Public Subsidies, Political Discretion, and Policy Effectiveness*

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Abstract

We estimate the effects of a large program of public investment subsidies to private firms in Italy. Investment projects presented by firms were ranked by numerical scores reflecting both objective measures of project quality and the discretion of local politicians, and they were funded according to such ranking until exhaustion of available funds. We estimate that subsidies increased investment of marginal firms near the cutoff by 39 percent, and employment by 17 percent over a 6-year period. Building on recent advancements in the econometrics of regression discontinuity designs, we characterize treatment effect heterogeneity and cost-effectiveness of subsidies across inframarginal firms. Smaller firms exhibit higher employment growth upon receiving the subsidy, but larger firms generate more jobs at a lower cost, and younger firms exhibit do better than older firms. Using the estimated distribution of treatment effects, we compute the effects of the policy under different allocation criteria. Eliminating political discretion would reduce the cost per job by 10 percent, and adopting an optimal criterion based on a set of observable firm characteristics would reduce the cost by over one third relative to the actual policy.

JEL Classification: H25, J08

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

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1 Introduction

Public subsidies to private firms represent an important component of public expenditure around the world. Before the Covid-19 pandemics, the total budget of place-based policies in the United States amounted to $61 billion per year, 80% of which consisted of tax credits and cash grants 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 €279 billion (€46.5 billion per year), to which one should add the resources invested by the single member states. In the wake of the pandemics, the budgets of these policies are bound to increase by an order of magnitude.

The effects of such policies depend crucially on the allocation of funds, and yet we know very little about the marginal effects of subsidies across different types of potential beneficiaries. For example, many believe that small (or infant) firms grant the highest returns to public capital while facing severe liquidity constraints (see e.g. Chodorow-Reich 2014, Schmalz et al. 2017, Criscuolo et al. 2019, Siemer 2019); at the same time, recent work suggests that market frictions rather affect larger, more mature producers, forcing them to forego available 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. On the other hand, discretion may be used for private benefits rather than for the public interest, as in the case of politically connected firms (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 realms of government intervention in the economy, including industrial policy (Laffont 1996).

In this paper, we investigate the relevance of allocation criteria – notably, political discretion – and firm characteristics for the effectiveness of public subsidies distributed under Law 488/92 (L488 henceforth), the largest program of public subsidies ever implemented in Italy and one of the largest in Europe (Giavazzi et al. 2012). Between 1996 and 2007, L488 financed 77 thousand investment projects over 35 open calls, with a total budget of nearly €26 billion (at constant 2010 prices) partly supplied through the ERDF. We estimate the causal impact of these funds on firm investment, job creation, and productivity.

This analysis faces three main challenges, common to virtually all policy evaluation studies, which typically impose trade-offs between desirable research objectives. First, subsidized and non-subsidized firms may differ along other, possibly unobserved dimensions (*internal validity*). Second, the effect of the subsidy may vary considerably with the characteristics of subsidized firms (*heterogeneity*). Third, it is hard to understand the policy impact in different contexts or under different allocation criteria (*external validity*). To achieve internal validity, we typically estimate average treatment effects across a subset of "compliers" with plausibly exogenous variation. However, average treatment effects may mask significant heterogeneity across potential beneficiaries, 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 quantifying aggregate effects (e.g., in terms of total job creation) and the evaluation of alternative allocation schemes, which would be most useful for the purposes of policy evaluation.

To overcome these limitations, we leverage on recent methodological advances in Regression discontinuity (RD) analysis (Angrist & Rokkanen 2015, Dong & Lewbel 2015, Cattaneo, Keele, Titiunik & Vazquez-Bare 2020, 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 cutoff. Together with the peculiar features of L488 and the detailed available firm-level data, these results allow us to characterize the effect of subsidies across potential beneficiaries under the actual and under alternative allocation criteria.

L488 subsidies were allocated through open calls for projects. The budget of each call was first allocated across the 20 Italian regions, privileging economically under-developed regions in the South; within each region, calls targeted firms in manufacturing and service industries, spanning the entire size and age spectra. Investment projects submitted by firms within each call-region were then ranked according to an analytical score of project quality, and financed on a first-ranked, first-served basis until the exhaustion of funds. Importantly, the selection criteria weighted both objective indicators ("rules") and subjective priorities indicated by local politicians ("discretion"), with weights that varied over time.

This allocation mechanism generates an ideal RD design. We find that firms just above the cutoff increased investment by almost 40 percent, compared to those just below the cutoff, over the six-year period during which they received government funds. This transitory shock to investment generated persistent increases in employment, averaging 10 percent during the subsidy period and reaching 17 percent over the following three years (i.e., 6 years after being awarded the subsidy). Allowing for spillover effects within local labor markets, we show that subsidized firms do not expand at the expenses of other non-subsidized firms, so our estimated effects capture a net increase in the total number of jobs. 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 subsidy is as-good-as-randomly assigned. However, Angrist & Rokkanen (2015) note that, unlike in other settings, selection into treatment in RD designs is entirely determined by the running variable — in our case, the application score. They then show 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 controls, then we can extrapolate treatment effects for any value of the running variable by matching treated and controls on X. Both conditions (i) and (ii) are testable and they hold in our case for a parsimonious set of firm characteristics. Using this approach, we recover the full distribution of treatment effects across all firms in our sample; we characterize its heterogeneity across different types of firms; and we compute the overall effect of the policy when allowing and not allowing for political discretion, respectively.

The results of this analysis show that treatment effects on employment growth and investment are approximately constant in the running variable (i.e., the applicant score). At the same time, treatment effects as well as the effectiveness of subsidies, as measured by the cost-per-newly-created-job, vary along other dimensions such as firm age and size. Smaller firms generate greater *percent* increases in employment (over +30%), but bigger firms generate a higher number of new jobs, and they do so at a lower cost. The cost per newly created job is 50 thousand euros for firms in the top decile by size, and increases by an order of magnitude for the smallest firms. Subsidies to younger firms generate larger treatment effects and are more cost-effective than subsidies to older firms.

The heterogeneity analysis uncovers, in addition, a stark divide between Northern and Southern Italy, respectively, as the cost per new job created by the subsidy is almost 4 times larger in the latter than in the former. The cost of investment exhibits a similar gradient, as each Euro of subsidy generates three Euros of investment in the North, but only one in the South.

To isolate the role of political discretion, we then re-compute employment effects under alternative, hypothetical criteria that ignore the subjective priorities indicated by local politicians. In particular, we re-compute the ranking of applicants when excluding the sub-component of the applicant score that is decided by local politicians, and integrate firm-specific treatment effects over the set of firms selected under the new rule. This exercise maintains that firms' decision to apply for L488 funds is 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, when 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 decreases by about 10% compared to the actual policy, and by more in southern regions. A similar result holds for the cost of new investment. Therefore, political discretion seems particularly detrimental in economically disadvantaged regions, which also received more funds. In a similar vein, 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.

Previous papers have investigated the impact of L488, reaching different conclusions on its effectiveness. Using a difference-in-differences approach, Bronzini & de Blasio (2006) estimate a positive impact on 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 anticipated already-planned investment projects, so the net effect on firm investment is not different from zero. Cerqua & Pellegrini (2014) reach an opposite conclusion – positive effects on investment and, also, employment – implementing an RD analysis on data for the second, third, and fourth call for projects (1997-1998). Our final dataset, obtained after merging L488 applications with administrative firm data, covers the majority of applications submitted to all L488 calls for projects during the period 1997-2007 – over 40 thousand projects submitted by 27 thousand firms.[1] We also complement these data with additional information on institutional rules and budgets allocated to specific categories of firms and projects. Such information allows us to identify the multiple sub-rankings and cutoffs within each call-region and, thus, to correctly construct the RD design.[2] Most importantly, we characterize the distribution of treatment effects for the whole sample and compute the effects and cost-effectiveness of L488 subsidies under alternative allocation criteria.

These results contribute to a broader literature on the a causal impact of public subsidies on investment, employment, and economic activity. The seminal paper by Hall & Jorgenson (1967) models the effect of fiscal stimuli on investment and estimates significant effects of investment subsidies in the US during the 1950s-60s. More recently, several papers estimated positive employment effects of the American Recovery and Reinvestment Act exploiting plausibly exogenous variation across US states (see, among others Wilson 2012, Chodorow-Reich et al. 2012). 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, exploiting as an RD design the rule allocating the bulk of funds to European regions with GDP per capita below 75% of the European average (including southern Italian regions receiving L488 funds). They find that eligibility to additional funds increase GDP growth by 1.6 percentage points, but the size of such effect varies dramatically with the "absorptive capacity" of recipient regions, as determined by human capital endowments and quality of local governments. At the same time, there is no significant effect on employment.[3]

All these previous papers rely on aggregate, regional-level data, while firm-level evidence on the effects of public subsidies on firm investment and employment remains limited. Notable

[1] For comparison, Bronzini & de Blasio (2006) and Cerqua & Pellegrini (2014) have information on 2,237 and 1,702 applicant firms, respectively.

[2] Cerqua & Pellegrini (2014) pool together all applications within the same call-region, ignoring that some were prioritized (e.g., those presented by small firms and projects eligible for EU funds). We discuss these issues in detail in Section 2 and Appendix B.

[3] 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). Bartik (2020) and Ehrlich & Overman (2020) provide recent surveys of different types of spatially targeted subsidies.

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 to R&D in a single Italian region were largely ineffective in raising investment. Both papers estimate larger and significant effects for smaller firms. We also find that smaller firms generated larger *percent* increases in investment and employment; however, they also received higher subsidies-per-worker, so subsidies to larger firms are ultimately more cost-effective.

We also contribute to a burgeoning literature on the effect of discretion for success 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 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.

The paper is organized as follows. The next section describes the institutional context, and Section 3 and 4 introduce the data and empirical strategy, respectively. Section 5 present the results for marginal firms near the cutoff, while Section 6 shows 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 concludes.

2 Institutional framework

Italy has been historically characterized by an extreme economic divide between northern and southern regions.[5] In 2001, the median value added per capita across local labor markets in northern regions was twice as high as that in southern regions – €18.5 and €9.5 thousand,

[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.

[5] Throughout the paper, the term "northern regions" refers to both regions classified as North and Center by the national statistical institute – 8 and 4 regions, respectively, on a total of 20 regions.

respectively. The level of economic activity also varied widely within southern regions, with a 90/10 ratio in value added across local labor markets equal to 3 – as compared to just 2 within northern regions.[6] The last few decades also witnessed a marked slowdown in workers' mobility. In 2005, the one-year mobility rate was one third than that in the United States, and one of the lowest ones across countries (Molloy et al. 2011).

Large territorial divides and low workers' mobility provide a strong rationale for spatially-targeted subsidies (Kline & Moretti 2014, Bartik 2020). During the post-war period, southern Italy received massive inflows of public funds, and L488 was the main policy tool for distributing these funds between the mid-1990s and mid-2000s. The law was approved by the Parliament at the end of 1992 but became effective only in 1996, and it remained in place until 2007; the total budget during such period amounts to €26 billion (at constant 2010 prices).

Several categories of projects were potentially eligible for receiving L488 subsidies. First, industrial projects aimed at creating, expanding, and modernizing establishments. Second, projects relating to the production and distribution of energy, steam or hot water. Third, projects relating to the construction sector. Fourth, projects relating to the IT sector, but only within the limit of 5% of the total budget.

Funds were allocated through open calls for tenders, each one targeting a specific economic sector – primarily industry, but also tourism and trade. All 35 calls issued between 1996 and 2007 are listed Appendix Table A.1, together with the total amount of funds, the number of applications received, and the target sector. Industry obtained the lion's share (€21.9 billion), followed by tourism (€2.7 billion); see Table 1.

The selection of applicants in each call proceeded in two steps. In the first step, funds were allocated across the 20 Italian regions. In line with the main objectives of the policy, almost 85% of the funds were allocated to less economically developed areas in the South (last column of Table 1). For instance, two of the poorest regions of the country, Campania and Sicily, received nearly €6 billion and €5 billion, respectively, as compared to €0.25 billion and €0.13 billion for Lombardy and Emilia Romagna, respectively. Figure 1 clearly shows the negative relationship between funds and regional GDP per capita.[7]

In the second step of the allocation process, projects submitted by applicant firms within each call-region were ranked and financed until the exhaustion of funds available for that call-region. The ranking depended on quantitative indicators of project quality, combined with rules regarding minimum quotas of L488 funds reserved to specific categories of applicants (e.g.,

[6]The Italian National Statistical Institute (ISTAT) defines local labor markets as clusters of contiguous municipalities on the basis of workers' commuting patterns – similar to the US commuting zones. In 1991, the mean population local labor markets was 73 thousand, compared to 7 thousand across municipalities. For additional details, see https://www.istat.it/en/labour-market-areas.

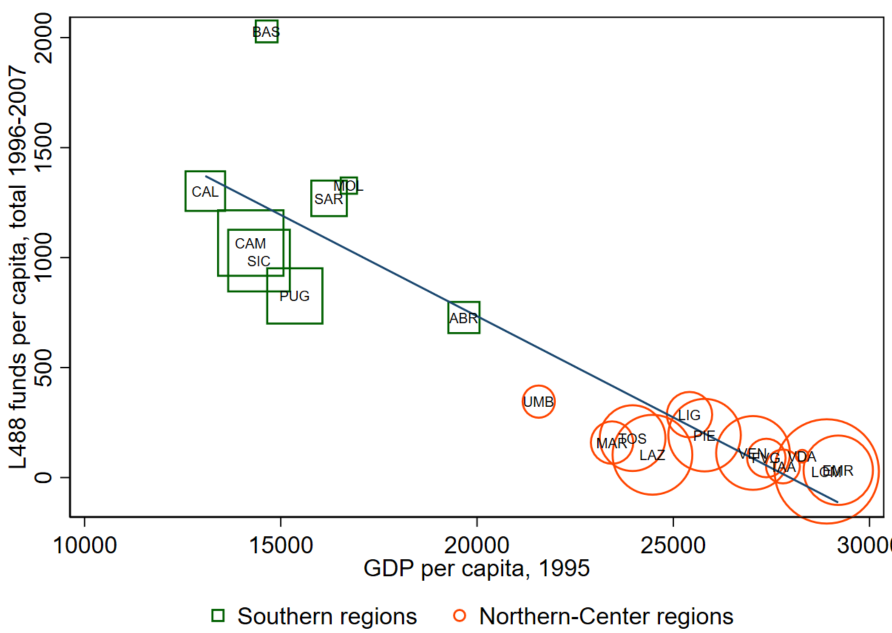
[7]Appendix Figures A.1 and A.2 provide additional descriptive evidence on the evolution and composition of funding over time and across geographical areas.

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 billion of euros at constant 2010 prices.

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.

small-medium firms) or eligibility for co-financing on EU funds. The information required to construct the quantitative indicators was elicited from applicants 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) the project is evaluated according to "political and regional priorities" ("*political discretion*")

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

Clearly, the introduction of criterion C4 increased the scope for political discretion in the allocation of funds, while the other criteria captured objective features of the project. In Section 6, we will examine the implications of political discretion on 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:

$$S_i = \sum_j (I^j_{ic} - \mu^j_c), \tag{2.1}$$

where I^j_{ic} is the value achieved by project i in call-region c on the j-th indicator, and μ^j_c 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 the additional rules, mentioned above, regarding specific categories of applicants and projects. In particular, there were four such rules:

[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 C.6 shows that none of these variables appears to be significantly related to the political discretion index. However, this null result may be due to the fact that measures of political proximity between regional and municipal governments do not adequately capture connections between applicant firms and the local government. Unfortunately, firm-level indicators of political connections of the type used, e.g., in 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. 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

$$S_i = \sum_{j=1}^{4} (I^j_{ic} \times I^5 - \mu^j_c)$$

where $I^5 = 1.05$ if the applicant is compliant with environmental certification, and $I^5 = 1$ otherwise.

- at least 50% of the budget within each region was reserved for small-medium enterprises, defined as those having fewer than 250 employees and either a turnover smaller than €50 million or balance sheets smaller than €43 million;
- at most 5% of the budget within each region could be allocated to firms operating in the service sector;
- projects meeting certain requirements – in terms, e.g., of location, type, and duration of investments – were eligible for additional co-funding on the EU structural funds, so they could be financed even when higher-ranked projects eligible only for national funding were not.

Appendix B explains these rules in greater detail. Importantly, such rules define multiple rankings within the unique ranking by region published in the *Gazzetta Ufficiale*. We recovered these multiple rankings exploiting additional information on firm size, operating sector, eligibility for co-financing, and geographical area, also provided in the *Gazzetta Ufficiale*, to construct the RD design.[10] In the context of this paper, whenever we refer to a "call-region" we really mean the more sophisticated definition of ranking that includes the four rules described in the previous paragraph.

The outcome of the selection process was published within four months since the deadline for submission of projects, and subsidies were paid in three equal instalments. The first instalment was paid within two months, while the other two were paid one and two years later, respectively, conditional on compliance with the planned execution of the project. Specifically, the second instalment was paid only if 2/3 of the project had been realized, while the last one was paid only if the project had been completed; if this was not the case, one or both of the last instalments were not paid, and the firm would be required to repay the previous instalments plus an additional fine. This monitoring system ensured a coherence between the projects proposed in the applications and their execution.

3 Data

The analysis leverages on a unique dataset combining administrative data on applications for L488 subsidies, registry data on applicant firms and their employees, and a proprietary database of balance sheet data. Starting with the first source of data, we obtained from the Italian Ministry of Economic Development detailed information on all applications at 26 calls for L488 funds held between 1996 and 2007 – a total of 74,584 projects submitted by 49,082 firms.[11] For each

[10] Previous evaluations of L488 relied only on the overall ranking by region.

[11] The original data did not include 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 the firm-level subsidies.

project, the dataset reports the fiscal identifier of the applicant firm, together with its location and sector; the requested subsidy; the final score obtained by the application and its sub-components; the amount eventually awarded to the applicant firm, if any. We complemented these data with additional information from the *Gazzetta Ufficiale* allowing us to identify the sub-rankings within each call-region, as explained in the previous section. Nearly 33 thousand projects were eventually eligible for financing, for a total amount of €22 billion.

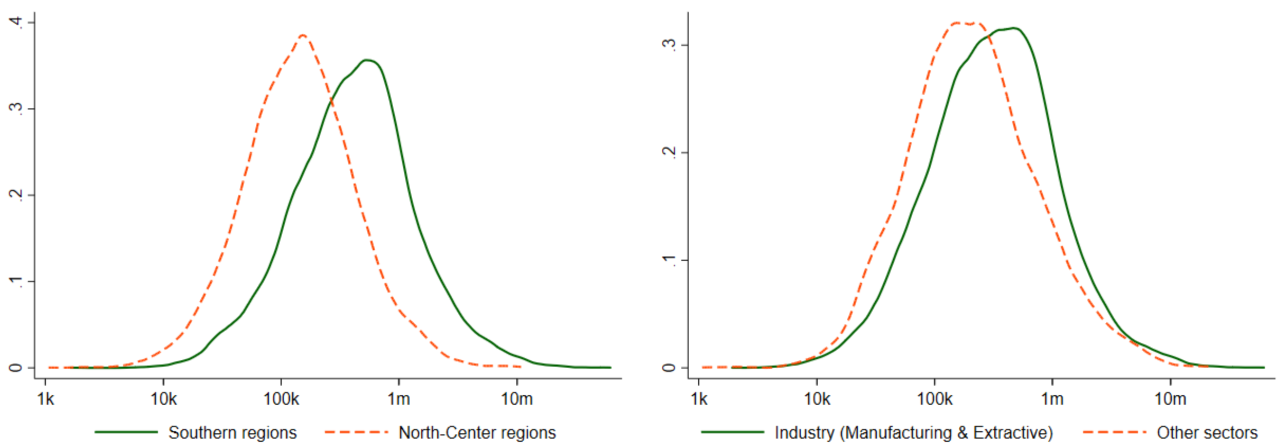
The second source of data are the administrative registries of the Italian Social Security Institute (INPS), which cover the universe of Italian firms with at least one employee (around 1.6 million firms each year). These data report the employment levels of each firm at monthly frequencies as well as business birth and cessation dates. Using these data, we can track very precisely, at monthly frequencies, the employment of applicant firms both before and after applying for (and possibly obtaining) the subsidy, as well as their 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 thousand applications by micro-enterprises. When estimating dynamic treatment effects of the subsidy, we drop another 10 thousand applications from firms that first appeared in the INPS data on the year of the call (i.e., start-up firms), as the credibility of our empirical strategy relies on the dynamics of outcomes in the period before the call. We also trimmed the top 1% of firms in terms of size, which employ on average 5 thousand workers (i.e., 100 times the median firm size in our sample). These are the dominant firms in high returns to scale industries (as 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 main results. Our main analysis of employment effects will rely on a sample of 40,344 projects submitted by 27,074 different firms.

For the vast majority of our sample, we could also retrieve 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 can thus observe additional outcomes, notably investment, revenues, and value added. Reassuringly, Appendix Figure A.3 shows that this final sample matches well the initial set of applications in terms of the main variables included in both datasets – namely, requested and awarded subsidy, score obtained by the project, and the sub-component for job creation.

We conclude this section by exploring how the subsidies actually paid to (winning) applicants vary by geographical area, economic sector, and firm characteristics. In particular, Figure 2 compares the distribution of the log of subsidy between Southern and North-Center regions (left graph) and between firms in industry and in other sectors (right graph). Larger budgets allocated to Southern regions and industry translate into larger firm subsidies, on average.

Figure 2: *Amount of L488 subsidies per firm, by geographical area and economic sector*



Notes: This figure shows the distribution of the log of L488 subsidy across firms by geographical area (left graph) and economic sector (right graph).

Not surprisingly, the total subsidy received by each firm is increasing in its size. When rescaling the subsidy by the number of employees in the previous year, smaller firms receive more generous subsidies on average. This is shown in the left graph of Figure 3, which relates the log of subsidy per employee to the log number of employees. The right graph in the same figure shows that subsidies were also more generous for younger firms. We will take these facts into account when evaluating the effectiveness of the policy across different types of firms.

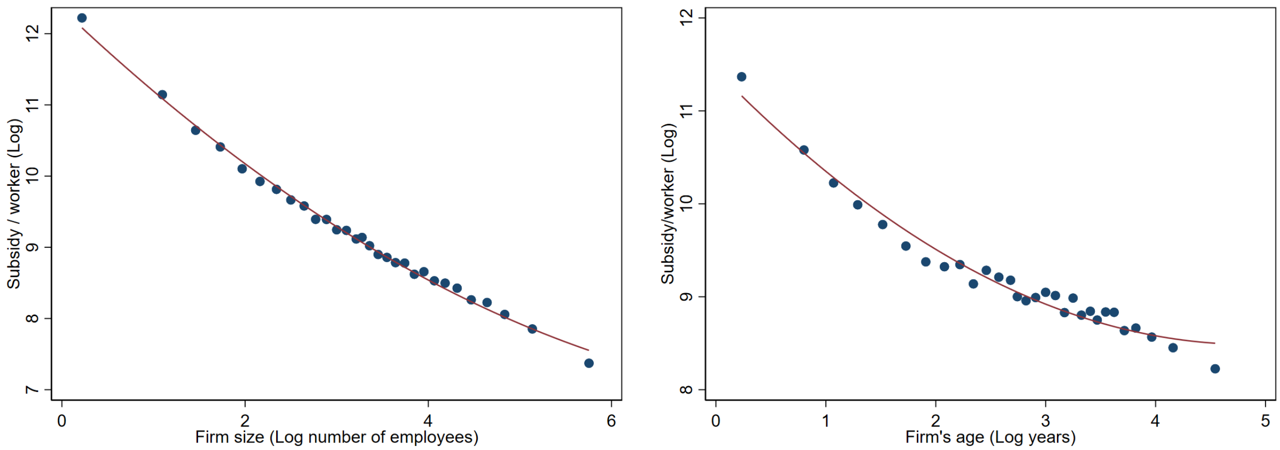
4 Empirical Strategy

Let Y_i^1 and Y_i^0 be the potential outcomes of firm i competing in call-region c when obtaining and not obtaining the subsidy, respectively. In addition, let $\tilde{S}_i$ be the score received by firm i and $\bar{S}_c$ the cutoff score required for obtaining the subsidy in call-region c. Then, $S_i = (\tilde{S}_i - \bar{S}_c)$ is the standardized score for each firm, with $S_i = 0$ at the cutoff, and $D_i = 1(S_i \geq 0)$ is a dummy indicating treated firms. To the extent that 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,

$$\tau = \lim_{s \to 0^+} E(Y|S = s) - \lim_{s \to 0^-} E(Y|S = s),$$

where $Y = DY^1 + (1 - D)Y^0$ is the realized outcome. In practice, we pool data across all calls and estimate τ parametrically by regressing firm outcomes on the dummy for receiving the subsidy,

Figure 3: *L488 subsidies per worker and firm characteristics*



Notes: This figure shows the relationship between the log subsidy per employee and the log number of firm employees (left graph) and the log firm's age (right graph) across equally-sized bins.

D, controlling for a flexible 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 call-region c and ε is a residual term summarizing the effect of other factors. Fixed effects implicitly control for common factors by region and economic sector, and standard errors are also clustered by call-region. We restrict the sample to applicants with an application score within the bandwidth $[-5, 5]$ (82% of our sample), and we use linear and quadratic polynomials in S. We also experiment with triangular kernels attaching greater weight to observations closer to the cutoff.[12]

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 marginal firms near the cutoff. The local RDD estimate of τ for this subset of firms has the same internal validity as the estimate obtained through a randomized experiment (Lee 2008). However, treatment effects on inframarginal firms away from the cutoff – and, thus, the overall effect of the policy – are not identified in general (see, e.g., Lee & Lemieux 2010). In our context, it is plausible to think that higher-scoring firms differ along some unobservable dimension (e.g., managerial ability). Angrist & Rokkanen (2015) propose a method to identify treatment effects across inframarginal units away from the cutoff. This method leverages on the fact that, in RD designs, treatment assignment is fully determined by the running variable – in

[12] As recommended by Gelman & Imbens (2019), we avoid specifying higher-order polynomials.

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 the potential outcomes are independent of the running variable conditional on X, that is if

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

then one can estimate treatment effects at any point of the running variable distribution by comparing treated and controls conditional on X. Condition (4.2), in fact, implies that confounding effects (e.g., higher-scoring firms also being better-managed) would be either absorbed by X or part of an error term which is uncorrelated with the outcome of interest.

To be more specific, write the running variable S as a function $S = g(X,u)$ of some (observable) variables X and some (potentially unobservable) variables u – for instance, 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 so because conditioning on X makes potential outcomes independent from S, hence from u too. Therefore, variables that would bias an estimate of treatment effects away from the cutoff because correlated with *both* potential outcomes and the running variable are either included in X or in u. In the former case, we control for them, whilst in the latter we can safely ignore them. In addition to (4.2), assignment to treatment must be probabilistic conditional on X, i.e.

$$0 < P(D = 1|X) < 1, \tag{4.3}$$

which is commonly known as common support condition.

Importantly, in this context both the conditional independence and common support assumptions are partially testable. That is, the RD design provides a test for the (usually untestable) assumption that conditioning on X removes all confounding differences between treated and controls. In fact, under conditional independence (4.2) and common support (4.3), we can rewrite the treatment effect at $S = s'$ as

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

Following Angrist & Rokkanen (2015), we estimate (4.4) using the linear reweighting estimator by Kline (2011). Specifically, we model conditional mean outcomes for treated and control firms as, respectively,

$$E[Y|S,X,D = 1] = \sum_k \gamma_k^1 S^k + X'\beta^1 \tag{4.5}$$

and

$$E[Y|S,X,D = 0] = \sum_k \gamma_k^0 S^k + X'\beta^0. \tag{4.6}$$

Failure to reject the restriction $\gamma_k^1 = \gamma_k^0 = 0,\ \forall k$, provides partial evidence consistent with the conditional independence assumption in (4.2) – the untestable part being the same restriction holds 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

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

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 identify average treatment effects all over the support of the running variable S.

Another important implication of the CIA restriction in (4.2) is that one can also explore the heterogeneity of the average causal effect across subgroups of units indexed by the observables Z:

$$E[Y^1 - Y^0|Z] = E[E[Y^1 - Y^0|X, Z]|Z] = E[E[Y^1 - Y^0|X]|Z] \tag{4.8}$$

where the last step exploits the independence between the potential outcomes and any other variable conditional on X. Then, using (4.5) and (4.6) we get:

$$E[Y^1 - Y^0|Z] = (\beta^1 - \beta^0)' E[X|Z] \tag{4.9}$$

Hence, the method allows us exploring whether the effects of subsidies vary with firm characteristics that are deemed important in the literature as well as in terms of policy as firm size and age, their sectoral or geographical dimension, financial conditions, and so on.

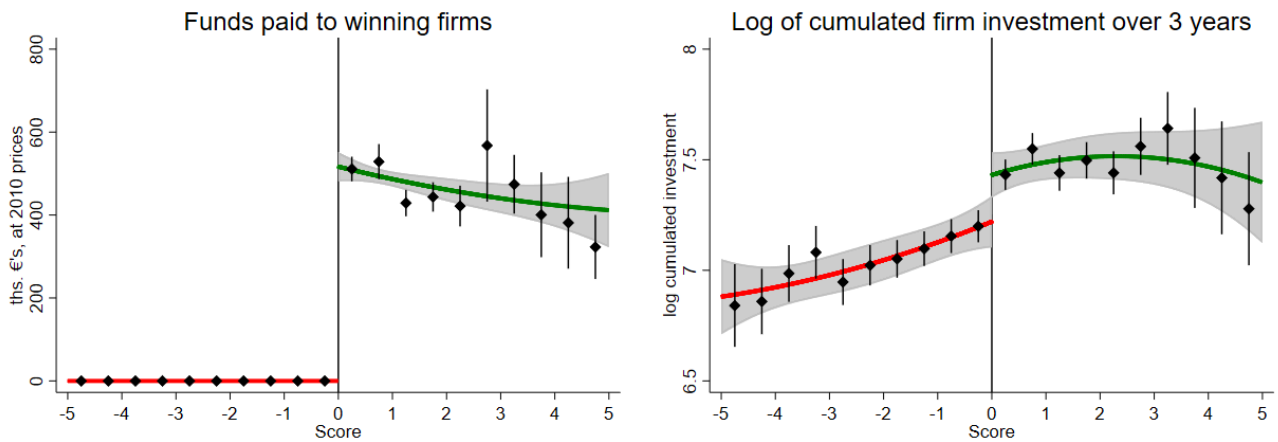
Finally, the same method allows us to identify the policy impact under alternative assignment rules – notably, the rule maximizing job creation. Such rule would rank applicants according to $E[Y^1 - Y^0|X]$. Importantly, evaluating the impact of alternative policies requires that firms' decision to apply for funds is not affected by the change of criteria. In the next section, we provide indirect evidence consistent with such assumption, based on the comparison of applicant firms before and after the major change in allocation criteria that occurred from the 3^{rd} call for projects.

5 Results at the RDD cutoff

Figure 4 plots the relationship between the score obtained by applicant firms and, respectively, the subsidy they received (left graph) and the log of total, cumulated investment over the three following years (right graph). Averages and confidence intervals for equally-spaced intervals for score of size 0.5 are presented, 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. During the same period, they increased investment by 23 percent on average, compared to other other (control) applicants that ranked just to the left of the cutoff.[13]

Figure 4: *Funds obtained by winning firms and investment over the following 3 years*



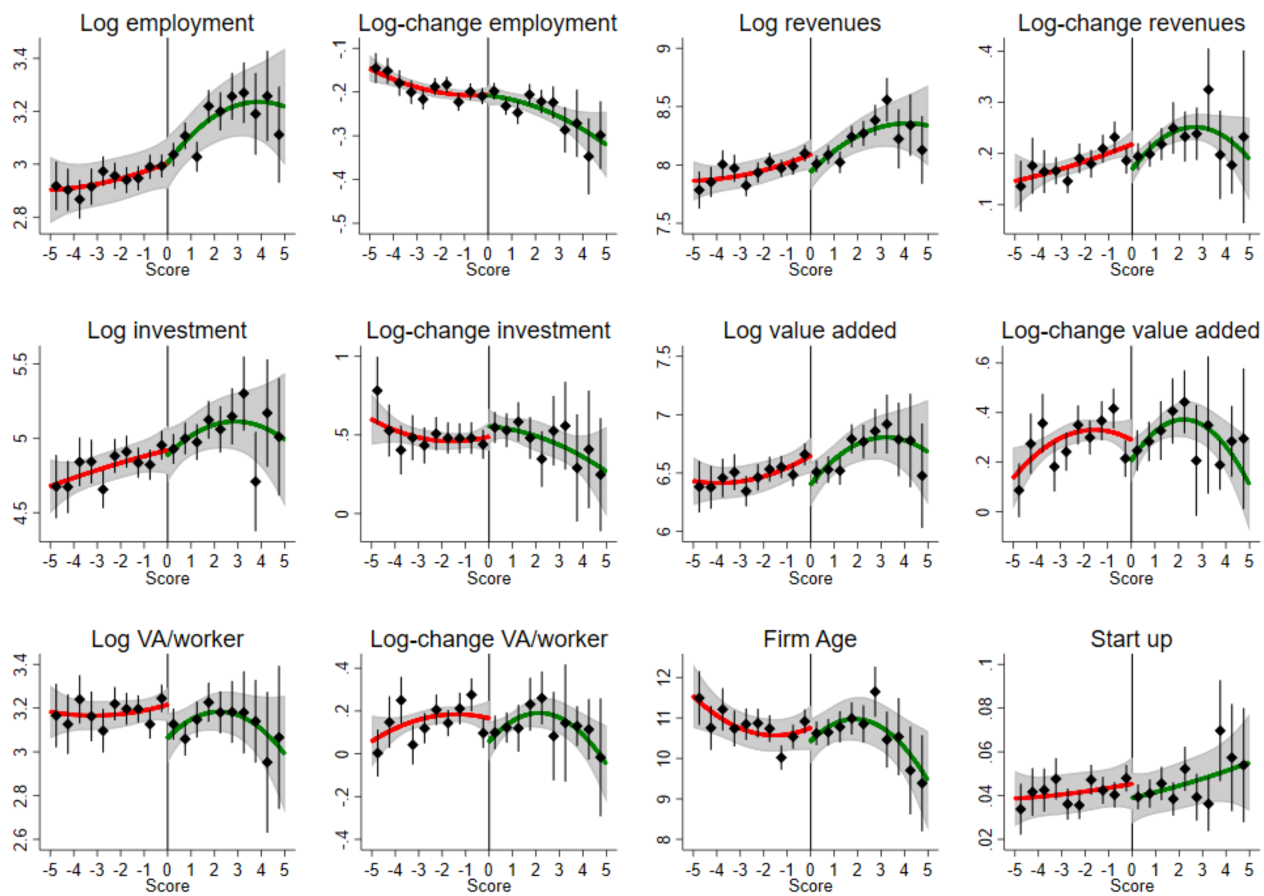
Notes: This figure shows the relationship between the standardized score obtained by firm applications for L488 funds, on the horizontal axis, and the amount of funds actually received (left graph) and the log of cumulated firm investment over the following three years (right graph). Bins represent averages over equally-spaced intervals of size 0.5 and confidence intervals for each bin are also shown by vertical lines. The predicted relationships between each outcome and the score are estimated using a quadratic polynomial regression. For both outcomes, confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by call.

Figure 5 shows that applicants ranking just above and below the cutoff, respectively, are on average equal in terms of a wide range of other characteristics, as measured one year before the call; Appendix Table A.2 provides the results of formal tests. Figure 6 shows that the five components of the score, described in Section 2, also vary smoothly around the cutoff. Finally, Figure 7 shows no evidence of discontinuity in the density of applications, as confirmed also by the formal test by McCrary (2008) (as implemented by Cattaneo, Jansson & Ma 2020).

Taken together, Figures 5, 6, and 7 strongly support the main identifying assumption that

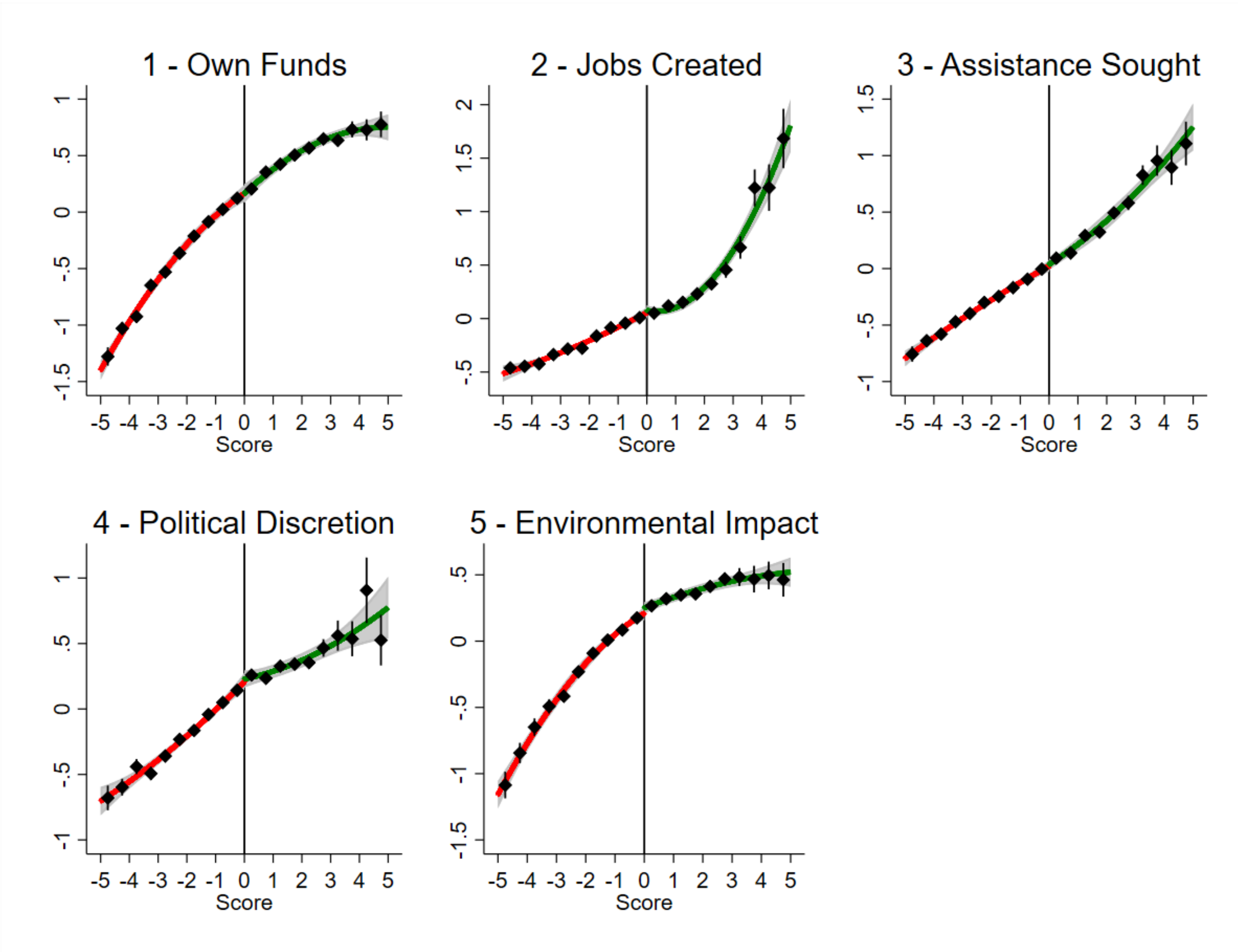
[13]Interestingly, firm investment varies positively with the score to the left of the cutoff (i.e., conditional on not getting the subsidy, higher-ranked firms invest more), while the relationship is flat to the right of the cutoff, and it even turns slightly negative for very high values of the score. This likely 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, high-score firms obtained – other things equal – lower subsidies and, as a consequence, they may generate lower additional investment.

Figure 5: *Balance of firm characteristics one year before the call*



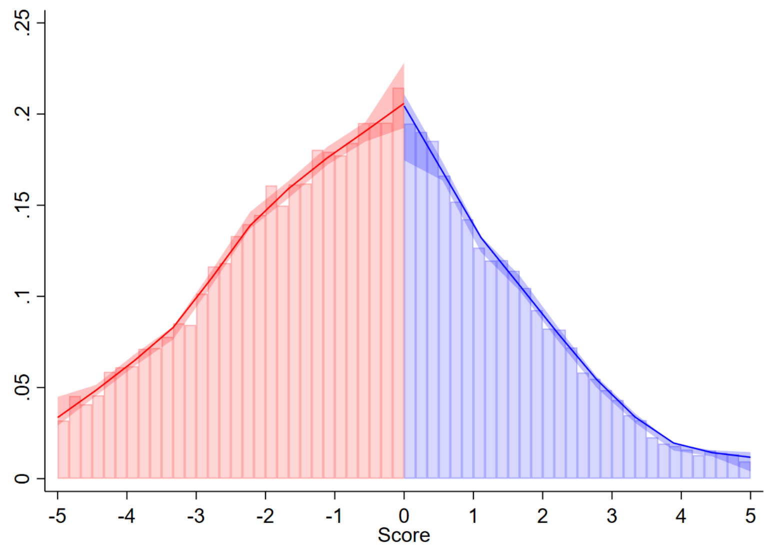
Notes: These graphs show the relationship between the standardized score obtained by firm applications for L488 funds, on the horizontal axis, and several firm characteristics measured one year before the call – namely, log and yearly log-change in revenues, value added, value added per worker, investment, and firm age and being a start-up. Bins represent averages over equally-spaced intervals of size 0.5 and confidence bands for each bin are also shown by vertical lines. The predicted relationships between each variable and the score are estimated using a quadratic polynomial regression, controlling for call-specific fixed effects. Confidence intervals for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by call.

Figure 6: *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 the previous Section 2). Bins represent averages over equally-spaced intervals of size 0.5 and confidence intervals for each bin are also shown by vertical lines. The predicted relationships between each variable and the score are estimated using a quadratic polynomial regression, controlling for call-specific fixed effects. Confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by call.

Figure 7: *Density of applicant scores*



Notes: The histogram shows the distribution of applicant scores. Local polynomial density estimates, computed according to Cattaneo, Jansson & Ma (2020) are also reported in the figure.

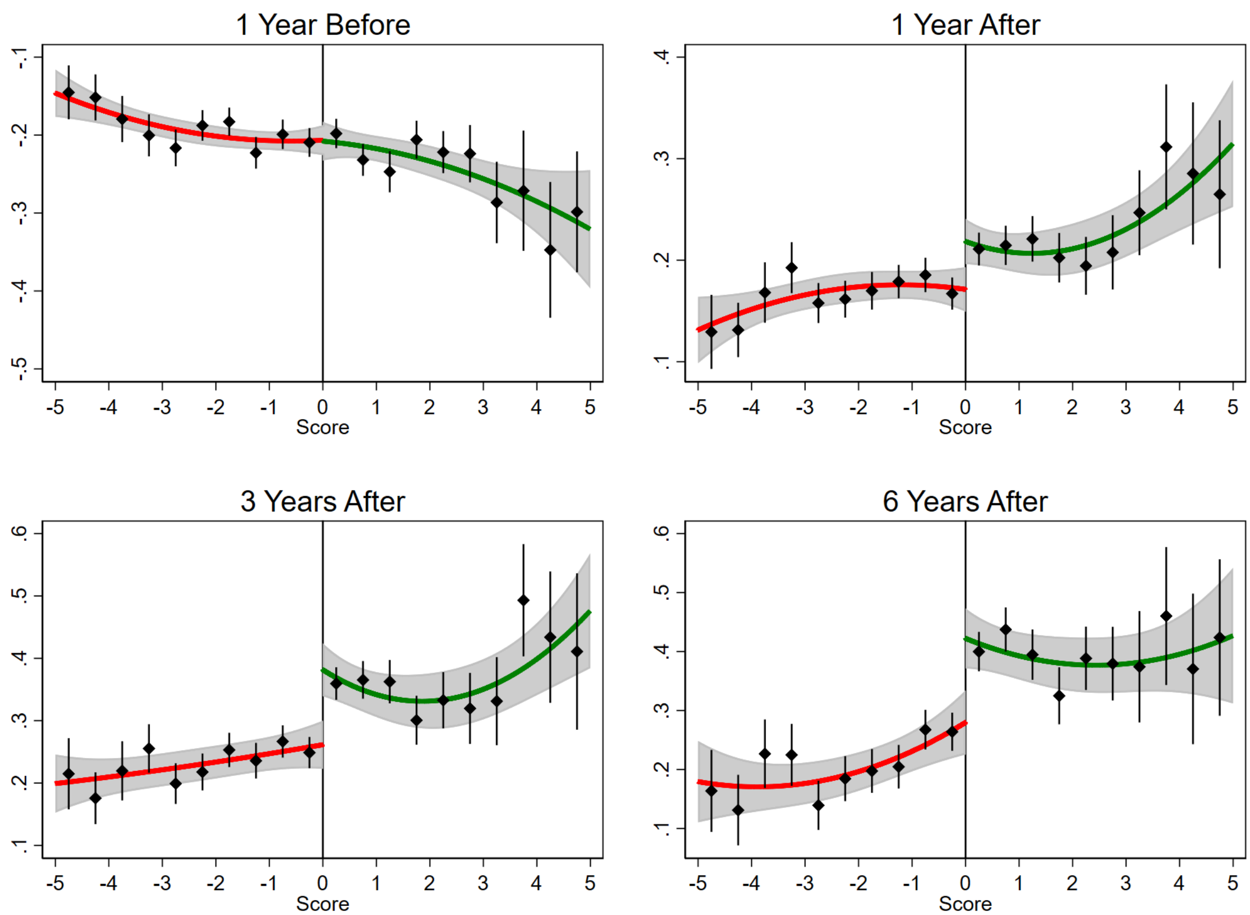
applicants within an arbitrarily narrow bandwidth of the cutoff are unable to precisely determine their assignment to either side of it. We can thus attribute any difference in outcomes between firms scoring just above and just below the cutoff, respectively, to the causal effect of the subsidy. We next move to the analysis of the main outcome of interest, namely employment.

5.1 Baseline results

The stated objective of Law 488 was to favor employment in disadvantaged areas. Figure 8 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. Such 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 during the following years (last graph).

Table 2 quantifies the evidence in Figures 4 and 8 by presenting estimates of equation 4.1 under a variety of specifications. Specifically, we experiment with linear and quadratic polynomials in the running variable (columns 1-4 and 5-8, respectively); with uniform and triangular kernels, the latter attaching a greater weight to observations near the cutoff; and including a full set of call-region fixed effects (even columns). All results remains virtually identical under all these

Figure 8: *The effect of the L488 subsidy on firm employment*



Notes: These graphs show the relationship between the standardized score obtained by firm applications for L488 funds, on the horizontal axis, and its (log) employment 1 year before the call for applications (left graph) and 1 year and 3 years after the assignment of the subsidy (middle and right graph, respectively). Bins represent averages over equally-spaced intervals of size 0.5 and confidence intervals for each bin are also shown by vertical lines. The predicted relationships between each variable and the score are estimated using a quadratic polynomial regression, controlling for call-specific fixed effects. Confidence intervals for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by call.

specifications. In light of this, throughout the rest of the paper we will focus on the simple linear specification with fixed effects (column 2 of Table 2). According to such specification, obtaining the subsidy increases firm investment by 33 percent over the following three years (Panel A), it increases employment by 10.5 percent over the same period (Panel C), and by 15 percent over a period of six years (Panel D). All these estimates are strongly statistically significant.

Figure 9 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, respectively), implying in turn that firm productivity remains approximately constant (fifth graph).

Finally, the last graph in Figure 9 shows that firms receiving the subsidy exhibit 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 mortality hits the lowest-performing firms in the control group (as it seems likely), the estimated effect on the other outcomes of interest – employment, revenues, value added, and productivity – is a lower bound to the average treatment effect when including also non-surviving firms.

5.2 Additional results

We next discuss two issues that potentially affect the interpretation of our estimates. First, applicants in a given call may re-apply (and obtain funds) in subsequent calls. Second, the effects on funded firms may spill over to other, non-funded firms.

Starting with the former issue, 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 $t + \Delta t$. The sign of this effect is a priori unclear. On the one hand, firms that already obtained funds in year t may not have additional (promising) projects to submit in year $t + \Delta t$, or they may be constrained in the amount of own resources that could be invested in such projects. In this case, the dynamic treatment effects on outcomes at $t + \Delta t$ reflect both the direct effect of the subsidy obtained at time t and the indirect, negative effect of receiving less subsidies between t and $t + \Delta t$. Hence, 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 chances of succeeding in year $t + \Delta t$, due to increased availability of resources, reputation effects, and so on. In this case, we would be over-estimating the direct treatment effects of the subsidy on outcomes in the following years.

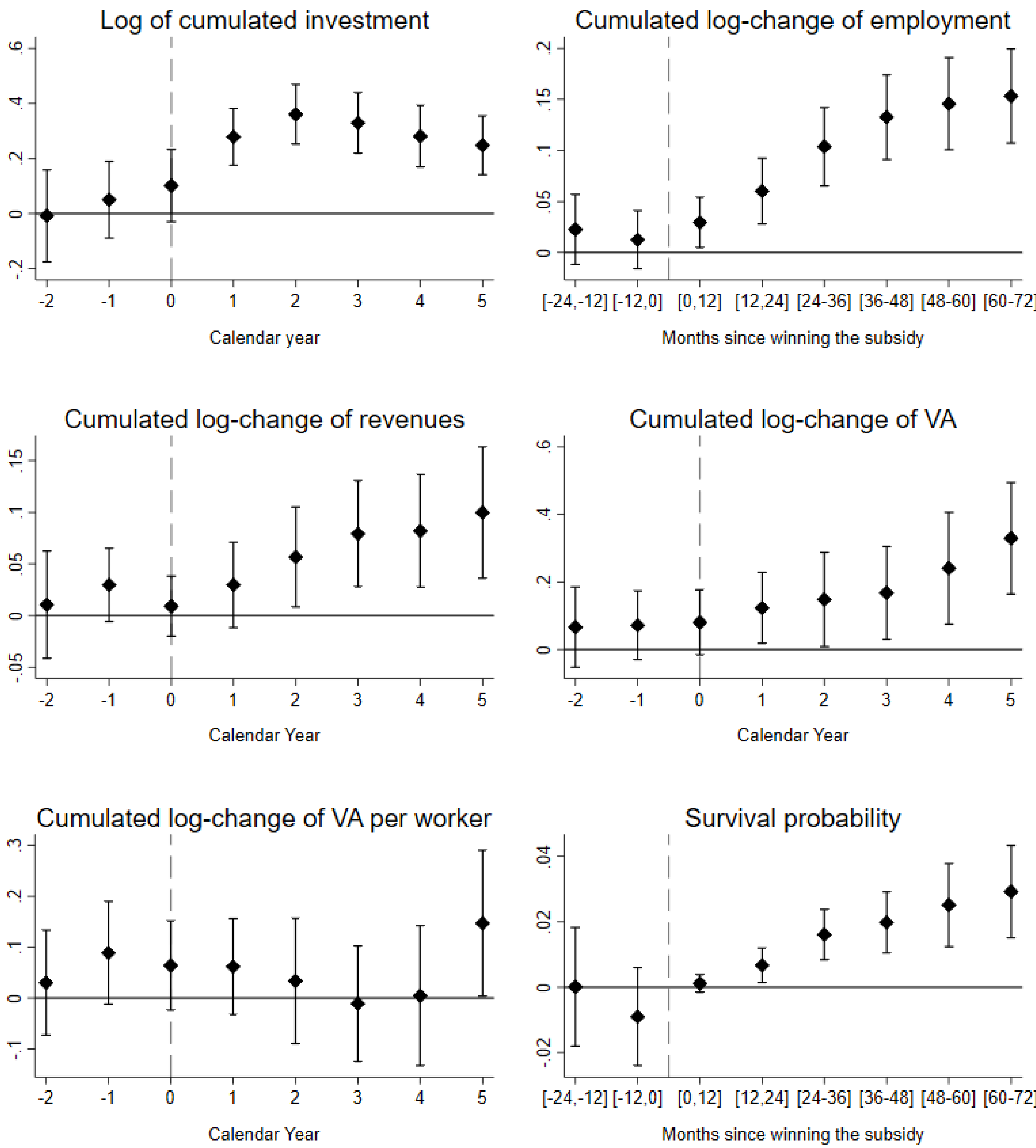
In practice, we sign the (indirect) effect of obtaining a subsidy on the probability of obtaining additional funds in the following years using our baseline RDD specification (4.1). Figure 10

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 being eligible for the subsidy 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 (calendar) year of subsidy assignment and in the two following years (Panel A); and log change of firm employment in the 12 months, 36 months and 72 months after subsidy assignment (Panels B, C, and D, respectively). 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; 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 call-region are reported in parenthesis. $*$, $**$, and $***$ denote statistical significance at the 10%, 5%, and 1% level, respectively.

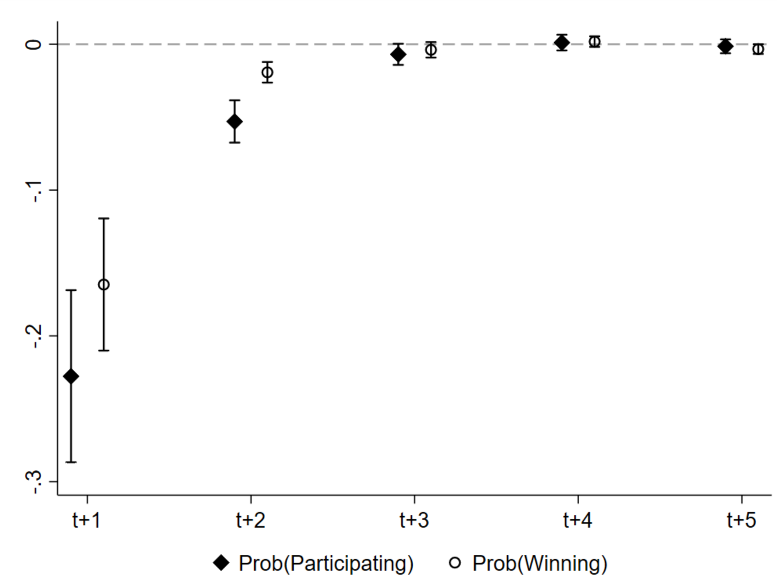
Figure 9: *Dynamic effects of the L488 subsidy on several firm outcomes*



Notes: These graphs show the estimated effects (and associated confidence intervals) of the subsidy on several outcome of interest, indicated on top of each graph, at different time horizons, indicated on the horizontal axis. 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 group fixed effects for firms competing in the same ranking and clustering heteroskedasticity-robust standard errors at the same level.

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 9 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, under testable restrictions, to isolate 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 10: *Direct and indirect effects for re-applicants*



Notes: The graph shows the estimated effect (and associated confidence intervals) of obtaining a subsidy in year t on the probability of participating and obtaining the subsidy in subsequent years, as estimated from an RD regression analogous to 4.1.

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. On the one hand, the growth of subsidized firms may benefit upstream and downstream producers in the same market; on the other, it may erode the market share of competitors – possibly including part of the 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 jobs generated in the local economy.

To address this possibility, we compared the dynamics of employment between non-subsidized firms within the same local labor market (LLM) and non-subsidized firms in other LLMs; spillover effects should affect more (or only) employment in the former group than on the latter. 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, however. These results, presented in Appendix D.2 imply that employment increases in subsidized firms represent a net increase aggregate employment, rather than a mere reallocation of jobs from non-subsidized to subsidized firms.

6 Results away from the RD cutoff

The results in the previous section show that L488 subsidies increase employment by 15 percent over a 6-year period across firms near the cutoff. However, this estimate is not informative about the effect of subsidies for other firms, so we cannot compute the total number of new jobs generated by the policy. To address this limitation, we estimate the full distribution of treatment effects following the approach of Angrist & Rokkanen (2015). This analysis will allow us, in turn, to 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.

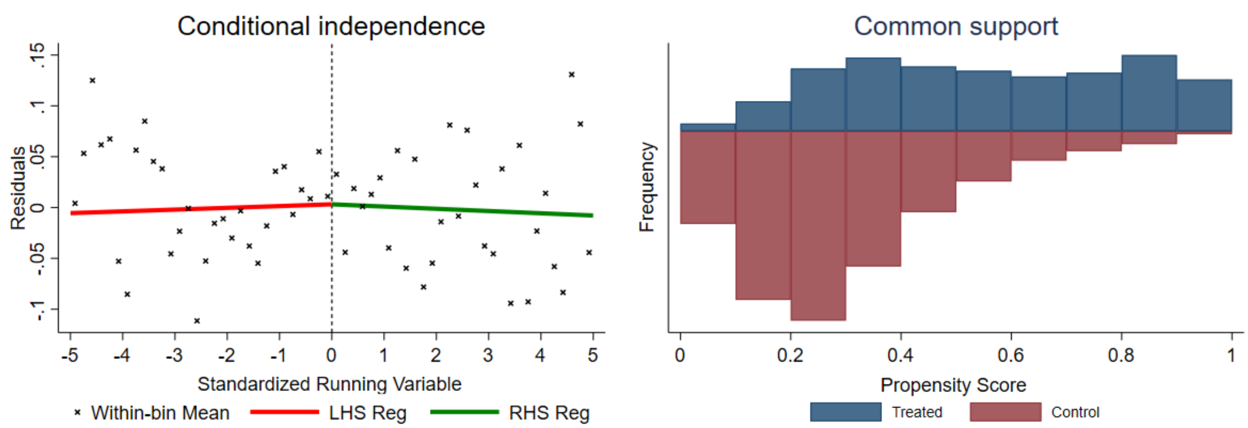
As discussed in Section 4, Angrist & Rokkanen (2015) invoke independence of the running variable and common support between treated and controls, conditional on a set of covariates. We experiment with alternative observable variables that are commonly associated to sources of firm growth, and achieve conditional independence and common support for a plausible set of predictors of growth potential.[14]. These include firm age (which is inversely related to growth, see Evans (1987)); lagged realizations of a firm's growth and 3-year forward growth of firms in the same market (identified by the LLM and 3-digit level); measures of workers' skills (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 to be financed topping public subsidies up with own fund (which should trigger a proportional increase in the workforce) scaled by initial employment.[15]. Importantly, all results are robust when selecting an alternative set of covariates based on the data-driven algorithm by Imbens & Rubin (2015). Such algorithm implements a *greedy approach* that selects, at each step, the variables making the ignorability condition most likely to hold. Appendix E describes this alternative approach in more detail.

[14] All information on firm employment, wages, and composition of the labor force is derived from the social security data data described in section 3, while the amount of subsidy and firm's own investment in the project is available from the application for L488 funds.

[15] More in 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; two dummies for managers or apprentices. All these variables are interacted with project size

Figure 11 visualizes the results of the tests for conditional independence (left panel) and common support (right panel). Let $X^{\star}$ be our set of covariates that satisfies (4.2) and (4.3). Starting with the former, the left graph plots 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. While employment growth increases, unconditionally, with the applicant's score on both sides of the cutoff (see Figure 8), the relationship becomes flat after conditioning on $X^{\star}$ (Appendix Table A.3 confirms that the slope parameters are precisely estimated zeros). At the same time, the right graph in Figure 11 displays considerable common support between treated and controls in the distribution of the propensity score $P(D = 1 \mid X^{\star})$.

Figure 11: *Testing the conditional independence and common support*



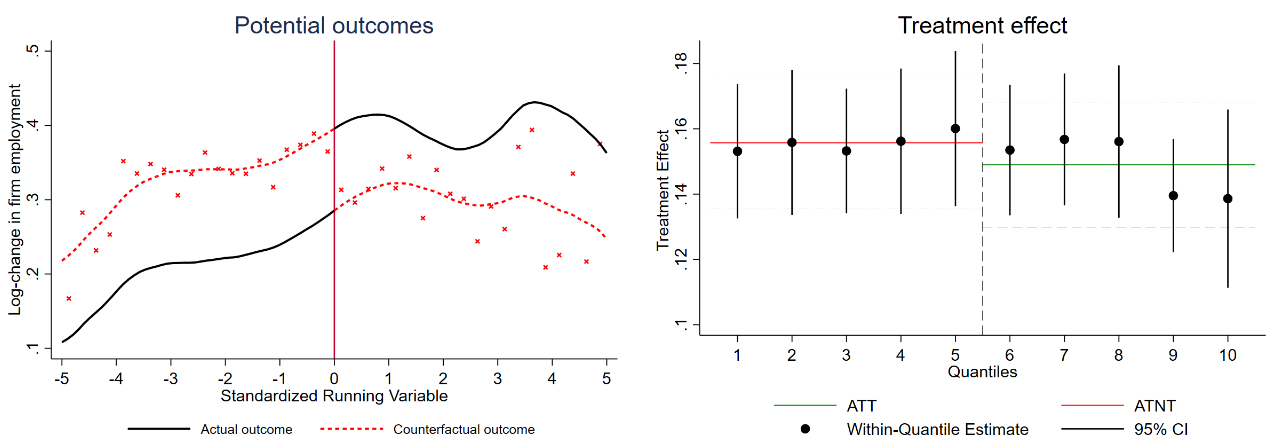
Notes: The left graph shows the test of conditional independence, $E[Y|S, X] = E[Y|X]$ (equation 4.2). Specifically, the graph plots the residuals of a regression of the outcome Y (i.e., firm employment growth in the 6 years after applying for L488 funds) on the set of covariates X, plotted against the running variable S (i.e., the application score). The graph plots the conditional means for applicants within 60 bins in the range (-5,5) of the running variable, along with conditional regression functions. The right graph shows the density of treated and control firms by decile of the estimated propensity score, $P(D = 1|X)$. The set of covariates X include firm age (5 classes), deciles of lagged employment growth, and their interaction; the average wage of white collar workers (deciles) and two indicators for having managers or apprentices in the payroll; deciles of 3-year firm employment growth of all other firms in the same LLM-industry (3-digit level); finally, all these variables are interacted with project size, as measured by the requested subsidy plus firm's own investment in the project.

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

The above conditions allow us to estimate treatment effects across firms away from the cutoff by comparing the outcomes of treated and control firms conditional on the vector of covariates X. Specifically, we use the estimated parameters in equation (4.5) to predict the potential outcome of control firm if they were treated; analogously, we use the estimated parameters in equation

(4.6) to predict the potential outcome of treated firms if they were not treated. The results are shown in the left graph of Figure 12. As it should be expected, both potential outcomes increase with the running variable: higher-ranked applicants exhibit stronger employment growth, both when treated and untreated. The two lines remain approximately parallel, implying that the treatment effect is constant over the distribution of the application score. This is shown in the right graph of Figure 12, which plots average treatment effects (and associated confidence intervals) within equally-spaced bins of the application score.

Figure 12: *Potential outcomes and treatment effect along the distribution of the application score*

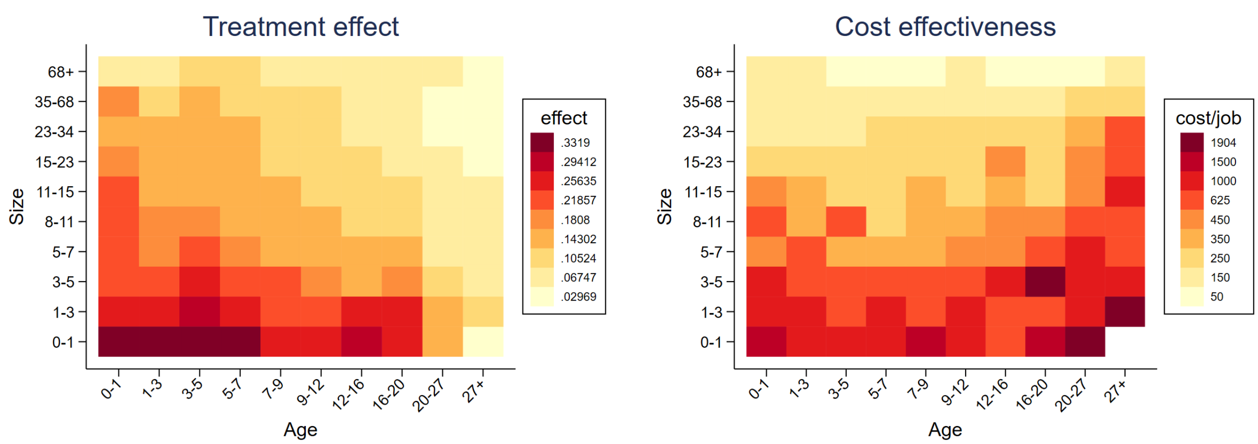


Notes: The left graph shows the potential outcomes when treated and untreated along the distribution of the applicant score. Counterfactual outcomes are estimated on the basis of equations (4.5) and (4.6), respectively. The right graph shows the average treatment effect (and associated confidence intervals) within each decile of the applicant score, estimated using the linear reweighting estimator in (4.7). Confidence intervals are computed relying on 2000 replications of a non-parametric cluster bootstrap.

However, the evidence in Figure 12 masks substantial heterogeneity in the effect of the policy across different types of firms, for three main reasons. First, although the treatment effect does not vary with the applicant score, it may still vary along other dimensions, such as firm size, age, and location. Second, an equal treatment effect on *percent* employment growth would translate into a larger *number* of newly created jobs in large firms than in small firms. Third, and relatedly, the cost-effectiveness of the subsidy depends as much on the number of newly created jobs as on the amount of funds required to create such jobs. This issue is particularly relevant in light of the extreme variation in the generosity of the subsidies across different types of applicants, documented in Section 3.

We characterize treatment effect heterogeneity leveraging on our matching estimator in Equation (4.9). Figure 13 shows treatment effect heterogeneity along (deciles of) firm age and size. The left graph shows that the effect on employment growth is larger for younger and smaller firms. Specifically, the increase in employment is above 30 percent for the smallest firms (i.e., selfemployees or firms with only 1 employee) that are below 8 years of age, and decreases to almost zero for large firms older than 27 years. At the same time, even small percent increases in employment for large firms translate into a high number of new jobs. In addition, larger firms receive lower subsidies per worker compared to smaller firms (see Figure 3) so the cost of creating a new job, as measured by subsidies received over the total number of new jobs, is lower in the former than in the latter. This result is shown in the right graph of Figure 13. The cost-per-newly-created-job starts at 50 thousand for firms in the top decile by size, and it increases by an order of magnitude for the smallest firms. The gradient by age, instead, is similar between the two graphs in Figure 13: older firms exhibit not only a smaller (percent) increase in employment but, also, a higher cost per job. Appendix E shows that the results of the heterogeneity analysis are very similar when using the data-driven algorithm by Imbens & Rubin (2015) to select an alternative set of covariates achieving conditional independence and common support.

Figure 13: *Treatment effect and cost per job by size and age of firms*



Notes: This figure shows the heterogeneity in the treatment effect of the subsidy on firm employment growth (left graph) and cost effectiveness (right graph) by deciles of age and size. The treatment effect for each group (Age=a, Size=s) is estimated as $E[Y^1 - Y^0 \mid \text{Age} = a, \text{Size} = s] = (\beta_1 - \beta_0) \cdot E[X \mid \text{Age} = a, \text{Size} = s]$, where β_1 and β_0 are estimated, respectively, from equations (4.6) and (4.5). The covariates included in X are listed at the beginning of Section 6. In the right graph, cost effectiveness is measured by total subsidies over the number of newly created jobs in each cell. The number of new jobs is computed multiplying the percent treatment effect estimated for each firm by its size, and aggregating across all firms in each cell.

Appendix figure A.4 replicates the analysis in the case of investment. Specifically, it plots the estimated effect of the subsidy on cumulated investment of treated firms up to six years after obtaining the subsidy. The percentage increase is higher among small and young (perhaps not infant) firms. The right hand side panel plots the "cost" of new investment, as measured by subsidies received over cumulated investment. The pattern is similar to that obtained in the case of employment. Among large firms, each €of public subsidy generates significant (up to eight) additional €'s of investment (the gradient looks U-shaped in age). This multiplier decreases with size, and firms in the bottom tercile are estimated to invest less than they receive subsidy (especially if older).

To provide an overall evaluation of the policy, Table 3 quantifies the cost of creating new jobs and additional investment across all firms. The first row of the table shows the cost per new additional job after 6 years since receiving the subsidy. Such cost reaches almost 200 thousand euros, this estimate remaining 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, respectively). Since each job may last several years, we also compute the cost per job-year through year 6, which stands at 55 thousand (columns 3 and 4). Since job duration may extend beyond the sixth year, these estimates are an upper bound to the actual cost of the policy.

The second and third rows of Table 3 highlight the large differences in the cost-benefit ratio of the policy between southern and centre-northern regions. The cost per new job is almost four times higher, and the cost per job-year is almost five times higher, in the former group of regions. These wide gaps in job creation per *e* of subsidy reflect analogous differences in (inverse) investment multipliers, as measured by the amount of the subsidy over new investment on a three-year time horizon. New investment in the south equals the amount of the public subsidy, while each *e* of public subsidy generates more than two additional *e*'s of investment in center-northern regions (columns 5 and 6).

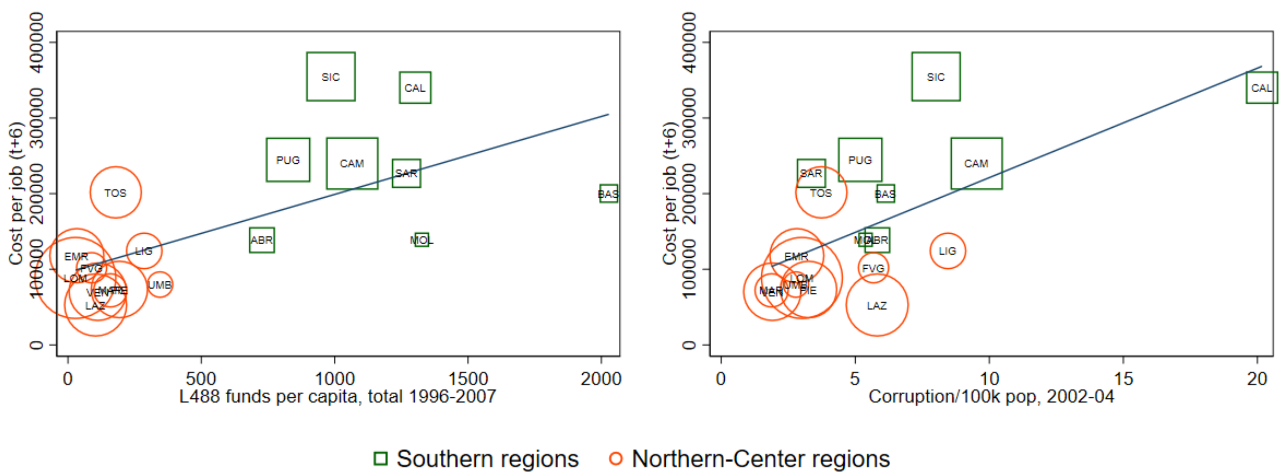
Table 3: *Cost of new jobs and investment generated by L488 subsidies*

	(1)	(2)	(3)	(4)	(5)	(6)
Cost measure:	cost per new job		cost per worker-year		cost of new investment	
CIA set of covariates:	manual	data-driven	manual	data-driven	manual	data-driven
all regions	195,241	195,950	54,836	55,988	0.751	0.687
south	269,857	242,718	84,194	75,798	0.974	0.901
north-center	69,759	86,954	16,878	21,064	0.313	0.281

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 billion of euros at constant 2010 prices.

Therefore, the cost-benefit ratio of the policy was (much) lower precisely in regions receiving the largest share of funds; the first two graphs of Figure 14 provide additional evidence in this respect. This relationship is consistent with the existence of decreasing returns to the mobilization of new public subsidies – as it seems natural to assume. The marginal returns schedule may be particularly steep in southern regions characterized by a scarcity of profitable investment opportunities, which is the very reason why they received a larger share of subsidies in the first place. We next ask whether, in addition to these unfavorable initial conditions, the allocation mechanism is also responsible, at least in part, for the high ratio of subsidy to new jobs and investment, respectively, particularly in southern regions. We address this question with specific reference to the role of political discretion, which intervened in the selection of projects in all calls but the first two.

Figure 14: *Cost per job and cost of investment across regions*



Notes: These graphs plot 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 region population.

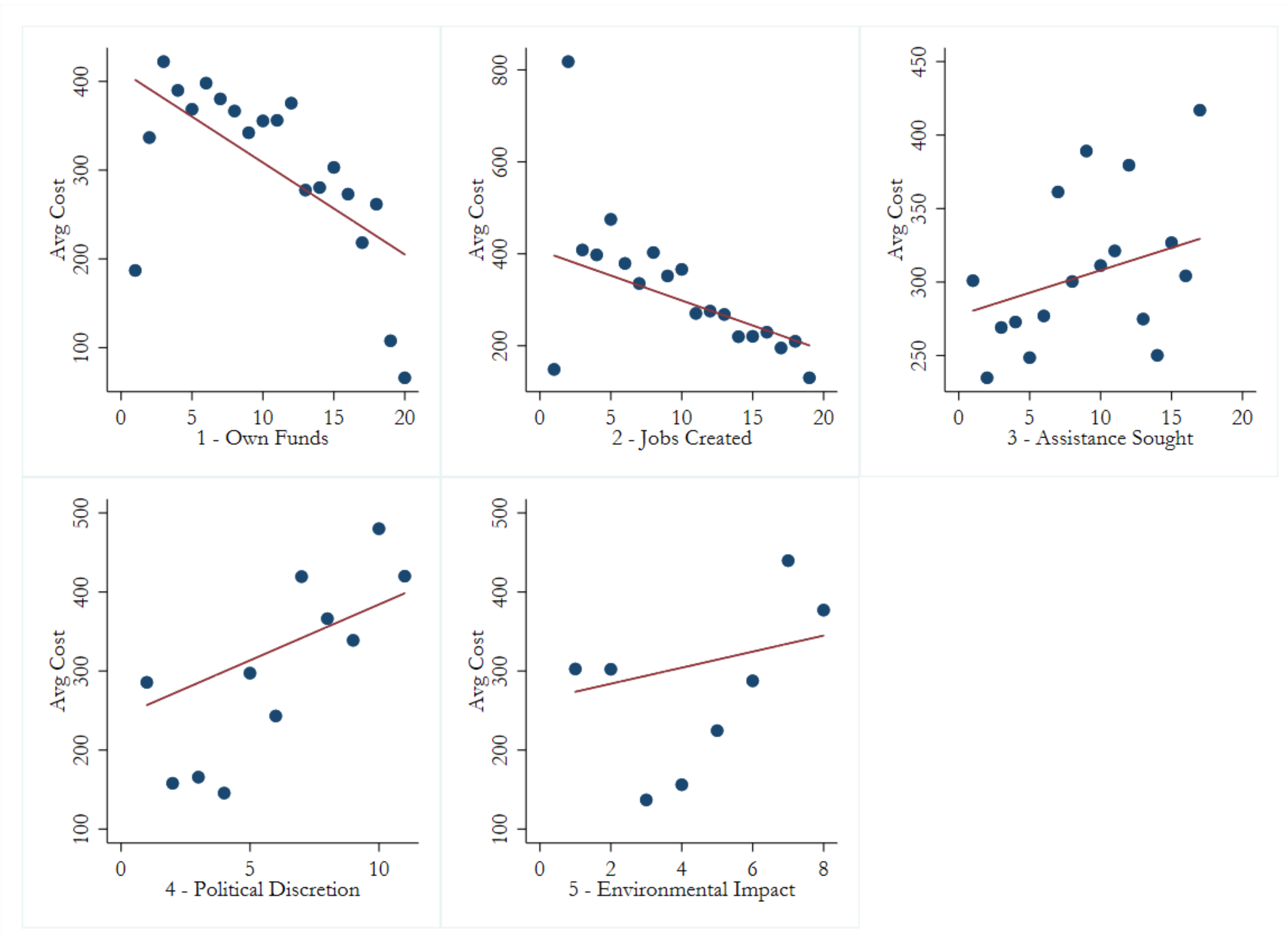
6.2 The role of political discretion

We investigate the impact of political discretion by re-computing the effects of the policy under the alternative policy scenario in which political discretion had no role in the allocation of funds, as explained in Section 4. As a preliminary step to such analysis, we relate all sub-components of the application score (including the sub-component decided, discretionally, by the regional government) to the firm-specific cost of creating new jobs estimated in the previous part of this section. Figure 15 shows that local politicians favor applicants characterized by a lower efficiency in the use of public funds, as reflected in higher cost per new job.[16]

Next, we compute the alternative ranking of applications in all calls for projects (starting with the third call) had the sub-component for political discretion not been included (as it was the

[16]The third and fifth sub-component – namely, the (inverse) of assistance sought and compliance with environmental certifications – also favor high-cost applicant firms; for the specific case of the latter component, this may be due to the fact that environmental certifications impose a burden on the firm. Instead, own investment and planned number of new jobs – respectively, the first and second sub-components – seem most effective in identifying applicants that are most efficient in the use of public funds.

Figure 15: *Cost per newly created job and sub-components of the applicant score*



Notes: These graphs show the relationship between the sub-components of the application score and the firm-specific cost of creating new jobs, as estimated from equation (4.9), across all applicant firms. Each marker corresponds to averages within equally-sized bins.

case in the first two calls). Similarly, we compute the new cutoff by assigning to each applicant the subsidy requested in the application, starting from the top applicants and down on this alternative ranking until the exhaustion of the funds assigned to each call-region. Therefore, some of the applicants admitted to funding under the actual policy would not be admitted under the alternative policy avoiding political discretion, and vice-versa. We then compute the total effect of the new policy by integrating the applicant-specific effects estimated above.

This exercise requires us to maintain that applicant-specific effects of the subsidy are invariant to the criteria used for selecting applicants. In general, this is arguably a restrictive assumption; in the present context, however, we can provide two pieces of supportive evidence. First, we can compare the characteristics of applicants in the first two calls for projects (1996-97), i.e. before political discretion was included among the criteria, and in the following two calls for projects (1998). Appendix Table A.4 shows that the two groups are on average very similar in terms of observable characteristics. Second, the results in 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.

Figure 16 shows that eliminating political discretion would reduce the cost of creating new jobs by 10 percent. Interestingly, the efficiency increase would be more marked in southern than in northern regions (11 percent and 6 percent, respectively). The left graph of Figure fig:changecostjobregions shows, indeed, that eliminating discretion implies a greater reduction cost per new job in regions characterized by a higher cost under the actual policy; the right graph shows similar evidence for the cost of investment.[17]

Finally, we consider an additional, hypothetical ranking assigning priority to firms generating new jobs at the lowest cost, based on the heterogeneity analysis in the right graph of Figure 13.[18] Figure 16 shows that the cost of creating new jobs would decrease by 37 percent in Southern regions, and by just less than 30 percent in Center-Northern regions.

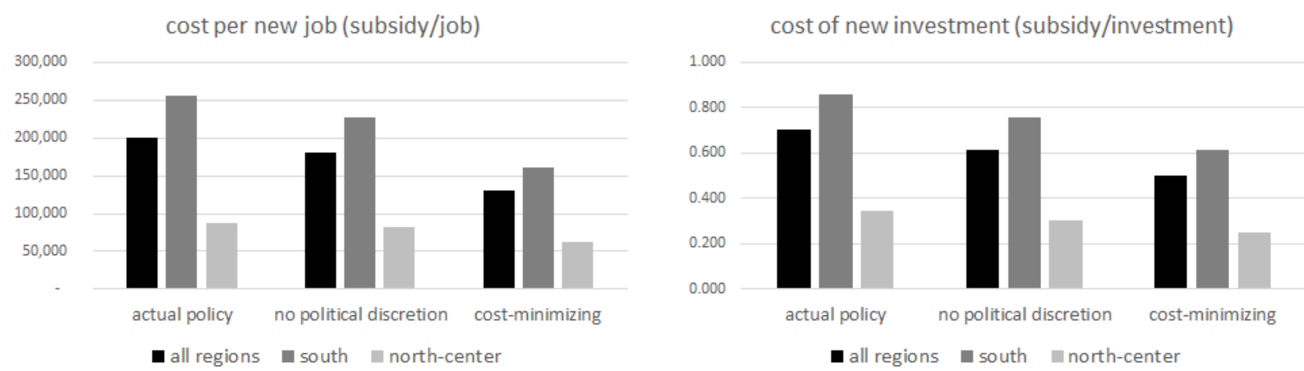
7 Conclusions

Governments around the world are investing trillions of dollars to help private business in the wake of the COVID-19 pandemics (Romer & Romer 2021). However, the effects of these policies may vary widely depending on the criteria used to allocated funds: policies effectively targeting

[17] Appendix Table A.5 provides the exact numbers underlying the graphs in Figure 17, and shows that they remain very similar when selecting the set of conditioning covariates according to the data-driven algorithm by Imbens & Rubin (2015).

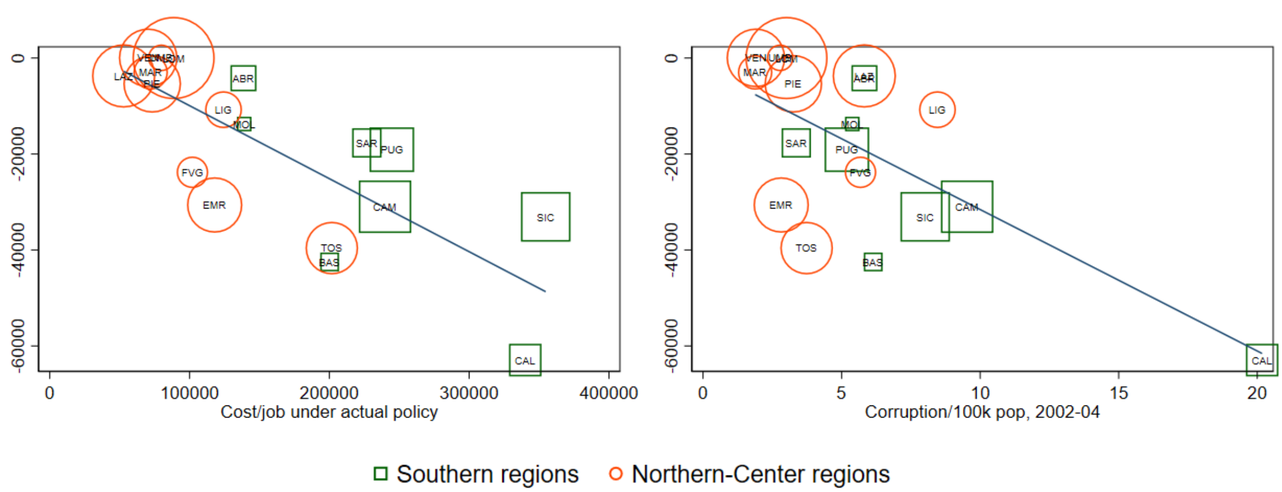
[18] Specifically, within each call we first select firms in the top decile of size, assigning highest rank to those falling in the 4th to 7th decile of age, second highest to large young firm (1st to 3rd decile of age) and third highest to large, older firms (8th to 10th decile of age). We then move to firms in the 9th decile of size, ranking fourth mid-age firms, fifth young firms and sixth older firms, and so on.

Figure 16: *Cost per newly created job and cost of new investment under the actual policy and under alternative allocation mechanisms*



Notes: These graphs show the cost per new job (left graph) and the cost of new investment (right graph) under the actual policy and under two alternative allocation mechanisms: eliminating the sub-component of the application score that was decided discretionally by politicians; and giving priority to applicants with lower cost of generating jobs and investment, respectively.

Figure 17: *Change in cost per job and cost of investment across regions when eliminating political discretion*



Notes: These graphs plot the change in the cost per job (left graph) and cost of additional investment (right graph) when allocating funds under an alternative allocation criterion free of political discretion, against the cost of generating jobs and investment under the actual policy across Italian regions. The size of markers is proportional to region population.

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 leverage quasi-experimental variation across a sub-population of Italian firms that obtained and did not obtain public subsidies from the government but were on average identical in all other respects. This approach, which is typical of policy evaluation studies, guarantees the internal validity of estimates. On the other hand, it does *not* allow us to characterize the heterogeneity of treatment effects across applicant firms, the total effects of the policy, and the effects of alternative allocation criteria.

We address these important issues using the RD design as a test for the (usually untestable) assumption of conditional independence. Using this approach, we uncover interesting heterogeneities in the effects of public subsidies across different types of firms – namely, smaller firms attain larger relative increases in employment, but older firms generate more jobs at a lower cost, and younger firms generate more employment than older firms upon receiving the subsidy. Under somewhat stronger assumptions, we can integrate such effects across different subsets of potential beneficiaries, in order to compare 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 about 10 percent.

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A Additional figures and tables

Table A.1: *List of calls in the L488 data (statistics for ordinary rankings only).*

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: The table reports information on the calls included in the L488 data available from the Ministry of Economic Development. The original data did not include 5 of the 35 calls (21, 24, 25, 26, 30), while for 4 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 such 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 isl ands, whilst call 7 was reserved to Veneto, Marche, Emilia-Romagna, Liguria, and Umbria. Fianlly Call 35 was reserved to a subset of firms in the province of Salerno.

Figure A.1: *Total L488 funds by year and geographical area*

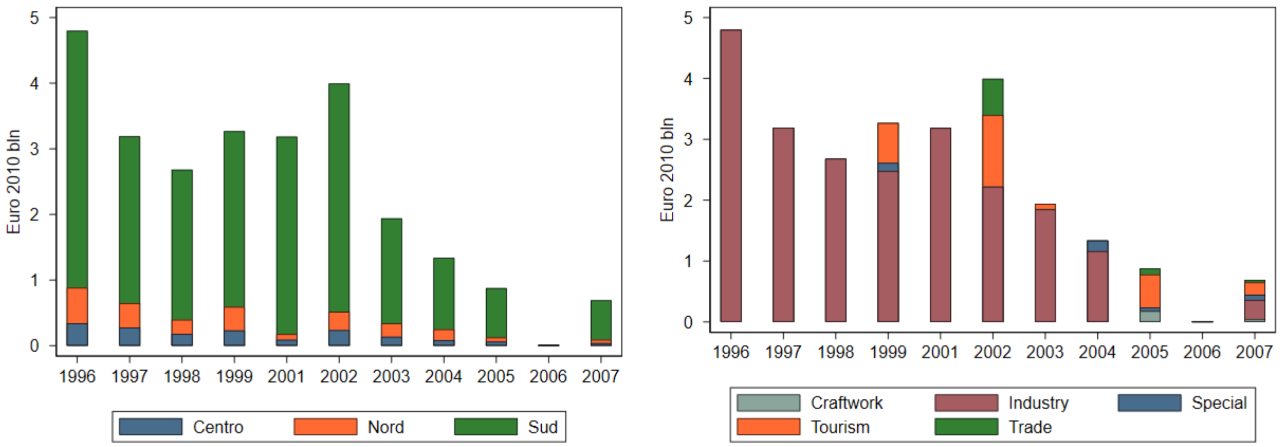


Figure A.2: *Total L488 funds by region, economic sector, and source*

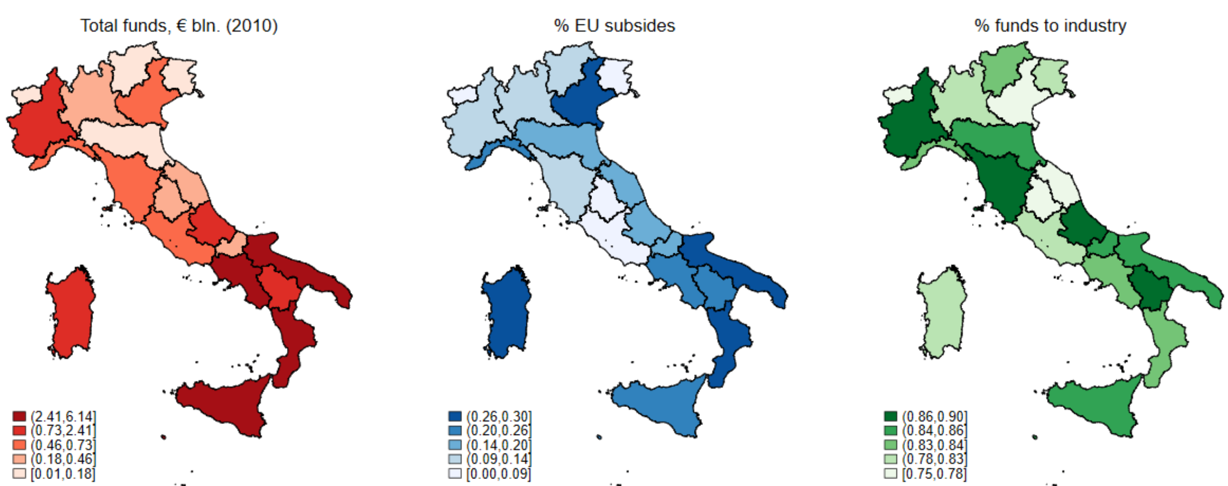
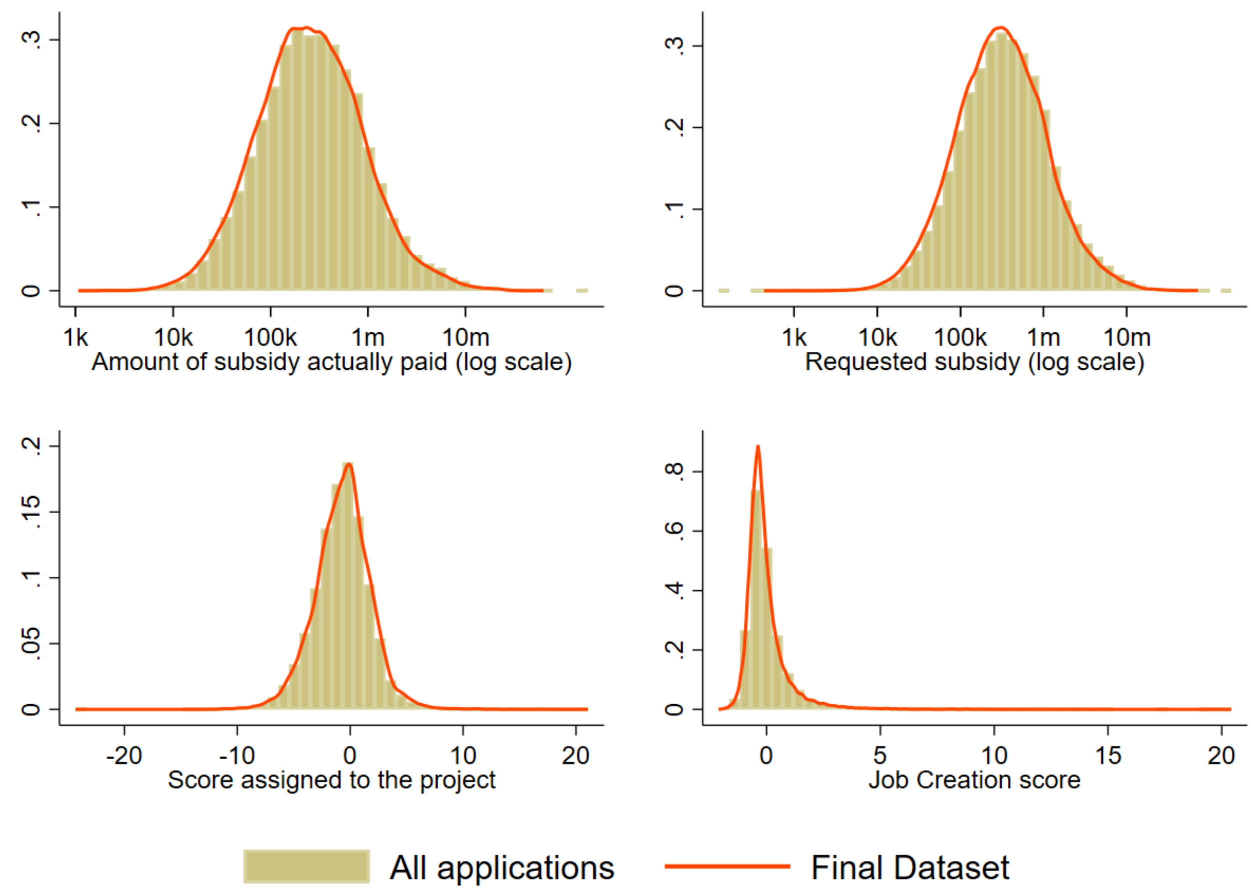


Figure A.3: *Distribution of selected variables across all applications and within the sub-sample of matched applications*



Notes: This figure shows the distribution of some applicant-level 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 A.2: *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.	0.017	0.02	0.026
	(0.043)	(0.034)	(0.04)	(0.034)	(0.048)	(0.04)	(0.048)	(0.04)
	[0.919]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
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)
	[0.919]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
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)
	[0.919]	[1]	[0.513]	[1]	[0.301]	[0.991]	[1]	[1]
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)
	[0.919]	[1]	[0.78]	[0.567]	[0.556]	[0.554]	[1]	[1]
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)
	[0.919]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
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)
	[0.575]	[1]	[0.918]	[1]	[1]	[1]	[1]	[1]
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)
	[0.795]	[1]	[0.235]	[0.476]	[0.092]	[0.181]	[0.365]	[0.433]
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)
	[0.919]	[1]	[0.918]	[1]	[1]	[1]	[1]	[1]
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)
	[0.737]	[1]	[0.298]	[0.91]	[0.23]	[0.335]	[0.365]	[0.426]
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)
	[0.82]	[0.975]	[0.78]	[0.752]	[1]	[1]	[1]	[1]
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)
	[0.919]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
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)
	[0.819]	[1]	[0.863]	[1]	[1]	[1]	[1]	[1]

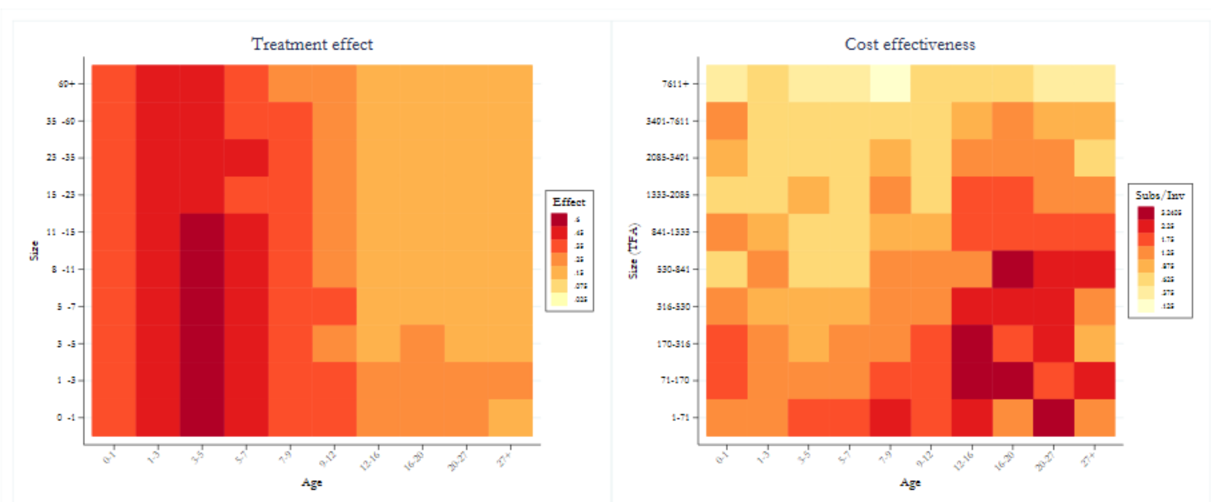
Notes: This table compares firm characteristics one year before the call between applicants scoring just above and just below the cutoff. Specifically, the table reports the estimated coefficients from RD regressions analogous to (4.1) in which the dependent variable is the firm characteristic indicated 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) include in addition 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 call-region are reported in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively. P-values adjusted for multiple hypothesis testing using the Westfall-Young procedure are also reported in square brackets.

Table A.3: *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 for employment growth in the 6 years after the application for L488 funds, regressed on the running variable separately for the sub-samples of observations to the left and right of the cutoff. The top panel shows the estimated coefficients when controlling for call fixed effects and for the vector of covariates $X^\star$, while the bottom panel reports the estimated coefficients when controlling only call fixed effects. The set of covariates $X^\star$ includes firm age (5 classes), deciles of lagged employment growth, and their interaction; average wage of white collar workers (deciles) and two indicators for having managers or apprentices in the payroll; and (deciles of) 3-year employment growth of all other firms in the same LLM and 3d industry. All these variables are interacted with a proxy for the size of the project financed thanks to the subsidy, obtained combining information contained in the application: the requested amount of subsidy and the firm top-up with own funds. Results are robust when we set $k = 2$.

Figure A.4: *Treatment effect and cost of investment by size and age of firms*



Notes: This figure shows the heterogeneity in the treatment effect of the subsidy on firm investment (left graph) and cost effectiveness (right graph) by deciles of age and size. The treatment effect for each group (Age=a, Size=s) is estimated as $E[Y^1 - Y^0 \mid \text{Age} = a, \text{Size} = s] = (\beta_1 - \beta_0) \cdot E[X \mid \text{Age} = a, \text{Size} = s]$, where β_1 and β_0 are estimated, respectively, from equations (4.5) and (4.6) where Y is the log of cumulated investment over 5 years. The covariates included in X are identified using the Imbens & Rubin (2015) procedure. In the right graph, cost effectiveness is measured by total subsidies over the amount of cumulated investment generated by the policy in each cell.

Table A.4: *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
(1y past) empl. growth	0.129	0.355	-0.29
(3y fwd) empl. growth in ind.X area cell	0.019	0.011	0.043
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

Table A.5: *Cost of creating new jobs and investment under actual and counterfactual policies*

	actual policy		no discretion		labor cost-minimizing	
	manual	data-driven	manual	data-driven	manual	data-driven
Panel A. Cost per new job (subsidy/job)						
all regions	200,830	200,959	181,519	179,622	130,284	132,116
south	255,722	234,522	227,988	208,987	160,549	149,453
north-center	88,245	108,592	82,520	99,576	62,623	82,914
Panel B. Cost of new investment (subsidy/investment)						
all regions	0.703	0.632	0.617	0.557	0.502	0.462
south	0.861	0.784	0.760	0.694	0.615	0.573
north-center	0.348	0.304	0.302	0.265	0.249	0.223

B Construction of sub-rankings of L488 applications

As explained in Section 2, the final ranking of L488 applicants depends on up to five main criteria – *skin in the game*, *job creation*, *no waste*, *political discretion*, and *environmental responsibility* – as well as on four additional criteria: (1) firm size; (2) activity in the service sector; (3) eligibility to receive EU funds; (4) the 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 to specific categories of firms (criteria 1-2) or by making additional EU funds available for specific types of projects (criteria 3-4).

Firm size. Each region had to reserve 50% of the budget it received from the national government to small and medium enterprises.[19] As soon as half of the budget was allocated to, say, large firms, only small/medium firms could be selected, leapfrogging higher scoring large firms in the official ranking.

Figure B.1: *Extract of the ranking published on 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		0 7300000	0 0103263	1 1111111	0 7150427	0 4611723	-0 2334272	0 94278780		M	1	S	A		N	994 62
81	90303		0 7300000	0 0101891	1 1111111	0,7150427	0 4451254	-0 2334272	0,92674090		M	1	S	A		N	326,55
82	15165		0 7356807	0 0068273	1 1764706	0,7428728	0 0519287	0 1297118	0,92451330		M	1	S	A		N	2.545,50
83	8219		0 6347110	0,0103466	1,1904762	0 2482163	0 4635466	0,2075272	0 91929010		P	1	S	A		N	200,79
84	38337		0 7450000	0,0062719	1,1764706	0 7885286	-0 0130310	0,1297118	0,90520940		P	1	S	A		N	670,38
85	45619		0 7480000	0,0053331	1,1904762	0 8032257	-0 1228332	0,2075272	0,88791970		P	1	S	A		N	1 358,73
86	64729		0 7037122	0,0041324	1,2500000	0 5862572	-0 2632673	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	0 0000000	1,2500000	1,0579768	-0,7465935	0,5382429	0,84962620		G	1	S		4		0,00
90	75995		0,5698718	0 0213675	1,0000000	-0,0694347	1,7525530	-0,8507631	0,83235520		M	1	S	A		N	833,16
91	50826		0,5441235	0 0063659	1,3333333	-0,1955771	-0,0020367	1,0012447	0,80363090		G	1	S		4		0,00
92	7939		0,7540000	0 0029062	1,2195122	0,8326201	-0,4066839	0,3688519	0,79478810		P	1	S	P	1	N	2.390,31
93	1707		0,5388774	0 0095278	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, in this order: 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 eligibility of the firm 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*.

An example of this crowding out effect is illustrated in Figure B.1, reporting a ranking belonging to the second call as published in the Official Journal. The projects are sorted in decreasing order according to the final score (in col. L). Looking at funds allocation (col. T) reveals that the

[19]Small and medium enterprises must have fewer than 250 employees and a turnover smaller than €50 million or a balance sheet total smaller than €43 million.

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, respectively, while the other two were submitted by large firms (see col. N: "G" stands for large, "M" for Medium and "P" for small). Had these projects been selected for funding, the 50% quota reserved to small and medium-sized firms would have been violated.

Activity in the service sector. Each region had to subsidize firms operating in the services industry within the limit of 5% of the budget. Therefore, it could happen that a project was 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 B.2.

Figure B.2: *Extract of the ranking published on 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, in order, 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 eligibility of the firm 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 (col. 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 col. P, where "S" stands for service provider).

Eligibility to EU funds. L488 was co-funded by EU structural funds (European Regional development Funds, EDRF). This means that projects that met certain characteristics were eligible for EU funding and could tap on the so-called "co-financed" portion of the budget. The other projects where eligible for the "national" portion only. [20] Hence, the former projects could be selected for funding even if the national funds had been exhausted, leapfrogging higher score projects eligible for national funds only.

Figure B.3: *Extract of the ranking published on 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 call 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 that 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, in order: 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 eligibility of the firm to get funded with EU funds (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 B.3. The projects ranked 171st and 172nd (IDs 40416 and 12997, respectively) were both presented by small firms. However, only the second, lower scoring project received funding. The reason is that 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 col. O; the "C" in col. R indicates the source of funds received were co-financed – "N" when national).

EU Objective Area. Even if firms proposed projects eligible to EU funding, they might be subject to a constraint on the type of ERDF program. This could happen in the case of firms in Northern or Central regions, which could tap on either Objective 2 resources (if located in areas in industrial decline) or Objective 5b, resources (if in disadvantaged rural areas). The amount of funds devoted to each type in the same region would typically be different.[21] Figure B.4 illustrates the case of projects being funded even if ranked lower than non-funded projects because the firms submitting them were eligible to different ERDF types due to their location..

[20]The eligibility criteria are required to fulfill the EU State Aid regulation and concern the characteristics of the beneficiaries, their location and type of activities, the type and duration of the investment investment to be financed, and the amount of eligible expenses.

[21]From 2000 onwards the EU slightly modified this distinction because some of the areas in the North-Central met the requirements to be left out of the program. Those areas were labelled as areas in "transitional support", whereas the remaining ones used the old label Objective 2.

Specifically, in the reported part of the ranking, 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 B.4: *Extract of the ranking published on 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	1 270,80
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, in this order: 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 eligibility of the firm 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*.

In other words, in our analysis 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

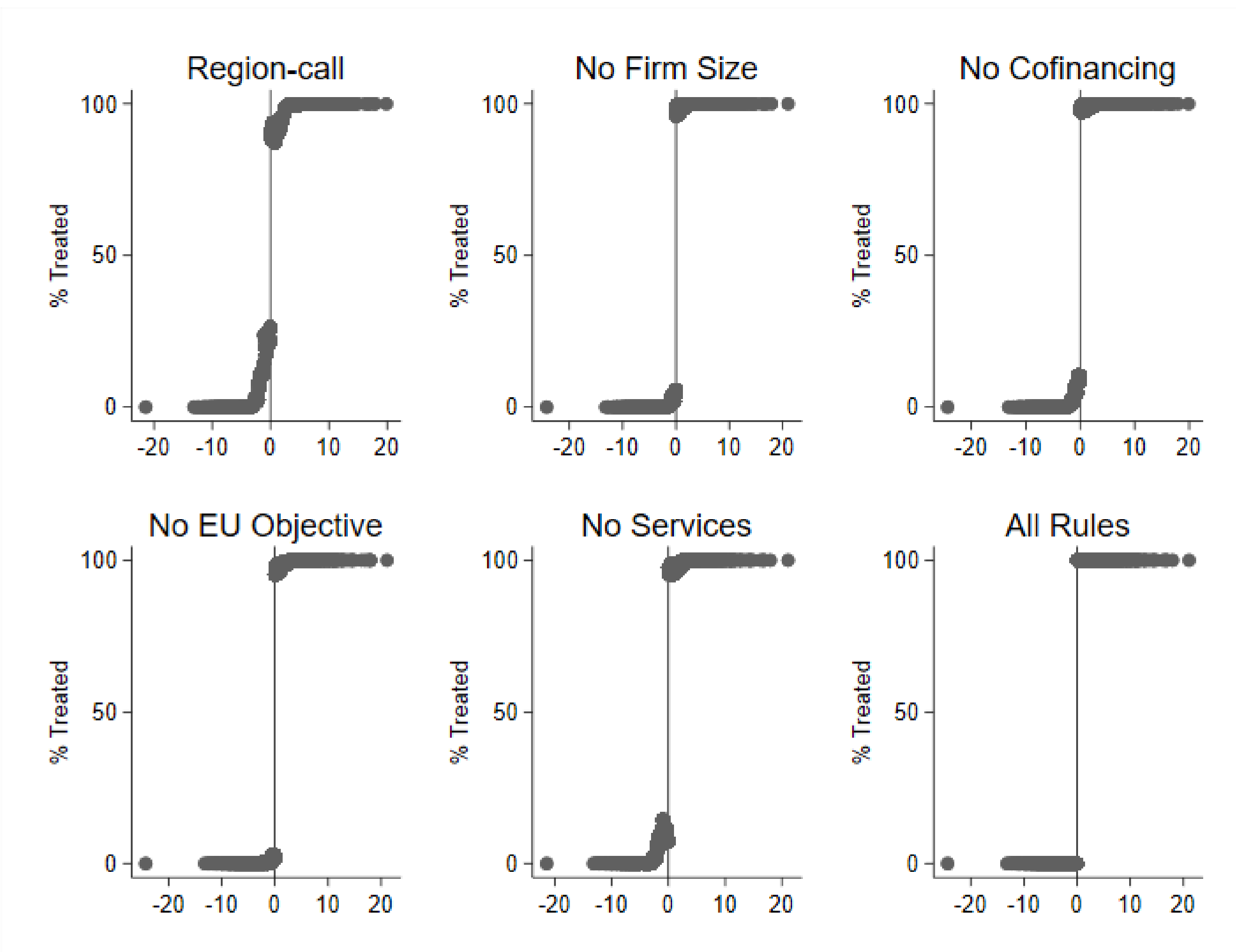
(5) eligibility to EU funding – there is one ranking for eligible firms and another for those not eligible

(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"

For further clarification, an example of a ranking 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.

In summary, if we had used the official rankings published in the Official Journal - i.e. one ranking per region call - we would not have uncovered the discontinuity in the assignment probability at the cutoff (top left panel in Figure B.5). Instead, 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 B.5). Moreover, the other panels in Figure B.5 show that each of the four dimensions described above (in addition to call and region) is essential to recover the sharp discontinuity in the assignment to treatment. Whenever we do not consider one of them, we fail to recover the discontinuity at the pooled cutoff.

Figure B.5: *Comparison of assignment to treatment rules.*



C The determinants of the political score

In Table C.6, we investigate the determinants of political preferences for different applicant firms. To this purpose, we regress, across applicant firms in all calls, 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. Specifically, we classify local governments' 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 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 dummy for political alignment between the regional and municipal government.[22] Columns (4)-(6) replicate the same specifications in (1)-(3) but weight observations by the municipal population.

D RD estimates at the cutoff: Additional results

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

Figure 10 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 $t + \Delta t$ reported in Table 2 and Figure 9 reflect both the direct effect of the subsidy obtained at time t and the indirect, negative effect of receiving less subsidies between t and $t + \Delta t$. Hence, they are likely to 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 $t + \Delta t$ is itself a causal effect of the subsidy received at time t. In terms of external validity, however, we may want to isolate the direct from the indirect effect, as only the former would apply in the context of one-off interventions.

We thus extend the estimating equation (4.1) to allow for dependence of firm outcomes on

[22]The source of all these data are the administrative registries of local politicians and elections that are publicly available from the website of the Italian Ministry of Interior (https://dait.interno.gov.it/). We borrowed the classification of local governments' ideology from the Local Opportunities Lab (https://www.localopportunitieslab.it/).

Table C.6: *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	NO	NO	NO	YES	YES	YES
R-squared	0.177	0.177	0.177	0.090	0.090	0.090

Notes: This table investigates the determinants of political preferences for applicant firms, as estimated from 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 unit of observations 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, respectively.

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.[23] With repeated interventions, the causal effect of the subsidy received in period $t = 1$ on the outcome in period $t = 2$ would read:

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

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. The following regression would be suitable to properly estimate $\tilde{\tau}_2$ – the causal effect of D_1 on the outcome in $t = 2$:

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

the operational problem being that τ_2 is unknown. However, an estimate could be recovered from a regression analogous to (D.1), run on firms participating to the call issued in period $t = 2$ but not to the previous call.

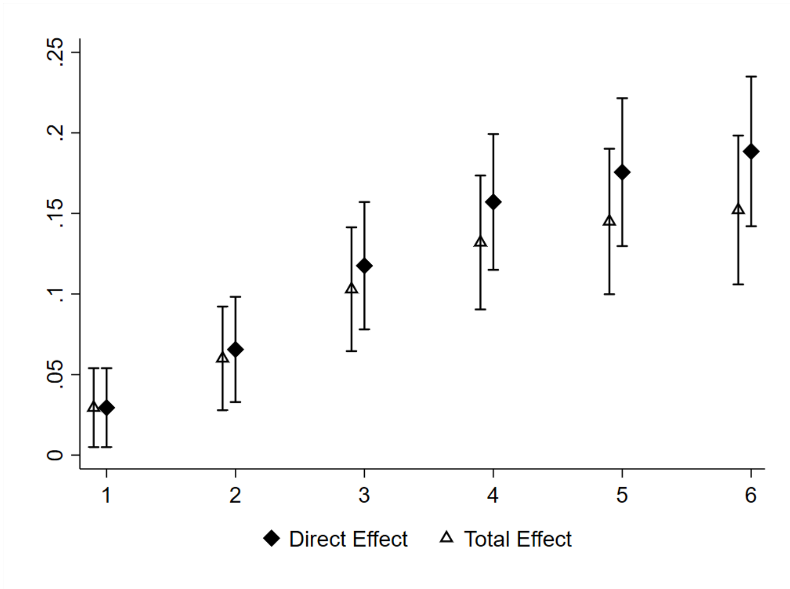
In practice, with calls issued across several subsequent years, we estimate (D.1) allowing for year-specific coefficients τ_1^t with $t = 1996, ..., 2006$, in a sample including all firms at their first application. Year-specific contemporaneous coefficients are then used to "net" outcomes of firms applying in two consecutive years: $\tilde{Y}_2 = Y_2 - \tau_1^t D_2$.[24] 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 D.1 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 9, with the direct effect obtained by subtracting the effect of subsequent subsides, estimated following the above described procedure. As expected, in light of the evidence in the right graph of Figure 10, 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.

[23] 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.

[24] 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 D.1: *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 9 (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).

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 impact more 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:

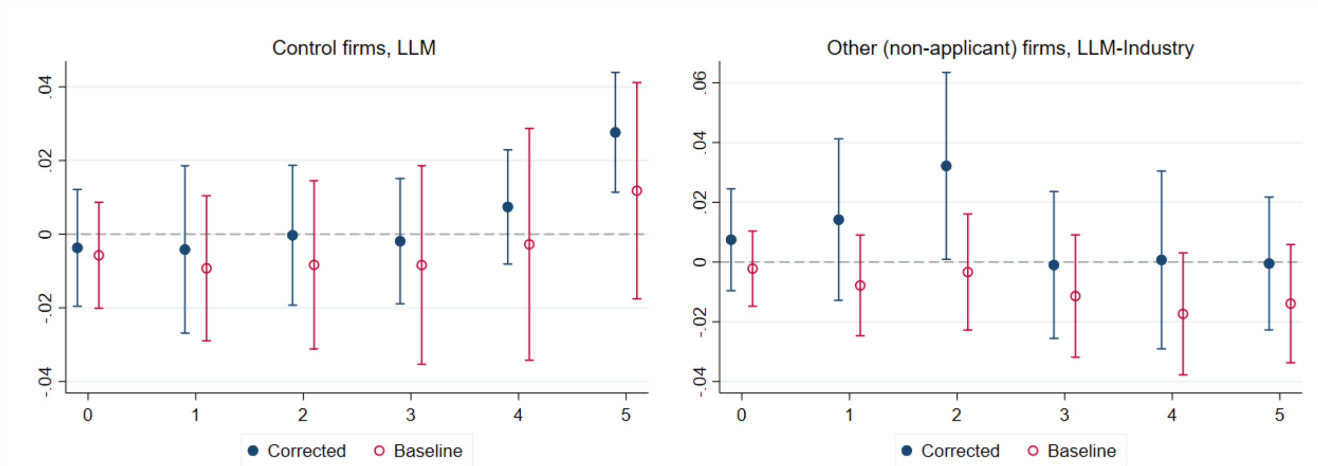
$$\ln L_{m,t+k} - \ln L_{m,t} = \theta_k D_{m,t} + \alpha \ln L_{m,t} + FE_m + FE_t + \epsilon_{m,t} \tag{D.3}$$

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, respectively, as available 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, respectively; and $\epsilon_{m,t}$ is a residual summarizing the effect of other factors. The coefficient on 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 D.2 plots the estimated of coefficients θ_k's for two different subset 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.[25] Both graphs present baseline difference-in-differences estimates as well as "corrected" estimates accounting for staggered research, using the approach suggested by de Chaisemartin & D'Haultfœuille (2020).[26] 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 D.3. These results imply, among other things, that employment increases in subsidized firms do not represent a mere reallocation of jobs from non-subsidized firms.

Figure D.2: *Spillover effects on other firms in the same labor market (binary indicator for subsidized firms)*

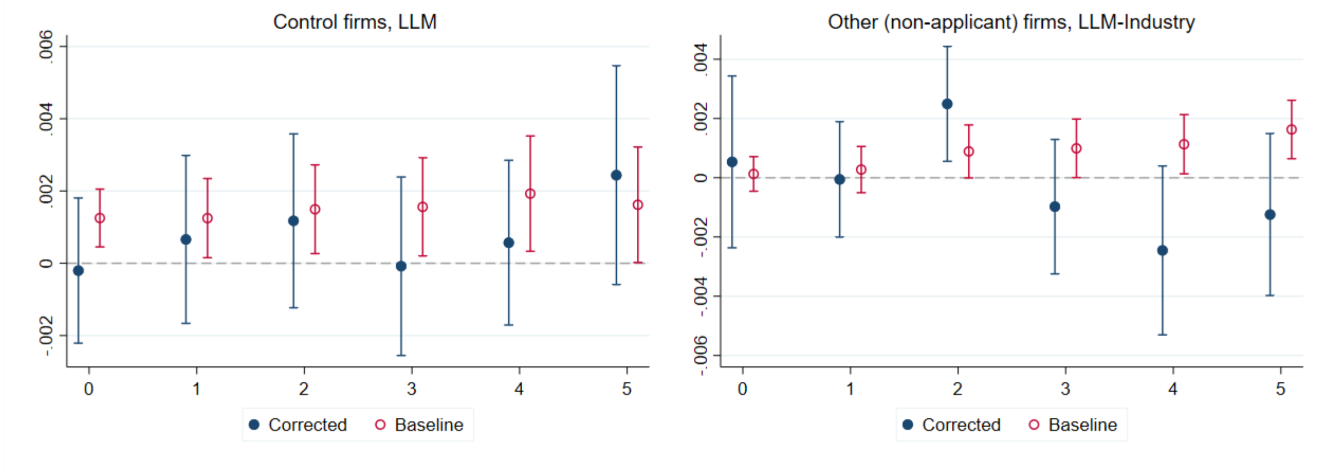


Notes: The graphs show the estimated spillover effects (and associated confidence intervals) of the subsidy on local employment at different time horizons, indicated on the horizontal axis. The left panel plots the aggregate employment response of control firms located in the same LLM as treated firms. 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.

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

[26]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 D.3: *Spillover effects on other firms in the same labor market (log of total subsidies)*



Notes: The graphs show the estimated spillover effects (and associated confidence intervals) of the subsidy on local employment at different time horizons, indicated on the horizontal axis. 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 X 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 for selecting 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 $j = 1, \ldots, k_2$,

$$Y = \sum_k \gamma_k^0 S^k + \sum_k \delta_k^0 D \cdot S^k + \mathbf{z}'\tau^0 + w_j \mu_j^0 + FE_c^0 + \nu^0, \quad \text{if } -h \leq S < 0,$$
$$Y = \sum_k \gamma_k^1 S^k + \sum_k \delta_k^1 D \cdot S^k + \mathbf{z}'\tau^1 + w_j \mu_j^1 + FE_c^1 + \nu^1, \quad \text{if } 0 \leq S \leq h, \tag{E.1}$$

   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

$$H_0^{(L)} : \gamma_{L,1} = \cdots = \gamma_{L,k} = 0 \quad \text{and} \quad H_0^{(R)} : \gamma_{R,1} = \cdots = \gamma_{R,k} = 0.$$

   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 $\mathcal{F}^R = \{F^{1,R}, F^{2,R}, \ldots, F^{k_2,R}\}$, respectively. 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.

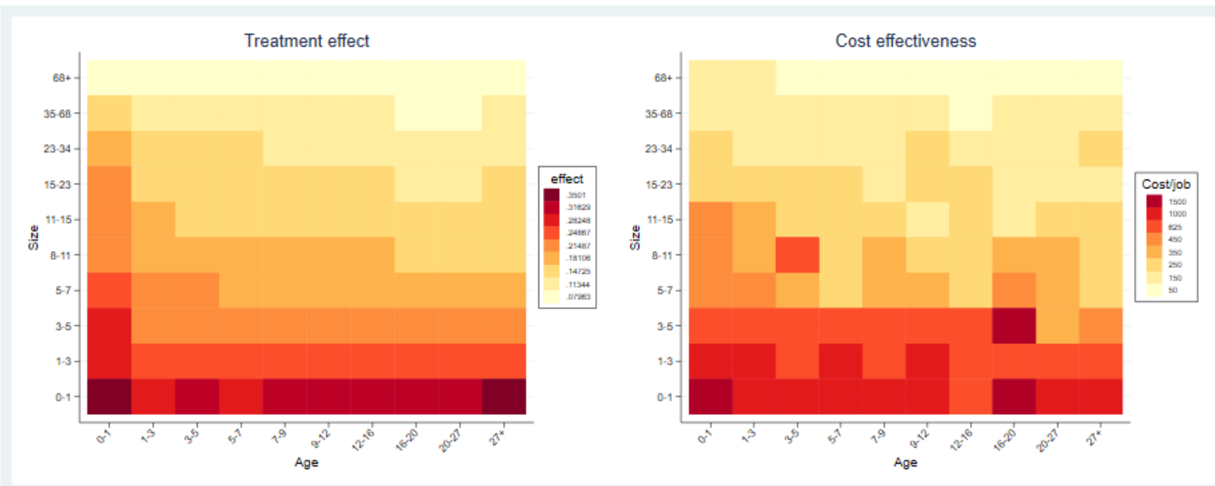
5. Repeat step (d) until one of the following stopping criteria is reached:

   - 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.[27]

[27] 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.

Figure E.1: *Treatment effect and cost per job by size and age of firms*



Notes: This figure shows the heterogeneity in the treatment effect of the subsidy on firm employment growth (left graph) and cost effectiveness (right graph) by deciles of age and size. The treatment effect for each group (Age=a, Size=s) is estimated as $E[Y^1 - Y^0 \mid \text{Age} = a, \text{Size} = s] = (\beta_1 - \beta_0) \cdot E[X \mid \text{Age} = a, \text{Size} = s]$, where β_1 and β_0 are estimated, respectively, from equations (4.5) and (4.5). The covariates included in X are identified using the Imbens & Rubin (2015) procedure. In the right graph, cost effectiveness is measured by total subsidies over the number of newly created jobs in each cell. The number of new jobs is computed multiplying the (average) percent treatment effect estimated for each firm by its size, and aggregating across all firms in each cell.