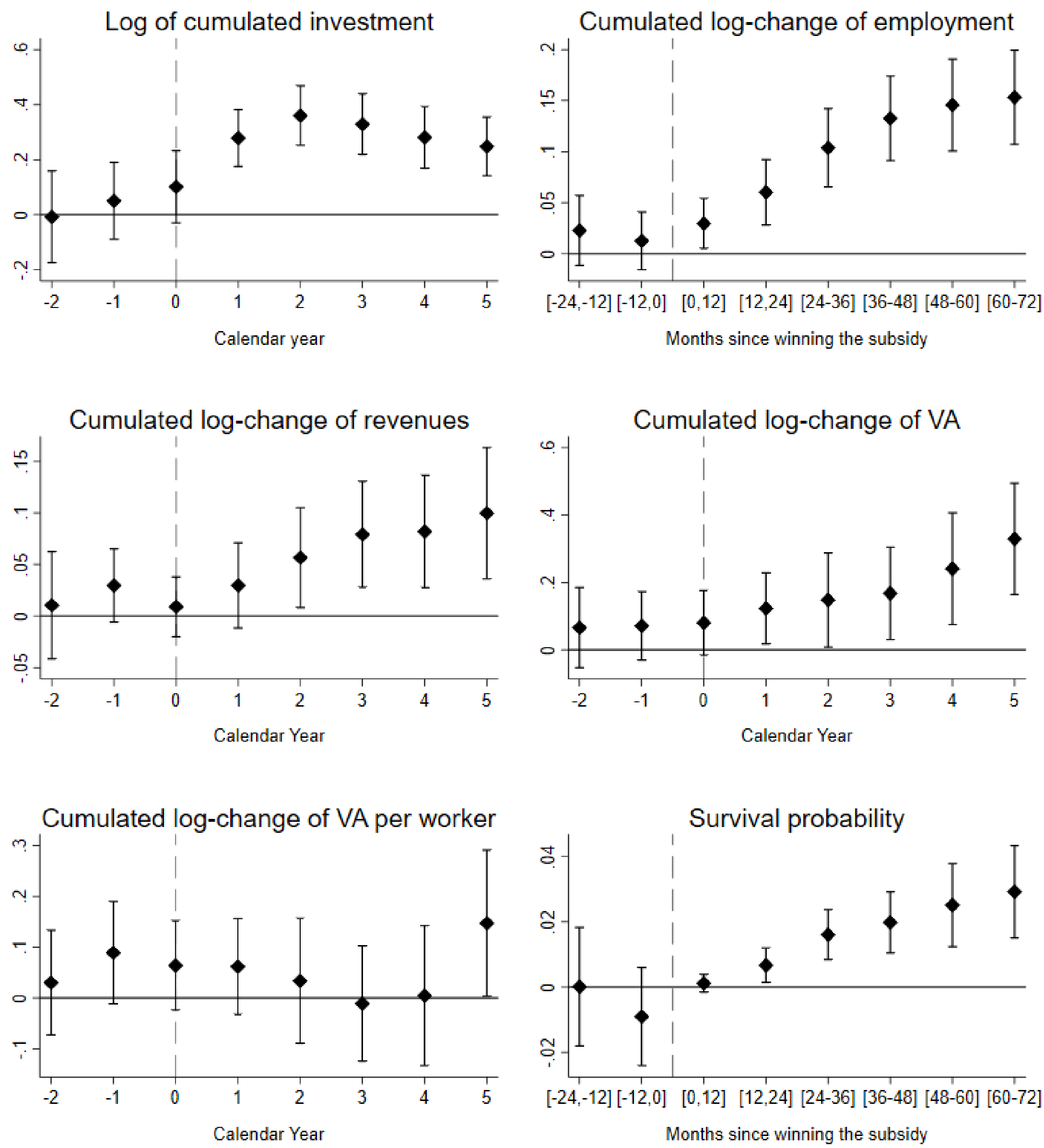
Notes: These graphs show the relationship between the standardized score obtained in firm applications for L488 funds, on the horizontal axis, and the (log) employment 1, 3, and 6 years after the award of subsidies. Bins represent averages over equally-spaced intervals, and confidence intervals (at the 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. Cells comprise defined by groups of firms competing for L488 funds within the same call, region, and (possibly) special category of applicants. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

Table 2: *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 subsidies on firm investment and employment growth, as estimated from the parametric RD regression in equation (4.1) across applicant firms in all L488 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 8: *Dynamic effects of the L488 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 2, namely a linear regression including cell fixed effects and clustering heteroskedasticity-robust standard errors at the same level.

ity 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.

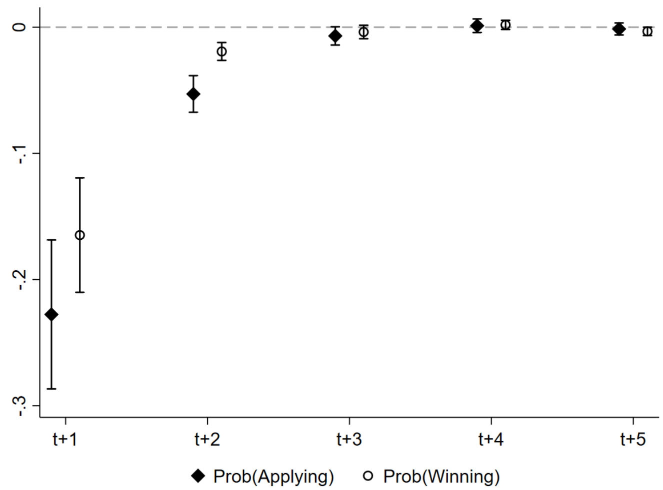
5.2 Additional results

There are two issues that could affect the interpretation of our results. One, applicants in a given call may re-apply (and obtain funds) in subsequent calls. Two, the effects on funded firms may spill over to other, non-funded firms.

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 $t + \Delta$. Therefore, the dynamic treatment effects on outcomes between t and $t + \Delta$ 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 9 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 2 and Figure 8 under-estimate the direct, dynamic treatment effects of the subsidy were there no indirect effects through the (lower) probability of re-applying and obtaining funds in subsequent calls. In Appendix D.1 we present a method that allows us to isolate, under testable restrictions, the direct effect from the total effects. As expected, the direct effect is larger than the total effect, but the two remain qualitatively similar.

Figure 9: *Direct and indirect effects for re-applicants*



Notes: The graph shows the estimated effect of obtaining the L488 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.

Turning to spillover effects, 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, our estimated coefficients would overstate the effects of the policy, as part of the employment increase estimated for subsidized firms would be a re-allocation of workers from control firms, as opposed to new local jobs.

To address this possibility, we compare the dynamics of employment between non-subsidized firms within the same local labor market (LLM) of a subsidized firm and non-subsidized firms in other LLMs; spillover effects should affect more (or only) employment in the former group than in the latter. However, our difference-in-differences estimates (appropriately accounting for the staggered nature of the research design, as in de Chaisemartin & D'Haultfœuille 2020) show no evidence of significant spillover effects. These results, presented in detail in Appendix D.2 imply that the increase in 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 subsidies generate few displacement effects across firms, if at all.

6 Results away from the RDD cutoff

The results in the previous section show that L488 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 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.

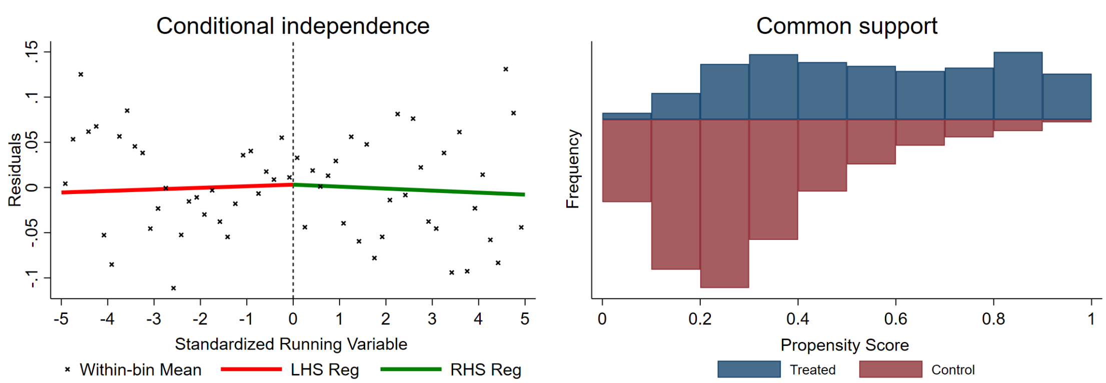
As discussed in Section 4, Angrist & Rokkanen (2015) invoke mean independence of the outcome on the running variable and common support between treated and control groups, conditional on a set of covariates X. We experiment with alternative covariates, and we achieve conditional independence and common support for a plausible set of predictors of growth potential: 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.[18] Importantly, all results are robust when selecting an alternative set of covariates based on a

[18] 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.

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 Appendix E.

In Figure 10 we show graphs of the results of the tests for conditional independence (equation 4.2) and common support (equation 4.3). Let $X^{\star}$ be the set of covariates satisfying both conditions. Starting with the former condition, the left graph is a plot of the residuals from a regression of the 6-year employment growth on $X^{\star}$ (on the vertical axis) against the applicant's score (on the horizontal axis), separately on each side of the cutoff, together with the conditional regression line. The relationship is flat, in contrast to the positive unconditional relationship in Figure 7.[19] In addition, the right graph in Figure 10 displays considerable common support between treated and controls in the distribution of the propensity score $\mathbb{P}(D = 1 \mid X^{\star})$.

Figure 10: *Testing the conditional independence and common support*



Notes: The left graph shows the test of conditional independence in equation (4.2) for the vector of covariates $X^{\star}$, described at the beginning of Section 6. It is a plot of the residuals of a regression of the outcome Y (i.e., firm employment growth in the 6 years after applying for L488 funds) on $X^{\star}$ against the running variable S (i.e., the application score). Conditional means within 60 equally-spaced bins (black crosses) along with conditional regression functions (solid lines) are reported. The right graph shows the density of treated and control firms by decile of the estimated propensity score of receiving the subsidy, conditional on $X^{\star}$.

[19] Appendix Table A3 confirms that the estimate slopes in equation 4.2, conditional on $X^{\star}$, are precisely estimated zeros on both sides of the cutoff.

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

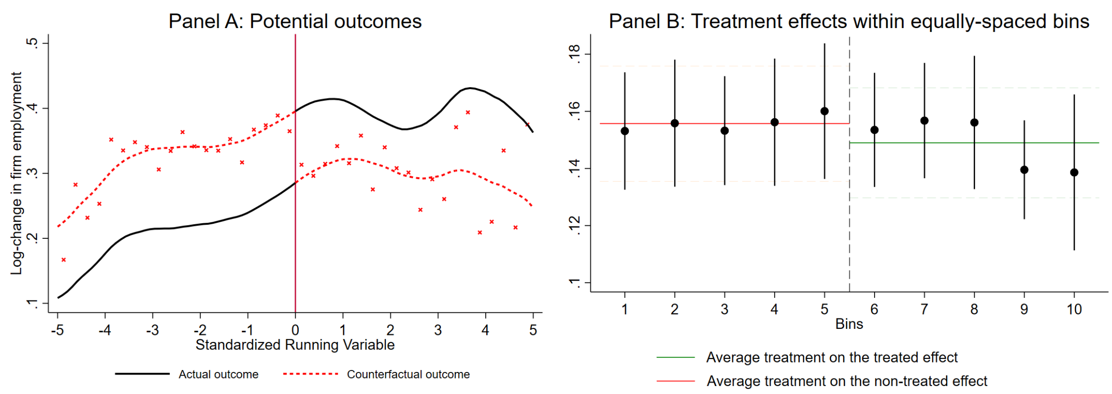
Under conditional independence and common support, we can estimate treatment effects across firms away from the cutoff by comparing the outcomes of treated and control firms keeping constant the covariates in $X^{\star}$. Specifically, we use the estimated parameters in equation (4.5) to predict the 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 11 plots fitted actual and extrapolated counterfactual outcomes along the distribution of the application score. 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. We show confirmation of this in Panel B of Figure 11. However, this finding mainly reflects composition effects, as (i) the score used to rank applications is standardized within each cell, and (ii) project quality likely varies mostly between cells (notably, between different regions) rather than within cells. Indeed, Appendix Figure A4 shows that the treatment effect clearly increases with the raw, non-standardized score. Therefore, the application score is generally informative about project quality.

6.2 Cost effectiveness and comparison with previous work

In Table 3 we show the cost of creating new jobs and additional investment across all firms. In the first row of the table we see that the cost per new additional job 6 years after receiving the subsidy is just below €180,000. This estimate remains virtually identical when using the baseline set of conditioning covariates and the alternative set of covariates selected by the data-driven algorithm (columns 1 and 2). Since each job may last several years, we also compute the cost per job-year through year 6, which stands at €54,000 (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.

These figures are much higher than previous estimates by Cerqua & Pellegrini

Figure 11: *Potential outcomes and treatment effects in $t+6$, along the distribution of the applicant score*



Notes: Panel A is a plot of average potential outcomes six years after obtaining or not obtaining the subsidy, along the distribution of the applicant score. Counterfactual outcomes are estimated by equations (4.5) and (4.6), and they are averaged within equally-spaced bins (red crosses). Solid and dashed lines are obtained fitting kernel-weighted local polynomial smoothers on such averages. 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). 90% confidence intervals are estimated using 2000 iterations of a non-parametric cluster bootstrap.

(2014), 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. While they experience larger effects of the subsidy in terms of employment growth, small firms generate new jobs (computed multiplying the percent treatment effect by the initial number of employees) at a much higher cost. To get a sense of the cost-size gradient, the cost of a subsidized job varies from over €325,000 among small (10-) firms, to around €75,000 in the case of large (250+) firms.

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 $71,000 in our case (column 3 of Table 3, after conversion to US$).

Table 3: *Cost of new jobs and investment generated by L488 subsidies*

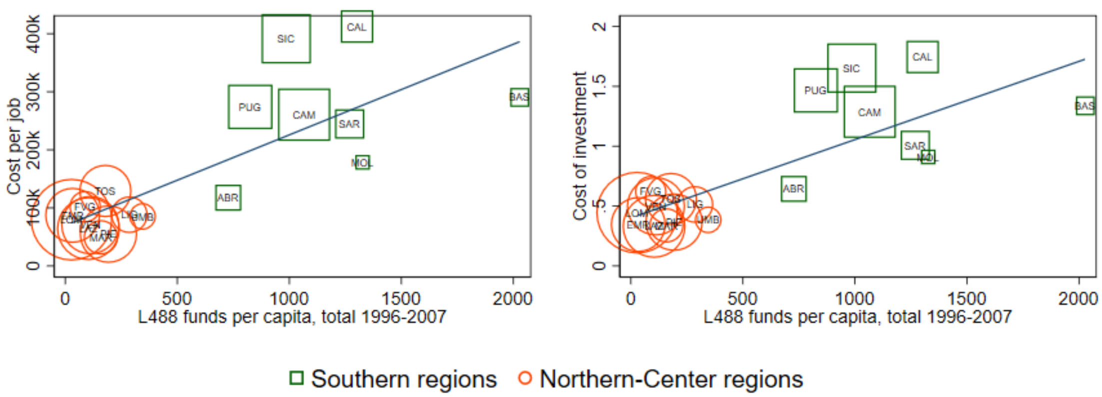
	(1)	(2)	(3)	(4)	(5)	(6)
Cost measure:	cost per new job (thousands of €'s)		cost per worker-year (thousands of €'s)		cost of new investment (cost per € of investment)	
$X^\star$:	manual	data-driven	manual	data-driven	manual	data-driven
all regions	178	172	54	58	0.812	0.745
south	241	215	77	76	1.052	0.979
north-center	68	78	19	25	0.351	0.314

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 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 Appendix E.

Overall, the cost-effectiveness of L488 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 3 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 subsidies was much lower in Southern regions, which received the largest share of funds; see also Figure 12. This 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.

Figure 12: *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 per capita across Italian regions. The size of markers is proportional to regional population.

6.3 Rules vs. discretion

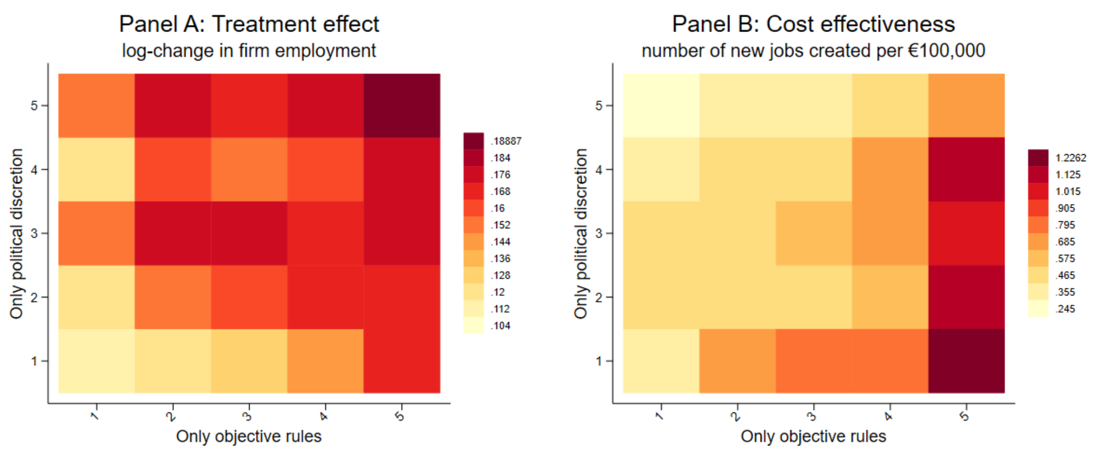
As explained in Section 2, the application score initially included only *objective rules* – namely, own resources invested by the applicant, number of newly created jobs, and proportion of funds requested in relation to a benchmark by type of project – but a fourth criterion reflecting only the *political discretion* of the regional government was added starting from the third call for projects.[20] Using the result in equation (4.9), we characterize the heterogeneity in the effect of the subsidy along these two sub-components of the application score.[21]

[20] The fifth criterion, relating to environmental responsibility, cannot be clearly defined as either discretionary or non-discretionary.

[21] Appendix Table (A4) shows that the required CIA condition (4.8) holds.

Panel A of Figure 13 plots treatment effects by quintiles of the sub-score obtained along either dimension (i.e., rules and discretion). Both the firms preferred by regional politicians and those scoring high on objective criteria generate larger employment growth compared to other applicants. At the same time, Panel B shows that political discretion generates new jobs at a higher cost than do objective rules. 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.

Figure 13: *Treatment effect and average cost per new job, 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 score sub-components relating to political discretion and objective rules. In Panel A, the treatment effect for each bin (Discretion = d, Rules = r) is estimated as $\mathbb{E}[Y^1 - Y^0 \mid \text{Discretion} = d, \text{Rules} = r] = (\beta_1 - \beta_0) \cdot \mathbb{E}[X^\star \mid \text{Discretion} = d, \text{Rules} = r]$. 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.

To further understand the impact 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 assumption that treatment effects of the subsidy are policy invariant. Although this is arguably a restrictive assumption, we can provide two pieces of evidence to support it. First, we can compare the characteristics of applicants in the first two calls for projects (1996-97), before criteria for political discretion and environmental responsibility were introduced, and in the following two calls for projects (1998). Appendix Table A5 shows that the two groups are on average very similar in terms of observable characteristics, the standardized difference remaining below the critical threshold of 2 for all variables. Second, the results in the previous section (5.2) seem to exclude the existence of strong spillovers to non-funded firms, which would be another source of general equilibrium effects potentially driving a difference in the effects under alternative selection rules.

We consider three main counterfactual allocation rules (keeping constant the budget allocation across regions): 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 investment under these counterfactual policies are presented in Table 4, along with the costs under the actual policy (column 1).[22] Column (2), Panel A, 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) and a similar gradient along the north-south dimension. The following column (3) shows the effect of an opposite policy, namely relying exclusively on political discretion for

[22]The costs reported in column (1) of Table 4 are slightly different from those in Table 3 because the latter 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).

allocating subsidies. Such a policy would greatly increase the cost of new jobs and investment by 47 and 30 percent, respectively.

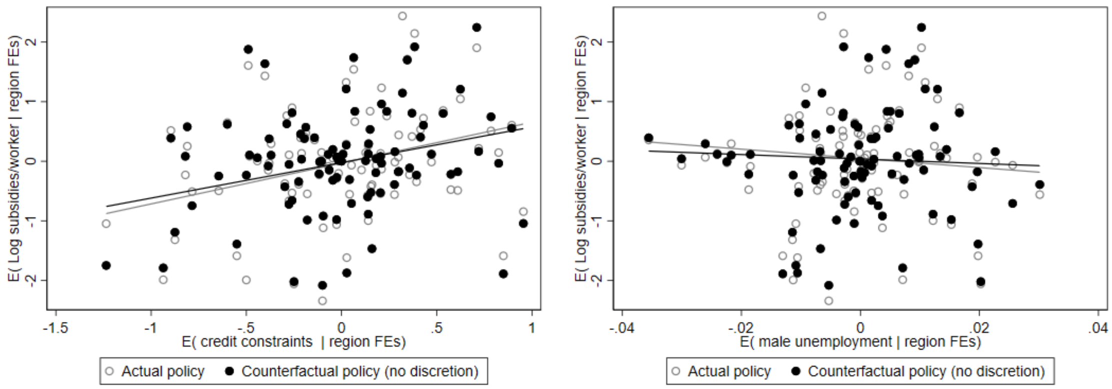
Table 4: *Cost of new jobs and investment under different counterfactual policies*

	(1)	(2)		(3)		(4)	
	Actual	**Counterfactual policies**					
	policy	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%	262	47%	96	-46%
south	225	198	-12%	307	41%	114	-50%
north-center	83	76	-9%	118	36%	51	-39%
Panel B: Cost per 1€ of investment							
all regions	0.76	0.67	-13%	0.99	30%	0.33	-57%
south	0.94	0.82	-14%	1.13	21%	0.38	-49%
north-center	0.39	0.33	-12%	0.47	21%	0.20	-59%

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 the sub-component of the application score that was left to politicians' discretion (column 2); eliminating each of the other subcomponents (columns 3-5); rank firms only based on political discretion (column 6); and giving priority to applicants with lower cost of generating jobs (column 7). 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.

Overall, the evidence in columns (1)-(3) of Table 4 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. In principle, we cannot exclude that a higher cost per job under political discretion reflects efforts by local politicians to target areas facing tighter credit constraints or higher unemployment. However, Figure 14 shows that allowing for political discretion does *not* shift the counterfactual allocation of subsidies towards more disadvantaged provinces within each region, compared to the actual policy.

Figure 14: *Credit constraints, unemployment and allocation of subsidies across provinces within each region under actual and counterfactual policies*



Notes: These graphs plot the relationship between the log of subsidies per worker against credit constraints (left graph) and male unemployment rate (right graph) across provinces, controlling for region fixed effects, under the actual policy (grey plots) and under a counterfactual policy eliminating political discretion (black plots). OLS regressions weighted by province populations are shown in the graphs. Credit constraints are measured by the spread between loan and credit rates in local credit markets, from Guiso et al. (2013).

Finally, we consider a counterfactual policy assigning priority to firms generating new jobs at the lowest cost. Column (4) of Table 4 shows that the cost of creating new jobs and investment would decrease by 46 and 57 percent. Even in this case, the reduction in the cost of new jobs would be larger in southern than in northern regions (50 and 39 percent).

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 47 percent.

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Online Appendix

Making Subsidies Work: Rules vs. Discretion

Federico Cingano, Filippo Palomba, Paolo Pinotti, and Enrico Rettore

A Additional figures and tables

Figure A1: *Total L488 funds by year and geographical area*

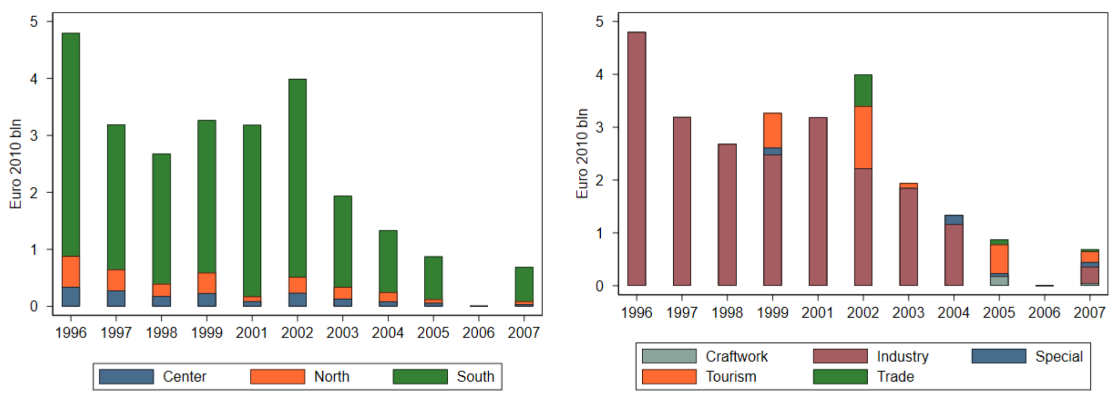


Figure A2: *Total L488 funds by region, source, and economic sector*

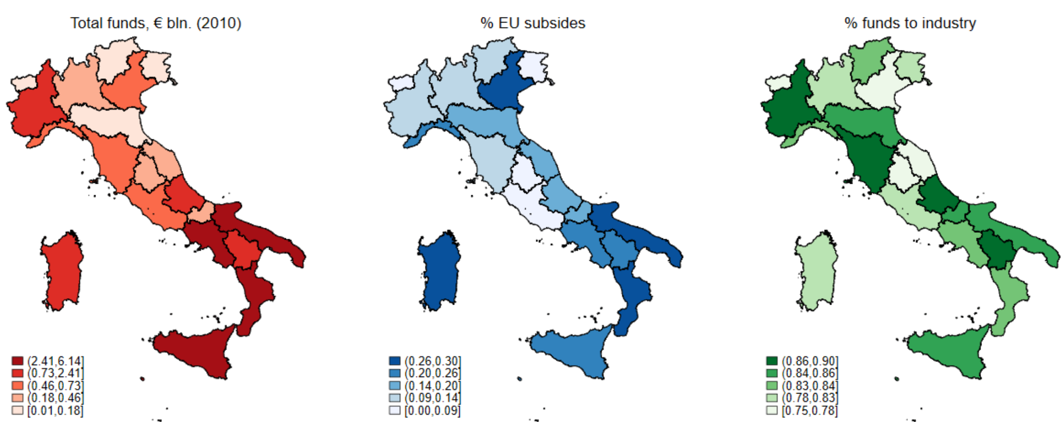
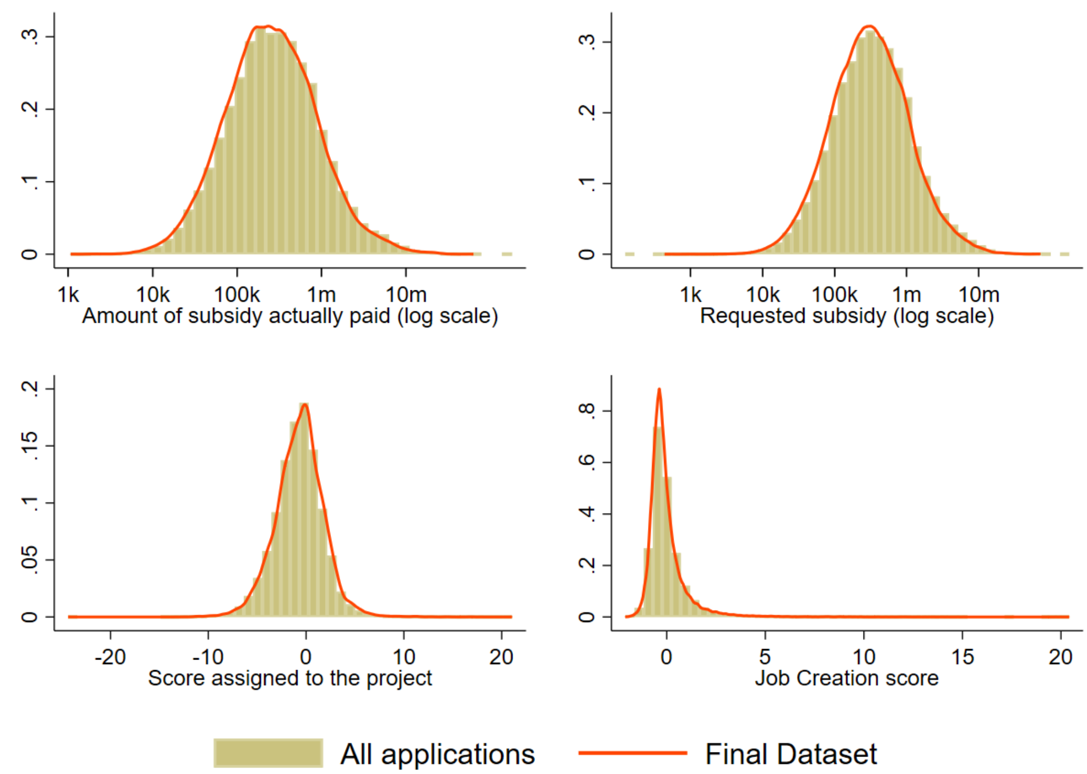


Figure A3: *Distribution of selected variables across all applications and within the sub-sample of matched applications*



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.

Table A1: *List of calls in the L488 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 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, 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. 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 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: *Conditional independence tests*

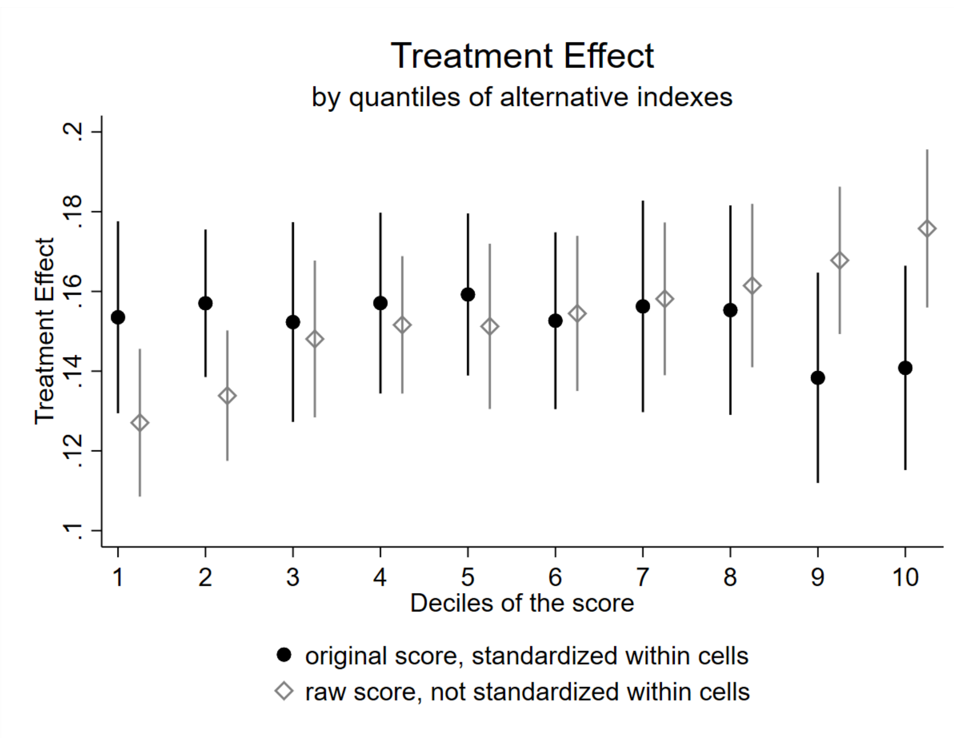
	(1)	(2)	(3)	(4)
	Employment growth		Investment	
	Left	Right	Right	Left
Sub-score for objective rules	0.00230	0.00384	-8.63e-05	0.00335
	(0.0105)	(0.00662)	(0.0197)	(0.0136)
Sub-score for political discretion	0.0250	0.00476	-0.00865	-0.0371
	(0.0157)	(0.0117)	(0.0301)	(0.0243)
Observations	8,020	14,646	6,013	11,013
R-squared	0.473	0.370	0.430	0.391
Test coeff joinly =0 (F)	1.299	0.194	0.0506	1.331
Test coeff joinly =0 (p-value)	0.274	0.824	0.951	0.265

Notes: The table reports regression-based tests of the conditional independence assumption in equation (4.8). We regressed employment growth in the six years after the award of L488 subsidies on the two sub-components of the scores relating to objective rules and political discretion (i.e., S_r and S_d in equation 4.8). The regression also includes all covariates in $X^{\star}$, listed at the beginning of Section 6, as well as 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.

Table A5: *Characteristics of firms participating to calls with and without political discretion.*

	Before discretion	**After discretion**	**Std. diff.**
Age	10.2	10.4	-0.018
Surviving at (t+3)	0.934	0.937	-0.015
(log) Employment	3.355	3.223	0.09
(log) Value Added	6.27	6.276	-0.004
(log) Lab product.y	2.579	2.714	-0.175
Average Wage	565.2	512.8	0.022
Investment rate	0.057	0.056	0.016
Cash Flow Assets	0.049	0.049	-0.011
Liquidity Assets	0.252	0.264	-0.023
Bank Debt (share)	0.195	0.193	0.02
Leverage	2.716	2.985	-0.026

Figure A4: *Treatment effect along the distribution of the standardized and non-standardized application score*



Notes: This figure shows average treatment effects by deciles of the application score, estimated using the linear reweighting estimator in (4.7). The black graph refers to the actual applicant score, which is standardized within each region-sector-type cell (see Section 2), while the grey graph refers to the raw, non-standardized score. 90% confidence intervals based on 2000 replications of a non-parametric cluster bootstrap are also shown.

B Construction of sub-rankings of L488 applications

As explained in Section 2, the final ranking of L488 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 budget to small and medium enterprises (i.e., fewer than 250 employees, turnover under €50 million, or balance sheets below €43 million).

Figure B5: *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 B5 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 B6.

Figure B6: *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	B	C	D	E	F	G	H	I	L	M	N	O	P	Q	R	S	T
				1	2	3	4	5									
Posiz. in grad.	Numero di progetto	Ragione Sociale	Prov.	Capitale proprio	Occupazione attivata	Agevolazione richiesta	Indicatore Regionale	Indicatore Ambientale	Somma indicatori normalizzati	Sett. Serv.	Dimen-sione	Ob.	Cofin	Esito con-clusivo	Cod. escl.	Agevolaz. Concedibile (LM)	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 B7: *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 B7. 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 B8 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 B8: *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"

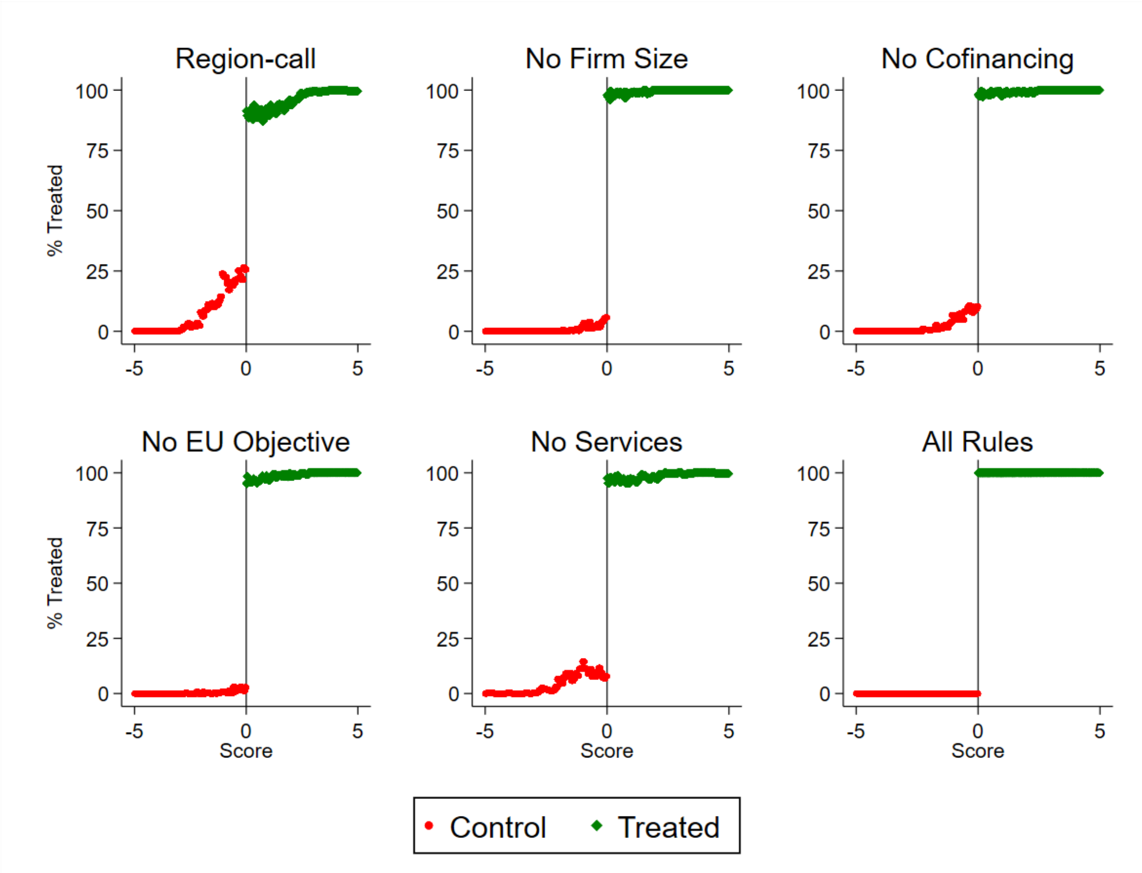
We define a *cell* as the interaction of elements (1) to (6). For example, a cell in our specification could be: projects submitted during the 2nd call in the Tuscany region by small and medium-sized enterprises not active in the service sector, eligible for EU funds, and operating in an Objective 2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

Considering only elements (1) and (2), as in previous evaluations of L488, introduces significant measurement error in treatment assignment near the cutoff (top left panel in Figure B9). 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 B9). The other panels in Figure B9 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.

C The determinants of the political score

In Table C6, we investigate the determinants of politicians' preferences. We regress the *political discretion* index (i.e., the component of the applicant score that is chosen discretionally by the regional government, see Section 2) on a number of explanatory variables potentially capturing political proximity between the regional government and the municipality in which the applicant firm is located. We classify ideology into five categories – left, center, right, local autonomy, and a residual category – and code a dummy equal to one if the regional and municipal government share the same ideology. The baseline specification in column (1) of the table includes such variables on the right-hand side of the equation, together with a set of dummy variables for the municipal government's ideology; region × year fixed effects, which control (among other things) for the ideology of the regional government, and a full set of municipality fixed effects. In column (2) we add a set of dummy variables equal to one if the regional governor and/or at least one alderman/councillor in the regional government were born in the municipality where the applicant firm is located; in column (3) we interact such variables with the

Figure B9: *Measurement error in treatment assignment due to errors in the construction of rankings*



dummy for political alignment between the regional and municipal government.[23] Columns (4)-(6) replicate the same specifications in (1)-(3), but weight observations by the municipal population.

[23]These data come from the administrative registries of local politicians and elections that are publicly available from the Italian Ministry of Interior (https://dati.interno.gov.it/). We obtained the classification of local governments' ideologies from the Local Opportunities Lab (https://www.localopportunitieslab.it/).

Table C6: *Determinants of the political score*

	(1)	(2)	(3)	(4)	(5)	(6)
Region-municipality alignment	-0.034	-0.036	-0.039	-0.014	-0.025	-0.040
	(0.030)	(0.030)	(0.033)	(0.047)	(0.044)	(0.052)
Birthplace governor		-0.043	-0.046		-0.175	-0.172
		(0.241)	(0.250)		(0.129)	(0.124)
Birthplace alderman		-0.000	0.001		-0.124	-0.137
		(0.059)	(0.063)		(0.086)	(0.088)
Birthplace councillor		0.078*	0.076		0.031	0.038
		(0.045)	(0.050)		(0.057)	(0.059)
Alignment × Birthplace governor			0.030			0.281
			(0.423)			(0.357)
Alignment × Birthplace alderman			-0.008			0.070
			(0.124)			(0.105)
Alignment × Birthplace councillor			0.012			-0.032
			(0.100)			(0.094)
Left	-0.075	-0.073	-0.072	-0.361***	-0.341***	-0.345***
	(0.047)	(0.045)	(0.045)	(0.117)	(0.094)	(0.095)
Center	-0.012	-0.012	-0.011	-0.273***	-0.248***	-0.250***
	(0.045)	(0.044)	(0.044)	(0.100)	(0.084)	(0.085)
Right	0.003	0.004	0.004	-0.155*	-0.139*	-0.145*
	(0.061)	(0.061)	(0.061)	(0.084)	(0.078)	(0.078)
Local autonomy	0.555	0.548	0.547	0.602	0.623	0.615
	(0.502)	(0.499)	(0.500)	(0.706)	(0.688)	(0.692)
Observations	51,425	51,425	51,425	51,418	51,418	51,418
Weighting by population	N	N	N	Y	Y	Y
R-squared	0.177	0.177	0.177	0.090	0.090	0.090

Notes: We estimate the determinants of political preferences for applicant firms. We use an OLS regression of the political discretion index on different proxies for the political proximity between the regional government and the municipality in which the applicant firm is located. The units of observation are the single applicant firms. All specifications include municipality and region × year fixed effects, and standard errors are clustered by municipality. Regressions in columns (4)-(6) are weighted by municipality population. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% level.

D RD estimates at the cutoff: Additional results

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

Figure 9 shows that firms subsidized at time t have a lower probability of being subsidized in the following years relative to control firms. In this case, the estimated dynamic treatment effects on outcomes in $t + \Delta$ reported in Table 2 and Figure 8 reflect both the direct effect of the subsidy obtained at time t and the indirect, negative effect of receiving less subsidies between t and $t+\Delta$, hence they underestimate the direct effects of the policy. 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:

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

where all variables are defined as in equation (4.1), and the sub-index "1" denotes the period.[24] 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 suitable to properly estimate $\tilde{\tau}_2$ (i.e., the causal

[24] 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.

effect of D_1 on the outcome in $t = 2$):

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

An estimate of τ_2 could be recovered from a regression analogous to (D.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 (D.1) allowing for year-specific coefficients τ_1^t ($t = 1996, \ldots, 2006$) 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: $\tilde{Y}_2 = Y_2 - \tau_1^t D_2$.[25] 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 (D.2). The procedure is then iterated to estimate the direct effects of the policy at further horizons.

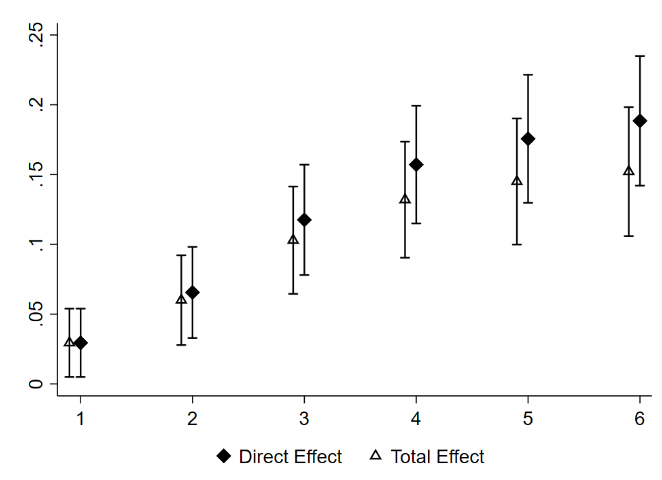
Figure D10 compares the total effect of the subsidy received at time t on employment growth at different time horizons, as reported also in Table 2 and Figure 8, with the direct effect obtained by subtracting the effect of subsequent subsides, estimated following the procedure described above. As expected, in light of the evidence in Figure 9, 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.

D.2 Spillovers

To estimate the spillover effects of subsidies, we compare the dynamics of employment between non-subsidized firms within the same local labor market (LLM) and non-subsidized firms in other LLMs. To the extent that there are significant spillover effects, they should have more of an impact on the former group than on the latter. We thus estimate the effect of having (at least) one subsidized firm in LLM m on (changes in) employment of non-subsidized firms within the same LLM, relative to non-subsidized firms in other LLMs, using the following specification:

[25] 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 D10: *Total and direct effects for re-applicants*



Notes: The graph compares the total effect of obtaining a subsidy, as estimated in Table 2 and Figure 8 (second graph), with the direct effect obtained by subtracting the contemporaneous effect of any subsidy obtained in subsequent calls, as detailed in equations (D.1) and (D.2).

$$\ln L_{m,t+k} - \ln L_{m,t} = \theta_k D_{m,t} + \alpha \ln L_{m,t} + FE_m + FE_t + \varepsilon_{m,t} \tag{D.3}$$

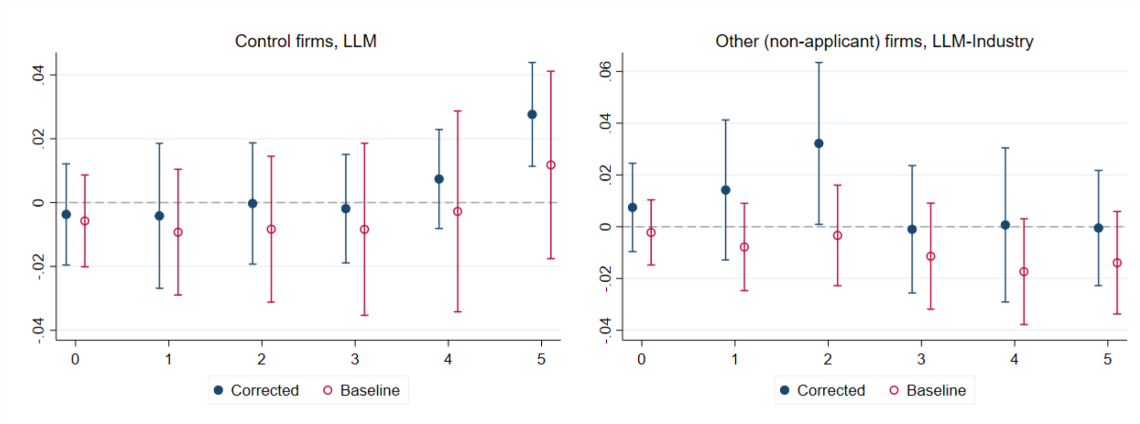
where $L_{m,t+k}$ and $L_{m,t}$ are the total employment of non-subsidized firms in the m-th LLM in year $t + k$ and t, taken from the INPS administrative data on the universe of workers in (non-agricultural) firms; $D_{m,t}$ is a dummy equal to 1 when at least one firm in LLM m received funds in year 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 D11 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.[26] Both graphs present baseline difference-in-differences estimates as well as "corrected" estimates accounting for the staggered research design, using the

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

approach suggested by de Chaisemartin & D'Haultfœuille (2020).[27] Overall, there is no evidence of significant spillover effects; the same is true when replacing the binary indicator $D_{m,t}$ in equation D.3 with the (log of) funds actually paid to subsidized firms in each LLM or LLM-industry, see Figure D12. These results imply, among other things, that higher employment in subsidized firms does not represent a mere reallocation of jobs from non-subsidized firms.

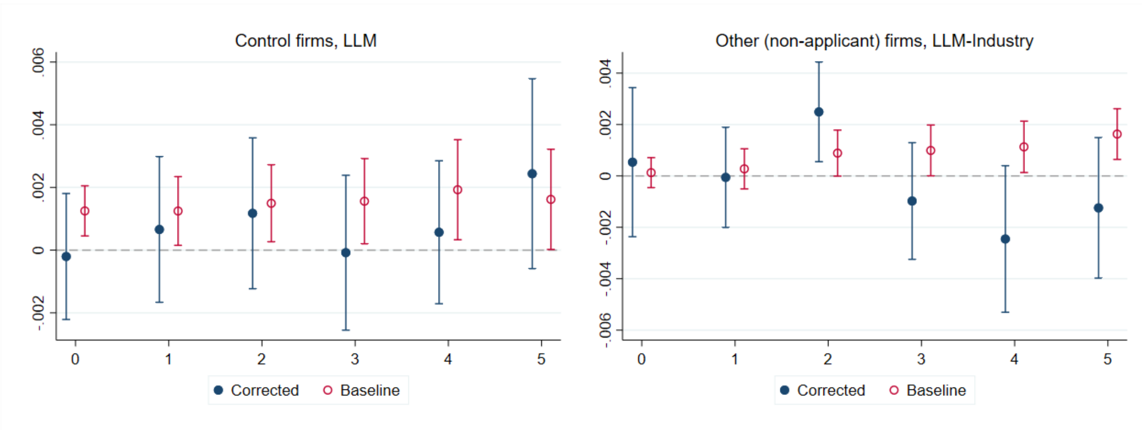
Figure D11: *Spillover effects on other firms in the same labor market (binary indicator for subsidized firms)*



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 an indicator for the Local Labor Market (or the LLM-industry cell) receiving some funds. "Baseline point" estimates and confidence intervals are obtained from specification D.3, 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.

[27] de Chaisemartin & D'Haultfœuille (2020) show that whenever treatment assignment is staggered across units (as it is the case in our context) and treatment effects are heterogeneous (as it is reasonable to assume), the estimated coefficient θ_k in equation (D.3) is a weighted average of all treatment effects with possibly negative weights and, as such, it is not informative about any population of interest (see also Goodman-Bacon 2018). For instance, the regression coefficient may be negative when all treatment effects are positive. de Chaisemartin & D'Haultfœuille (2020) then propose an alternative estimator addressing this issue by restricting the sample to units switching treatment status in each period.

Figure D12: *Spillover effects on other firms in the same labor market (log of total subsidies)*



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 D.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.

E 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 $C_1 = \emptyset$.

- 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 $j = 1, \ldots, k_2$,

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

   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 (E.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 (E.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.