Making Subsidies Work: Rules vs. Discretion[*]

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Abstract

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

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1 Introduction

Public subsidies to firms represent a substantial and growing portion of public expenditures worldwide. Prior to the Covid-19 outbreak, the United States allocated a total budget of $61 billion annually towards place-based policies, with 80% of this amount being dedicated to cash grants and tax credits for firms (Bartik, 2020). Similarly, between 2014 and 2020 the European Union's Regional Development Fund (ERDF) dedicated €279 billion (€46.5 billion per year) to bolster economic development in less prosperous European regions. Unsurprisingly, government support increased further in the aftermath of the Covid-19 pandemic. In the nine OECD countries included in the project 'Quantifying Industrial Strategies', government grants and tax expenditures surged from approximately 1.5% of GDP in 2019 to 3% in 2021 (Criscuolo, Díaz, Lalanne, Guillouet, van de Put, Weder and Deutsch, 2023).

The increase in industry subsidies budgets has sparked renewed interest in understanding their impact. However, there remains significant uncertainty regarding the specific effects of subsidies on different types of recipients (Juhász, Lane and Rodrik, 2023). For instance, there is a belief that small or emerging firms tend to derive the highest benefits from public capital, which aids them in overcoming liquidity constraints (see e.g. Chodorow-Reich, 2014; Criscuolo et al., 2019; Siemer, 2019). On the other hand, market frictions may cause larger, more established producers to miss out on potential investment opportunities (see e.g. Hsieh and Olken, 2014; Akcigit et al., 2020).

Against this backdrop, discretionary power by bureaucrats and politicians can potentially enhance rigid policy rules by incorporating additional information about the quality of firms and projects into subsidy allocation; or, it can help direct public support towards disadvantaged areas and groups. On the other hand, discretionary power increases the risk that subsidies serve private benefits rather than the public interest, as exemplified by cases involving political connections (see, e.g. Fisman, 2001). This "rules vs. discretion" dilemma is a longstanding concept in macroeconomic policy (Persson and Tabellini, 2002) and extends to various areas of government intervention, including industrial policy (Laffont, 1996).

In this paper, we examine the relevance of firm characteristics and allocation criteria, with a specific focus on the rules versus discretion trade-off, to assess the effectiveness of public subsidies provided to private firms. Specifically, we investigate the impact of Italian Law 488/92 (henceforth L488/92), which stands as the largest program of investment subsidies ever implemented in Italy and one of the largest in Europe (Giavazzi, D'Alberti, Moliterni, Polo and Schivardi, 2012). Over a period spanning from 1996 to 2007, L488/92 financed a total of 77,000 investment projects through 35 open calls with a total budget of nearly €26 billion (at constant 2010 prices), partly sourced from the EU's ERDF.

To assess the impact of these funds on firm investment, job creation, and productivity, we need to tackle three key challenges commonly encountered in policy evaluations. First, to ensure *internal validity*, we must compare subsidized and non-subsidized firms that are similar in all aspects, except for the subsidy receipt. Second, the effect of the subsidy may be *heterogeneous* across firms. Third, and relatedly, it can be challenging to interpret the *external validity* of estimates in different contexts or when different allocation criteria are considered.

These objectives involve important trade-offs. To establish internal validity, we typically estimate average treatment effects among a subset of "compliers" with plausibly exogenous variation. However, relying solely on average treatment effects might mask substantial heterogeneity, and restricting the analysis to small sub-populations of compliers may severly limit the external validity of the estimates. These limitations hinder our ability to evaluate the effectiveness of alternative allocation schemes, which would be valuable for policy evaluation purposes.

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

L488/92 subsidies were allocated through open calls for projects. The total budget of each call was preliminarily assigned to the 20 Italian regions, with a preference for economically underdeveloped regions in the South. Firms could then submit tenders to fund specific investment projects, which were ranked within each call-region based on a numerical score of project quality. Funding was granted on a first-ranked, first-served basis until the available funds were fully allocated. Importantly, the numerical score was determined by two main components. The first component focused on objective criteria ("rules"), while the second component involved regional priorities indicated by local politicians ("discretion"). Using machine-learning methods, we show that the discretionary component of the overall score tends to favor projects submitted by applicant firms that are smaller, demand larger subsidies, and are located in relatively disadvantaged areas, compared to firms that would be selected solely based on objective criteria.

The allocation mechanism of L488/92 entails an ideal RD design. We find that applicant firms submitting projects that scored just above the cutoff increased investment by 43 percent during the three-year subsidy period, compared to applicant firms just below the cutoff that did not receive the subsidy. This temporary boost in investment resulted in an average 11 percent increase in employment for these firms during the same period. Importantly, employment growth continued after the end of the subsidy period, reaching 17 percent six years after being awarded the subsidy. Taking into account spillover effects within local labor markets, we demonstrate that the expansion of subsidized firms did not come at the expense of non-subsidized firms. Therefore, our estimated effects indicate a net increase in local employment. Revenues and value added exhibit a similar change, implying that firm productivity remained approximately constant. Over the same six-year horizon, firm survival increased by 3 percentage points (+6 percent above the baseline).

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

Given the conditioning on X, we can estimate the range of treatment effects across the application score, which allows us to assess the overall effectiveness and cost-efficiency of the policy. The estimated cost-per-new-job over a time horizon of six years stands at €178,000, with a stark divide between Northern and Southern regions – €68,000 and €241,000 per job, respectively. The cost of investment shows a similar gradient, as each Euro of subsidy generates nearly three Euros of investment in the North, but only one Euro in the South.

We then show that not only the overall score achieved by the project, but also its two components that summarize the objective indicators and the discretionary evaluation by local politicians, are irrelevant for the outcome when conditioned on X. This finding allows us to examine the variation in treatment effects across these two sub-scores. Successful applicants ranking high on either dimension generate the largest percent effect on employment upon receiving the subsidy. The effect rises from +11% among applicants in the first quintile of both sub-scores to 16% for those in the top quintile of either sub-score, and further to 19% for applicants in the top quintile of both sub-scores. However, the same relative increase translates into a lower *number* of jobs-per-euro-of-subsidy for applicants with high discretionary scores compared to those with high rule-based scores. This discrepancy arises because, on average, firms with high discretionary scores are smaller in size, so the same percent increase corresponds to a lower number of new jobs. Additionally, these firms demand larger subsidies than other applicants.

To assess the economic implications of political discretion, we compare two counterfactual policies. The first policy disregards the subjective preferences of local politicians, thus excluding the discretionary component from the score used to rank applicants, while the second policy relies exclusively on the preferences of local politicians. Under each policy, we reevaluate and rank applicants based on the new criteria and then calculate the cost-per-new-job by considering the treatment effects for the set of firms that would be funded under the respective counterfactual ranking. Throughout this analysis, we assume that firms' decisions to apply for L488/92 funds and the projects they submit are not influenced by the specific criteria used to award subsidies. While apparently strong, this assumption is supported by empirical evidence, which shows that both applicants and their projects exhibit similar characteristics between the initial two calls for projects, where political discretion was not a part of the selection criteria, and the subsequent two calls immediately following the introduction of political discretion.

In the absence of political discretion, the cost per new job and the cost of new investment would decrease by 11% and 13% respectively, compared to the actual policy. Conversely, if we solely relied on political discretion, the cost of job creation and additional investment would increase by 42% and 22% respectively.[1] Under both counterfactual policies, the impact of political discretion is particularly detrimental to the economically disadvantaged Southern regions. Additionally, we also computed the optimal ranking of applicant firms based on their estimated treatment effects. If this alternative criterion were to be adopted, it would result in reducing the cost per new job by more than half. Once again, the greatest benefits from this approach would be seen in the Southern regions.

These results contribute to a vast body of literature that examines the causal impact of public subsidies on investment, employment, and overall economic activity. The seminal paper by

[1] Prior to this exercise, we ensure that the discretionary score does not respond to the objective score (see Section 2).

Hall and Jorgenson (1967) estimates significant effects of investment subsidies in the United States during the 1950s and 1960s. More recent research has focused on fiscal policies that target disadvantaged areas (e.g., Greenstone et al., 2010; Bloom et al., 2019) or aim to stimulate economic recovery after recessions (e.g., Wilson, 2012; Chodorow-Reich et al., 2012), with findings consistently indicating positive impacts on employment and output. Many of these previous studies assess policy effectiveness using the cost-per-new-job metric and report figures that align closely with our findings. For an overview of recent surveys on this topic, see Chodorow-Reich (2019), Bartik (2020), and Slattery and Zidar (2020).[2] Turning to Europe, Becker, Egger and von Ehrlich (2010) and Becker, Egger and Von Ehrlich (2013) assess the impact of the ERDF, which also contributed to the budget of L488/92, across different European regions. Their findings indicate that eligibility for additional funds through the ERDF results in a 1.6 percentage point increase in GDP growth. However, they do not find a significant effect on employment. D'Amico (2021) highlights the importance of political economy constraints for the allocation of transfers across EU regions.

These papers rely on aggregate, regional-level data, and there is limited firm-level evidence on the direct effects of public subsidies on firm investment and employment. Notable exceptions include Criscuolo, Martin, Overman and Van Reenen (2019), who estimate a positive effect of the UK Regional Selective Assistance on firm investment and employment; and Bronzini and Iachini (2014), who find, instead, that R&D investment subsidies in a single Italian region were largely ineffective. More closely related to our work, two previous papers have evaluated the effects of L488/92 with firm-level data, finding opposing results. Using a difference-in-differences approach, Bronzini and de Blasio (2006) concluded that, rather than spurring a net increase in firm accumulation, the subsidy induced firms to anticipate planned investment projects. Using an RD approach, Cerqua and Pellegrini (2014) found positive net effects on investment and employment. They estimate a much lower cost-per-job than we do, ranging from €60,000 to €100,000 at 2010 prices compared to almost €200,000 in our case. The discrepancies between their findings and ours may stem from several factors.[3] Most importantly, compared to these papers we extensively characterize the distribution of treatment effects across different types of firms, and compute the

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

[3] For one thing, our study covers a much larger sample size, including data from almost all calls for projects and regions (over 40,000 projects submitted by 27,000 firms), while Cerqua and Pellegrini (2014) focused only on three calls for projects in six Southern regions (1,702 applicant firms in total). Moreover, the analysis factors in institutional rules and budgetary priorities that are crucial for a correct construction of the regression discontinuity design but were not accounted for by previous works. We discuss these issues in detail in Section 2, and in Section S1 of the Supplementary Materials.

cost-effectiveness of public subsidies under alternative allocation criteria, specifically considering the trade-off between rules and discretion.

This contributes to the growing literature on the impact of discretion on the effectiveness of public policies. In a field experiment conducted in Pakistan, Bandiera, Best, Khan and Prat (2020) find that shifting authority from monitors to procurement officers led to lower prices without compromising quality. In the Italian context, several papers have assessed the effects 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. Generally, greater discretion did not lead to worse observable procurement outcomes (Coviello, Guglielmo and Spagnolo, 2018), although the effects varied among different procuring agencies. Less transparent and less qualified agencies tended to select politically connected firms (Baltrunaite, Giorgiantonio, Mocetti and Orlando, 2018) or firms owned/run by individuals with a criminal record (Decarolis, Fisman, Pinotti and Vannutelli, 2020).[4] Our study differs from these previous ones because the institutional features of L488/92 provide an observable indicator of politicians' preferences, specifically through the sub-component of the applicant score determined by local politicians. This aspect allows us to estimate the effectiveness of the policy under different levels of discretion. This is particularly relevant in Italy, where political clientelism is widespread and the state plays a significant role in the economy (see, e.g. Golden and Picci, 2008; Cingano and Pinotti, 2013). However, it is worth noting that lower returns to public subsidies under discretion may arise if politicians prioritize equity objectives that may trade-off with economic efficiency (see, e.g. Kline and Moretti, 2014).

In the next section we describe the institutional context, and in Section 3 and 4 we introduce the data and empirical strategy. In Section 5 we show the results for marginal firms near the cutoff, and in Section 6 the results for inframarginal firms away from the cutoff, the heterogeneity of treatment effects, and the overall policy effect under alternative allocation criteria. Section 7 concludes.

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

2 Institutional framework

Italy has long been marked by significant economic disparities between its northern and southern regions. [5] In 2001, the median value added per capita in the northern regions (€18,500) was double that of the southern regions (€9,500). Additionally, economic activity within the southern regions exhibited significant variation, with a 90/10 ratio in value added across local labor markets reaching 3, compared to just 2 within the northern regions.[6] Concurrently, the past few decades have witnessed a notable decline in workers' mobility. By 2005, the one-year mobility rate in Italy was only one-third of that in the United States, ranking as one of the lowest among countries (Molloy, Smith and Wozniak, 2011).

The substantial regional disparities and limited labor mobility form a compelling case for targeted subsidies in spatial development (Kline and Moretti, 2014; Bartik, 2020). In the postwar era, the southern regions of Italy received substantial financial support from both the Italian government and the European Union.

Various project categories were eligible to receive L488/92 subsidies, encompassing industrial projects designed to establish, expand, or modernize facilities; projects related to energy, steam, or hot water production and distribution; construction sector projects; and, to a limited extent, IT sector projects (up to 5% of the program's total budget). Funds were distributed through open calls for tenders, each targeting specific economic sectors, primarily industry, but also tourism and trade. The funds allocated for each call were then distributed among the 20 Italian regions. Table 1 displays the allocation of L488/92 funds across sectors and regions over the entire 1996-2007 period. Industry received the largest share at €21.9 billion, followed by tourism at €2.7 billion. In line with the primary objectives of the policy, nearly 85% of the funds were directed toward less economically developed areas in the South. For instance, two of the poorest regions in the country, Campania and Sicily, received approximately €6 billion and €5 billion, respectively, in contrast to Lombardy and Emilia Romagna, which received €0.25 billion and €0.13 billion, respectively.[7]

Projects submitted by applicant firms within each call-region were subsequently evaluated and

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

[6] Local labor markets are defined as clusters of contiguous municipalities based on workers' commuting patterns by the Italian National Statistical Institute (ISTAT), similar to the commuting zones in the United States. For more details, refer to `https://www.istat.it/en/labour-market-areas`.

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

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

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

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

ranked, with subsidies granted until the available funds were exhausted. The rankings were determined by a combination of quantitative project quality indicators and regulations concerning the minimum allocation of L488/92 funds set aside for particular applicant categories (e.g., small to medium-sized enterprises) or eligibility for co-financing with EU funds.

2.1 Ranking projects on objective rules (1996-1997)

During the initial calls for projects in 1996 and 1997, three primary quality indicators were established:

(I1) Ratio of applicant's investment to the requested funds (*"skin in the game"*);

(I2) the number of jobs the project would generate (*"job creation"*);

(I3) Proportion of requested funds compared to a benchmark set by the EU Commission (*"no waste"*).

The first and third indicators emphasized the entrepreneur's commitment to their project, favoring those with greater dedication and involvement. In contrast, the second indicator aligned with L488/92's core objective: to boost employment. The necessary data to calculate these indicators was sourced directly from the funding applications. Subsequently, this information was conveyed to the Ministry of Economic Development by local branches of approved banks. These banks were also responsible for an initial evaluation of the project submissions.

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

$$S_{ir} = \sum_{j=1}^{3} \frac{(I_{ir}^{j} - \mu_{r}^{j})}{\sigma_{r}^{j}}, \tag{1}$$

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

2.2 Incorporating political discretion (1998-2007)

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

(I4) regional government-assigned points based on priorities (*"political discretion"*);

(I5) adherence to environmental management system like ISO 14001 or EMAS (*"environmental responsibility"*).

With the introduction of indicator I4, there was a pronounced shift towards federalism and decentralization, largely spurred by Law 59/1997. This law, commonly referred to as the "*Bassanini*" Law—named after the then Ministry of the Public Administration—mandated the central Government to delegate administrative tasks promoting regional growth to the 20 Italian Regions. These tasks encompassed directives related to economic and industrial activities, focusing primarily on the growth and development of various sectors such as manufacturing, commerce, agro-industrial operations, and production services.

In the context of L488/92, regional governments were vested with the power to allocate a score ranging from 0 to 10 to various municipalities within their jurisdiction. These scores were based on factors such as the proposed location of the project, the pertinent industrial sector, and the nature of the investment, like "new productions," "expanding existing outputs," "switching business activities," and so forth. Crucially, this point allocation, which was specific to each municipality, industry, and type of investment, needed to be predetermined. Moreover, these allocations were to be conveyed to the Ministry of Economic Development by the 30th of October in the year preceding each call for projects. The specifics of these allocations were kept confidential.

Indicator I4 was formulated as the standardized cumulative points earned across various dimensions, contingent on the project's location, industry, and investment type.

To calculate the overall project score for calls post-1998, we standardized and aggregated the new indicators, I4 and I5, using the formula defined in (1).[8] For the same time frame, the aggregate of the first three standardized indicators, I1-I3, is denoted as the sub-score for objective ”rules”, SR (alternatively, the ”objective sub-score”). Meanwhile, I4 is referred to as the sub-score for ”discretionary”, SD (or the ”discretionary sub-score”). As the I5 indicator for environmental responsibility doesn’t distinctly fall into either the objective or discretionary category, we exclude it from both SR and SD.

2.3 Ranking and funding of projects

To determine the fund allocation for each call for projects, applicants were ranked by their overall score S. This ranking was conducted region-wise and was influenced by additional rules that prioritized certain applicant and project categories. Specifically, there were three main rules:

1. Within each region, a minimum of 50% of the budget was set aside for small-medium enterprises. These are defined as entities with fewer than 250 employees and either a turnover of less than €50 million or a balance sheet total of under €43 million.
2. Up to 5% of the regional budget was allocated to service sector firms.
3. Projects that qualified for supplemental co-funding from EU structural funds were prioritized over those projects that only qualified for national funding.

A detailed examination of these rules can be found in Section S1 of the Supplementary Materials. Based on these guidelines, multiple sub-rankings were established within each regional ranking, as documented in the *Gazzetta Ufficiale*. By leveraging additional data on firm size, sector, co-financing eligibility, and geographical location—all of which is available in the *Gazzetta Ufficiale*—we discerned distinct ”*cells*” of firms vying for L488/92 funds within identical calls, regions, and potentially, specific applicant categories.[9]

The results of this selection process were made public within four months post the application deadline. Beneficiaries received their subsidies in three equal payments. The initial installment

[8] In certain calls, the fifth indicator (I5) was not simply combined with the others to determine the project’s final score. Instead, if a project met environmental certifications, I5 would enhance all other sub-scores by 5%, as illustrated by: $S_{ir} = \sum_{j=1}^{4} \frac{I^j_{ir} \times I^5_{ir} - \mu^j_r}{\sigma^j_r}$, where $I^5_{ir} = 1.05$ if the applicant i is compliant with environmental certification, and $I^5_{ir} = 1$ otherwise.

[9] Previous evaluations of L488/92 formed the RD design only by call and region. Our Supplementary Materials delve into the repercussions of ignoring special applicant categories and provide a clearer depiction of our method for accurately identifying applicant cells. Refer to Supplementary Material Figure S5 for more insights

was dispatched within two months after the rankings were published. The subsequent installments followed one and two years later, contingent on the project's progress. Specifically, the second installment was released only if at least 2/3 of the project was completed. The final installment was contingent on full project completion. Failure to meet these milestones meant forfeiting the remaining payments and possibly having to repay prior installments, along with potential fines. This rigorous monitoring ensured that executed projects aligned closely with their initial proposals.

3 Data

Our analysis capitalizes on a unique dataset, merging administrative records, firm registry data, and a proprietary balance sheet database. A comprehensive description is available in the Online Appendix Section 2.

The Italian Ministry of Economic Development supplied comprehensive information on all applications to 26 calls for L488/92 funds from 1996 to 2007. This encompasses 74,584 projects valuing nearly €22 billion (from the total €26 billion allocated by L488/92), submitted by 49,082 firms. Additional insights are available in Online Appendix Table S2.[10]

For each project, the dataset reports the fiscal identifier of the applicant firm, together with its location and sector; the subsidy requested; the applicant's final score and its components (I1-I3 in 1996-97, and I1-I5 after 1998); and the amount eventually awarded to applicants scoring above the cutoff. We enhanced this dataset with the *Gazzetta Ufficiale*, helping pinpoint the specific competition "cell" for every applicant.

Nearly 33,000 projects scored above the threshold and were thus eligible. About 20% of these projects were not funded, eventually, for a number of reason.[11] Our dataset unfortunately lacks specific reasons for each declined subsidy. As explained in the next section, we will focus on the effect of being eligible for the subsidy (i.e., scoring above the cutoff), which provides a lower bound to the effect of actually receiving the subsidy. In any event, we will discuss the implication of (non-random) selection into receiving the subsidy among eligible applicants whenever it is relevant for interpreting our results – particularly, when comparing the effect between applicants

[10]Our dataset misses data from 5 out of the 35 calls (21, 24, 25, 26, 30). Moreover, for 4 calls (5, 18, 23, 34) firm-level subsidies remained unretrievable.

[11]These included (i) failure to provide the documentation showing compliance of the investment with the conditions and limits set in the auction; (ii) non-compliance with nation-wide legislation concerning, e.g., labor laws, environmental or urban real estate legislation; (iii) large deviations from the targets underlying the objective score; (iv) violations of the non-cumulation requirements; (v) the rental, disposal or sale of the subsidized asset.

selected on objective rules vs. political discretion.

We merged these data with the Italian Social Security Institute (INPS) archives, which cover all Italian firms with at least an employee (around 1.6 million annually). These records present monthly employment statistics and document business start and closure dates. This allows us to meticulously trace the workforce trajectory and longevity of applicant firms pre- and post-subsidy application. However, the INPS dataset typically anonymizes sole proprietorships, causing a loss of data on about 20,000 micro-enterprise applications. For our study, we further excluded 10,000 applications from startups and trimmed the largest 1% of firms, which employ on average 5 thousand workers (i.e., 100 times the median firm size in our sample). These are the dominant firms in high-returns-to-scale industries (e.g. utilities, automotive, or chemicals), which would be difficult to reliably match to comparable units. Nevertheless, these sample alterations do not impact our findings. The principal investigation into employment outcomes focuses on a pool of 40,366 projects from 27,084 firms

Table 2: Characteristics of applicant firms

	Average	P_{10}	P_{25}	P_{50}	P_{75}	P_{90}	**Obs**
Panel A: Administrative data on L488/92							
Funds requested	685	61	130	303	697	1448	40,366
Score $\geq$ cutoff	0.45	0	0	0	1	1	40,366
Project funded — Score $\geq$ cutoff	0.80	0	1	1	1	1	40,366
Panel B: Social security data (INPS)							
Firm age	11	0	3	8	17	26	40,366
Firm size (N. employees)	36	1	4	12	30	74	40,366
Industrial	0.72	0	0	1	1	1	40,366
South	0.67	0	0	1	1	1	40,366
Employment growth $(t+6)$	0.28	-0.60	-0.10	0.21	0.66	1.37	35,156
Survive $(t+6)$	0.87	0	1	1	1	1	40,366
Panel C: Balance sheet data (CERVED)							
Total Assets, ths. €	13,153	394	1044	2921	8434	24173	27,856
Total Revenues, ths. €	12,784	311	925	2780	8377	24545	27,384
Investment rate	0.08	0	0	0.02	0.10	0.22	27,856
Revenues growth $(t+6)$	0.46	-0.36	0.09	0.44	0.85	1.41	18,516

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

For a significant portion of our sample (about 70% of firms in the matched L488/92-INPS data) we also retrieved balance data from Cerved – a proprietary database covering all limited liability companies incorporated in Italy. This supplementary data, which are available for 17,226 companies presenting 27,612 distinct projects, provides information on investment, revenues, and value-added. Importantly, this final sample matches the initial population of applicants on the main variables included in all datasets.[12]

In Table 2, we present the distributions of our dataset's primary variables. Reflecting the well-documented traits of Italian firms (e.g., Schivardi and Torrini, 2008), the data reveals a preponderance of smaller enterprises. On average, applicant firms employ 36.4 individuals, with a median of 11.5. Remarkably, a quarter of these firms have a staff of 3 or fewer. Regarding assets and revenues, the distribution displays a pronounced skewness. While the average for both metrics hovers around €13 million, the median is below €3 million, signifying a wide variance. Table 2 further shows that the typical subsidy request from applicants is just below €700,000. Notably, 45% of these applications surpass the eligibility threshold, indicating potential funding. However, among these eligible candidates, only 80% secure the actual funds. The remaining 20% face disqualifications for various reasons detailed earlier.

3.1 Evidence on political discretion

Before turning to estimating the treatment effect of subsidies, we examine the implications of political discretion for their allocation across firms. As explained in Section 2.2, the discretionary sub-score SD is ideally designed to mirror overarching regional government objectives. Indeed, regional governments allocated discretionary points by municipality, industry, and investment type prior to the release of project calls – and, thus, before the pool of applicants was revealed. However, one cannot exclude, a priori, that local politicians might manipulate point allocations to favor specific applicants. For example, they could grant more points to municipalities or industries that house firms with political connections. Yet, it is essential to recognize that the discretionary sub-score assigned to a project cannot directly be influenced by the value of its objective indicators.

In light of these institutional features, the extent to which political discretion might have been exercised to benefit particular applicants over broader policy objectives remains ambiguous. To delve deeper into this matter, we investigate the factors influencing the objective and discretionary sub-scores—denoted as SR and SD respectively. For this analysis, we employ a LASSO regression, a machine-learning algorithm that selects a subset of relevant regressors while, at the same time,

[12]For detailed comparisons on subsidy distributions, project scores, and other important metrics between the base and enhanced sample, refer to Online Appendix Figure A5 and Online Appendix Table A2.

estimating their relationship with the dependent variable of interest. Specifically, the LASSO estimator is

$$\widehat{\theta}^{\texttt{LASSO}} := \arg\min_{\theta \in \mathbb{R}^k} \left\{ \sum_{i=1}^{n} (Y_i - Z_i'\theta)^2 + \lambda \sum_{j=1}^{k} |\theta_j| \right\}, \tag{2}$$

where Y_i is the outcome variable of interest (in our case, the sub-scores SR and SD) in the sample $i = 1, 2, ..., N$; Z_i is a k-th dimensional vector of candidate predictors, and θ is the $k \times 1$ vector of associated parameters of interest; finally, $\lambda \geq 0$ is a "shrinkage" parameter. When $\lambda = 0$, LASSO is equivalent to the OLS estimator, while $\lambda > 0$ introduces an additional penalty for non-zero coefficients. Therefore, as λ moves away from 0, the coefficients of variables with lower explanatory power are shrunk to 0. We choose the optimal λ using the one-standard-deviation rule (see James et al., 2013).

The vector Z_i includes a wide array of applicant characteristics such as age, employment size, and industrial sector; indicators of local socio-economic conditions measuring labor market and population characteristics, the local economic structure, and the extent of credit constraints;[13] and political proximity between the regional government and the municipality in which the applicant firm is located. Online Appendix Table B1 reports the complete list of variables and their description.

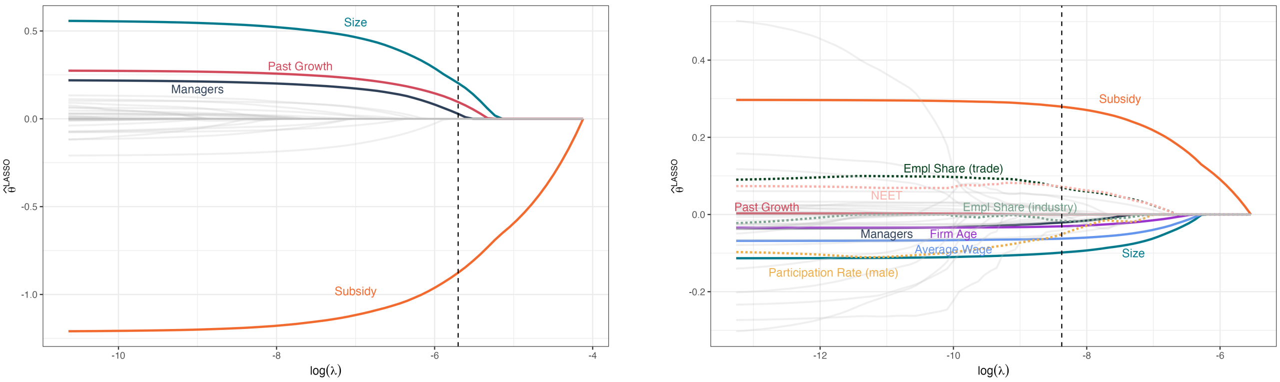
Figure 1i and Figure 1ii plot the estimated effect $\widehat{\theta}^{\texttt{LASSO}}$ of each variable in Z_i on SR and SD, respectively, as λ varies, together with the optimal λ (vertical dashed line). The most important predictors of the objective sub-score, SR, are firm size (positively) and the subsidy demanded by the applicant firm (negatively). Interestingly, such variables are also the most important predictors of the discretionary sub-score, SD, but with the opposite sign. In particular, smaller firms are penalized on the (planned) number of newly-created-jobs (indicator I2 of the objective sub-score), because they cannot compete with large firms on this dimension, but they receive on average higher discretionary evaluations from local politicians; the same is true for firms demanding larger subsidies, which are instead penalized on indicators I1 and I3 of the objective sub-score. These patterns are also shown in Online Appendix Figure A4, which plots either of the sub-scores SD and SR, on the horizontal axis, against average firm size and log of subsidy amount, on the vertical axis, controlling for cell fixed effects.[14]

In addition to the applicant's size and the requested subsidy amount, a number of municipality

[13]These are obtained combining municipality level, decennial population census data (e.g. the rate of participation, employment and unemployment; the local employment composition by sector and skill-content; schooling achievements and NEET rates among the youth; population density; etc.) with credit constraints measures from Guiso, Pistaferri and Schivardi (2013) (the spread between loan and deposit rates in local credit markets).

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

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



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

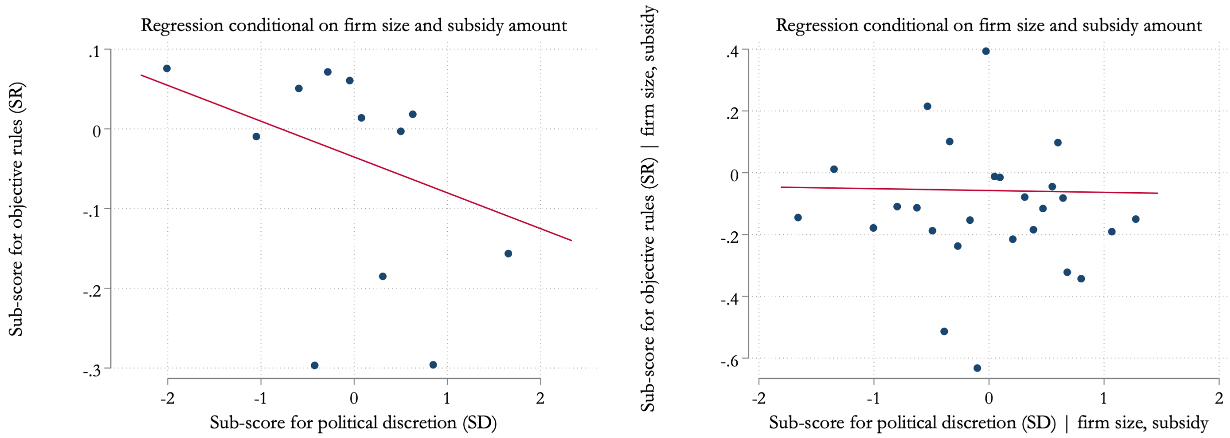
Notes: Panel i) and ii) report the LASSO coefficients in the regression of SD and SR on a rich set of covariates for various penalty parameters λ. The black dashed line denotes the optimal value of the penalty parameter according to the one-standard-deviation rule (see James et al., 2013, for additional details). More information on the source and the description of the included predictors can be found in Online Appendix Table B1 (second and fourth panels).

characteristics contribute to explaining the discretionary sub-score. The most important ones, according to LASSO, are the proportion of youth aged 15-24 ”Not in Education, Employment, and Training” (NEET); the male participation rate; and the employment share in manufacturing and non-manufacturing sectors. All these variables enter the regression with a positive sign, suggesting that local politicians tend to target more disadvantaged areas. Interestingly, these same variables do not predict the objective sub-score, which likely reflects more directly the quality of investment projects rather than other contextual factors. We discuss the implications of these findings in Section 6.4, where we assess the cost-effectiveness of counterfactual policies weighting differently the two sub-scores.

Finally, Figure 2 shows that SD and SR are negatively correlated (left graph), but this inverse relationship reflects the fact that both variables are correlated, in opposite directions, with firm size and the subsidy amount; controlling for these two factors, the relationship between SR and SD becomes flat (right graph).[15]

[15] In Section 6.4, when presenting the results of the counterfactual analysis, we discuss – both theoretically and empirically – the possibility that politicians assign the discretionary score SD by taking into account the (expected) objective scores SR obtained by the different projects. In line with the evidence in the right graph of Figure 2, we find that SD does not systematically respond to SR.

Fig. 2: Political discretion and objective rules



Notes: The figure plots the sub-score for political discretion against that for objective rules across quantile-spaced bins, controlling for cell fixed effects (left graph) and, in addition, for firm size and amount of requested subsidy (right graph). Covariate adjustment and the choice of the optimal number of bins are performed according to Cattaneo, Crump, Farrell and Feng (2022).

4 Empirical Strategy

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

As discussed in the previous section, there is one-sided non-compliance with treatment assignment: about 20% of applicants scoring above the cutoff do not receive the subsidy, while no applicant scoring below the cutoff is subsidized. To the extent that D is as-good-as-randomly-assigned across applicants that are arbitrarily close to the cutoff – an assumption that seems very plausible in the present context, and that we validate in the next section – the difference in outcomes between applicants just above and below the cutoff,

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

identifies the "intention-to-treat" (ITT) effect of scoring above the cutoff. In what follows, we focus on estimating this parameter, which provides a lower bound for the magnitude of the (local-at-cutoff) average treatment on the treated (ATT) effect of receiving the subsidy. Under the assumption that treatment assignment (i.e., scoring above the cutoff) affects outcomes only through the actual treatment (i.e., receiving the subsidy), the ATT effect equals the ITT effect divided by the share of compliers (roughly 0.8 in our context, see Table 2).

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

$$Y = \tau D + \sum_{\ell=1}^{p} \gamma_\ell S^\ell + \sum_{\ell=1}^{p} \delta_\ell D \cdot S^\ell + FE_c + \varepsilon. \tag{3}$$

Following Fort, Ichino, Rettore and Zanella (2022), we include fixed effects at the cell level to control for the fact that the cutoffs are endogenously determined – i.e., not set ex-ante. We restrict the sample to applicants with an application score within the bandwidth $[-5, 5]$ (82% of our sample), and we use linear ($p = 1$) and quadratic ($p = 2$) polynomials in S, as well as triangular kernels attaching greater weight to observations closer to the cutoff.[16]

Under the assumption that other determinants of Y vary smoothly at the cutoff, the coefficient τ in equation (3) identifies the reduced form effect of the subsidy across firms near the cutoff (Hahn, Todd and Van der Klaauw, 2001). However, the effects across inframarginal firms away from the cutoff – and, thus, the overall policy effect – are not identified in general, as high- and low-scoring firms may differ along some unobservable dimension . However, Angrist and Rokkanen (2015) note that, in RD designs, treatment assignment is fully determined by the running variable – in our case, the application score – which is therefore the only source of selection bias. Hence, if there exists a set of covariates X such that potential outcomes are mean independent of the running variable conditional on X,

$$\mathbb{E}\left[Y(d) \mid S, X\right] = \mathbb{E}\left[Y(d) \mid X\right], \quad d \in \{0, 1\}, \tag{4}$$

then one can identify average treatment effects at any $S = s'$ and estimate them by comparing treated and controls conditional on X. The conditional (mean) independence assumption (CIA) in equation (4) implies that potential confounders (e.g., high-scoring firms being better-managed) would be either absorbed by X or uncorrelated with the outcome.

To be more specific, following Angrist and Rokkanen (2015) let the running variable S be a function $S = g(X, u)$ of some observable variables X and some unobservable variables u . If

[16] As recommended by Gelman and Imbens (2019), we present baseline results for linear and quadratic specifications of equation (3) (i.e., $p \leq 2$), but we show in Online Appendix Figures A9 and A10 that all results are virtually identical for $p = 0$ and $p = 3$, and they are even stronger when estimating non-parametric RD regressions for different multiples of the MSE-optimal bandwidth (as defined in Calonico et al., 2014).

potential outcomes are mean independent from the score S conditional on X, then controlling for X is sufficient to eliminate selection bias when comparing units away from the cutoff. This is because conditioning on X makes potential outcomes independent from S and, thus, from the treatment status D. Therefore, variables that would potentially bias estimates of treatment effects away from the cutoff are either included in X or in u; in the former case, we control for them, and in the latter we can safely ignore them. In addition to the CIA condition in (4), this approach requires the classical common support between treated and controls with respect to X,

$$0 < \mathbb{P}\left(D = 1 \mid X\right) < 1 \qquad \text{a.s..} \tag{5}$$

Crucially, the CIA is partially testable. That is, the RD design provides a test for the usually untestable assumption that conditioning on X removes all confounding differences between treated and controls. In addition, it is straightforward to check whether (5) holds. If both conditions hold, by the law of iterated expectations we can identify the average treatment effect at $S = s'$ as

$$\mathbb{E}\left[Y(1) - Y(0) \mid S = s'\right] = \mathbb{E}\left[\mathbb{E}\left[Y \mid X, D = 1\right] - \mathbb{E}\left[Y \mid X, D = 0\right] \mid S = s'\right]. \tag{6}$$

Following Angrist and Rokkanen (2015), we estimate (6) using the linear reweighting estimator by Kline (2011):

$$\mathbb{E}\left[Y \mid S, X, D = d\right] = \sum_{\ell=0}^{q} \alpha_{d,\ell} S^{\ell} + X'\beta_d, \quad d \in \{0, 1\}, \tag{7}$$

where q represents the order of the polynomial basis in S. Failure to reject the restrictions $\alpha_{0,\ell} = \alpha_{1,\ell} = 0, \ell = 1, \ldots, q$, provides evidence consistent with the CIA in (4).

If such restriction holds, we can indeed substitute (7) into (6), to obtain

$$\mathbb{E}\left[Y(1) - Y(0) \mid S = s'\right] = (\beta_1 - \beta_0)' \, \mathbb{E}\left[X \mid S = s'\right]. \tag{8}$$

We can estimate equation (7) for $d = 1$ ($d = 0$) across treated (control) units, retrieve predicted outcome values, and take their difference to estimate (8). If common support (5) holds, this method allows us to characterize treatment effects all over the support of the running variable S.[17]

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

[17] In a companion paper, Palomba (2023) introduces a new Stata package, `getaway`, which implements different methods for extrapolating RD estimates away from the cutoff together with several tests and graphical tools. The package is available at https://github.com/filippopalomba/getaway-package.

SD. If the CIA holds,

$$\mathbb{E}\left[Y(d) \mid SR, SD, X\right] = \mathbb{E}\left[Y(d) \mid X\right], \quad d \in \{0, 1\}, \tag{9}$$

we can estimate conditional average treatment effects as

$$\mathbb{E}\left[Y(1) - Y(0) \mid SR = sr', SD = sd'\right] = (\beta_1 - \beta_0)' \, \mathbb{E}\left[X \mid SR = sr', SD = sd'\right]. \tag{10}$$

Equation (10) will allow us to assess the contribution of objective rules and political discretion, respectively, to the effectiveness of public subsidies.

4.1 Evaluating the impact of counterfactual assignment rules

To evaluate the impact of counterfactual assignment rules, we need to impose the policy invariance condition that neither the distribution of applicant firms with respect to their characteristics relevant for the outcome nor the characteristics of the projects they submit are affected by a change in the rules for assigning subsidies. Formally, let F_Z^{a} and F_Z^{cf} be the distributions of the pool of applicants with respect to the characteristics Z relevant for the outcome under, respectively, the actual and the counterfactual assignment rule and let $SR^{\mathsf{a}}(i)$ and $SR^{\mathsf{cf}}(i)$ be the objective sub-score obtained by the i-th project under, respectively, the actual and the counterfactual assignment rule. The policy invariance condition we invoke is

$$F_Z^{\mathsf{a}} \sim F_Z^{\mathsf{cf}} \text{ and } SR^{\mathsf{a}}(i) = SR^{\mathsf{cf}}(i), \quad \forall\, i. \tag{11}$$

In principle, this condition could be violated for several reasons. For instance, some applicants may be more or less willing to apply once local politicians can directly intervene in the scoring process, or they may submit projects that are more in line with the priorities of the regional government in terms of location, industrial sector, and type of investment (see Section 2.2). However, the priorities communicated by regional governments to the Ministry of Economic Development were not disclosed publicly, so it is unclear whether firms took those into account when preparing their applications. Although assumption (11) is not immediately testable, in Section 6.4 we provide evidence consistent with such assumption by comparing, difference-in-differences, the number and quality of projects (as measured by the sub-score for objective rules, SR) of applicants before and after the introduction of discretion, and between regions in which the regional government decided not to use discretion and other regions.

Holding condition (11), we shall use the CIA condition (9) to assess how the average impact of subsidies would change under alternative assignment rules, e.g. under an assignment rule based only on the sub-score for objective rules, SR. Condition (9) plays a crucial role in this exercise, since it implies that conditioning on X the objective sub-component SR is as good as random, thus allowing us to evaluate what the average impact would be at each point of the support of SR:

$$\mathbb{E}\left[Y(1) - Y(0) \mid SR = sr'\right] = (\beta_1 - \beta_0)' \, \mathbb{E}\left[X \mid SR = sr'\right]. \tag{12}$$

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

5 Results at the RDD cutoff

Figure 3 plots the relationship between the score obtained by applicant firms, the subsidy they received and the log of total, cumulated investment over the three following years. The left graph confirms that only those scoring above the cutoff are funded. Treated firms near the cutoff received on average half a million euros (at constant 2010 prices) over three years, and significantly increased investment compared to other control applicants that ranked just to the left of the cutoff (right graph).[18]

Online Appendix Figure A7 shows that applicants ranking just above and below the cutoff are on average equal on a wide range of other characteristics measured one year before the call. Online Appendix Table A1 presents the results of formal tests. Online Appendix Figure A8 shows that the five components of the score, described in Section 2, also vary smoothly around the cutoff.[19]

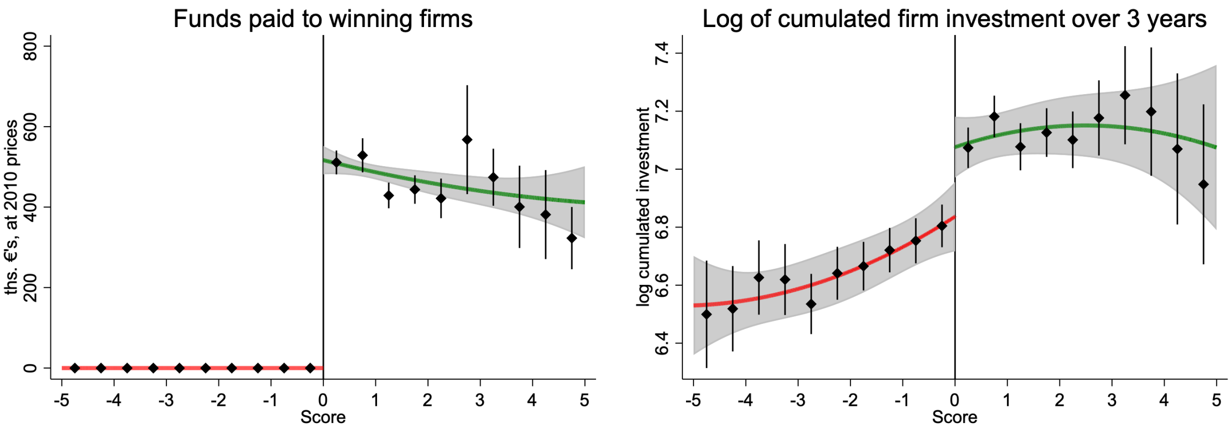
Taken together, Online Appendix Figures A7 and A8 strongly support the main identifying assumption that applicants within an arbitrarily narrow bandwidth of the RD cutoff are unable to precisely determine their assignment to either side of it (see, e.g. Lee and Lemieux, 2010). We can thus attribute any difference in outcomes between firms scoring just above and just below the cutoff to the causal effect of the subsidy.

The stated objective of L488/92 was to increase employment in disadvantaged areas, so Figure 4 shows the effect of the subsidy on the log-change of firm employment. In the year before the

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

[19] Online Appendix Figure A6 shows no evidence of discontinuity in the density of applications. The formal test by McCrary (2008), as implemented by Cattaneo, Jansson and Ma (2020), does not reject the null hypothesis of no discontinuity at the cutoff with a p-value of 0.2.

Fig. 3: Funds obtained by winning firms and investment over the following 3 years



Notes: This figure shows the relationship between the amount of funds obtained by firms applying for L488/92 subsidies (left graph) and the log of cumulated firm investment over the following three years (right graph) against the standardized score they obtained (on the horizontal axis). Bins represent averages over equally-spaced intervals of size 0.5, and confidence intervals (at the 90% significance level) are also shown by vertical lines. The predicted relationships between each outcome and the score are estimated using a quadratic polynomial regression. 90% confidence bands for the predicted relationship (in grey) are computed based on heteroskedasticity-robust standard errors clustered by cell.

L488/92 call, firm employment is balanced between treated and control firms near the cutoff (see Figure A7), but the subsidy progressively opens a gap between the two groups during the following years. The gap is already noticeable one year after obtaining the subsidy (first graph); it increases at the end of the subsidy period (second graph) and persists in subsequent years (third graph).

Table 3 shows the effect of obtaining the subsidy on (log) employment and investment, as estimated from different specifications of equation (3). All results remain virtually identical, so we focus on the simplest linear specification with cell fixed effects throughout the paper. According to this specification, presented in column (2) of Table 3, the subsidy increases firm investment by 0.36 log points, i.e. +43 percent over the following three years (Panel A), and it increases employment by 11 percent over the same period (Panel B), and by 17 percent over a period of six years (Panel C). All these estimates are strongly statistically significant. Figures A9 and A10 in the Online Appendix replicate the analysis for employment and investment, respectively, using non-parametric methods. The results are robust to varying the bandwidth between $0.5B^*$ and $3B^*$, where B^* is the MSE-optimal bandwidth according to Calonico, Cattaneo and Titiunik (2014), and to varying the degree of the polynomial in the running variable between 0 and 3.

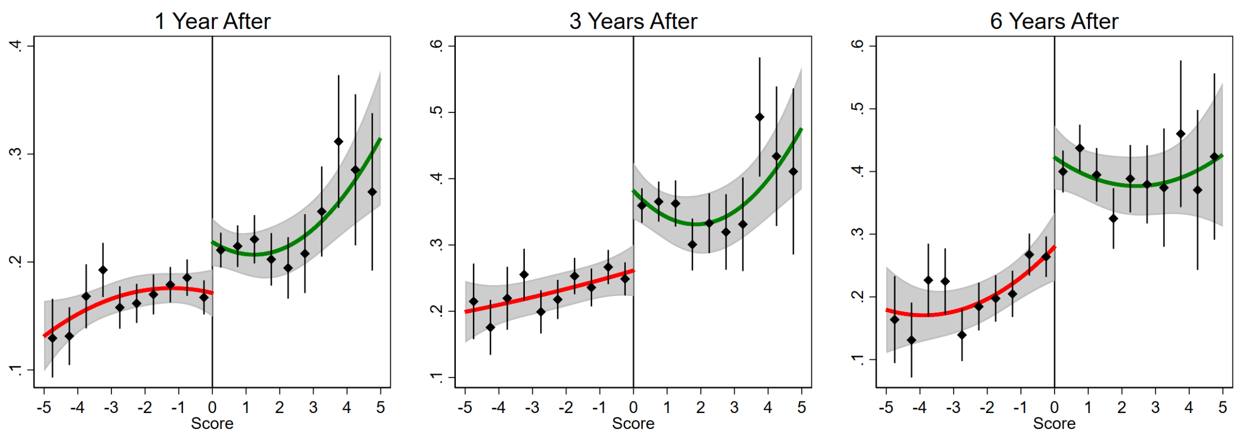
Figure 5 plots the estimated dynamic treatment effects on firm investment, employment, and other

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification:	linear				quadratic			
Kernel:	uniform		triangular		uniform		triangular	
Group fixed effects:	no	yes	no	yes	no	yes	no	yes
Panel A: Log of cumulated investment over 3 years								
Subsidy	0.300***	0.360***	0.277***	0.321***	0.240***	0.278***	0.255***	0.277***
	(0.060)	(0.055)	(0.060)	(0.058)	(0.077)	(0.073)	(0.077)	(0.073)
Observations	17,425	17,425	17,425	17,425	17,425	17,425	17,425	17,425
R-squared	0.016	0.229	0.014	0.230	0.016	0.229	0.014	0.230
Panel B: 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 C: Log-change in employment over 6 years								
Subsidy	0.147***	0.153***	0.145***	0.139***	0.142***	0.124***	0.131***	0.119***
	(0.023)	(0.024)	(0.023)	(0.023)	(0.030)	(0.029)	(0.032)	(0.030)
Observations	28,759	28,759	28,759	28,759	28,759	28,759	28,759	28,759
R-squared	0.007	0.066	0.007	0.067	0.007	0.066	0.007	0.067

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

Fig. 4: The effect of the L488/92 subsidy on firm employment



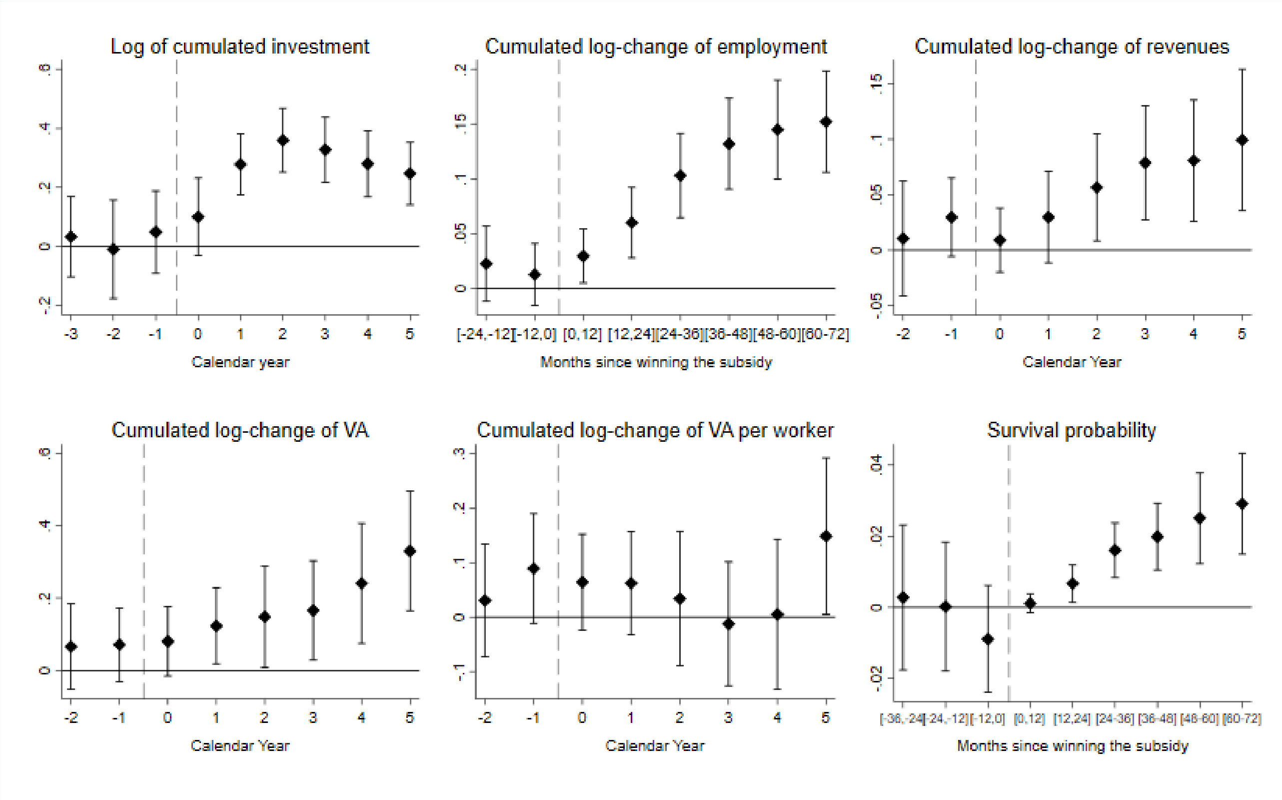
Notes: These graphs show the relationship between the standardized score obtained in firm applications for L488/92 funds, on the horizontal axis, and the (log) employment 1, 3, and 6 years after the award of subsidies. Bins represent averages over equally-spaced intervals, and confidence intervals (at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

outcomes of interest, as well as (placebo) estimates for the years before obtaining the subsidy. The first two graphs confirm that the subsidy generates a transitory effect on investment, which translates into a long-lasting increase in firm employment; revenues and value added exhibit a similar percent increase as employment (third and fourth graph), implying in turn that firm productivity remains approximately constant (fifth graph).

The last graph in Figure 5 shows that firms receiving the subsidy have higher survival rates than control firms. The difference after 6 years amounts to 3 percentage points, on a baseline survival rate of 87 percent. To the extent that excess mortality hits the lowest-performing firms in the control group (as it seems likely), the estimated effect on the other outcomes of interest – employment, revenues, value added, and productivity – is a lower bound to the average treatment effect when including non-surviving firms as well.

In Online Appendix 3 and in Section S2 of the Supplementary Materials, we discuss two issues that could affect the interpretation of our results. First, applicants in a given call may re-apply (and obtain funds) in subsequent calls. We show that applicants obtaining funds are *less* likely to re-apply and to obtain funds in subsequent calls, so our estimates provide a lower bound to the direct effect of subsidies in one-off calls. We also show that the difference between the direct effect and the total effect (i.e., accounting for the different probability of re-applying and obtaining

Fig. 5: Dynamic effects of the L488/92 subsidy on several firm outcomes



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

funds in subsequent calls) remains small. Second, the effects on funded firms may spill over to other, non-funded firms. The sign of potential spillover effects is unclear a priori. In case of positive local spillovers, our baseline estimates would understate the aggregate effect of subsidies; if, on the other hand, subsidized firms eroded the market share of competitors, including firms in the control group, our estimates would be biased upward. We show that, empirically, there are no strong spillovers from subsidized firms to firms in the control group or those operating in the same local labor market and sector.

6 Results away from the RDD cutoff

The results in the previous section show that L488/92 increase employment by 17 percent over a 6-year period across firms near the cutoff. We next estimate average treatment effects away from the cutoff following the approach of Angrist and Rokkanen (2015). With this analysis we can characterize the heterogeneity across different groups of firms; the cost-effectiveness of the policy, as measured by the ratio of public funds over the number of created jobs, and the effectiveness of the policy under alternative allocation criteria.

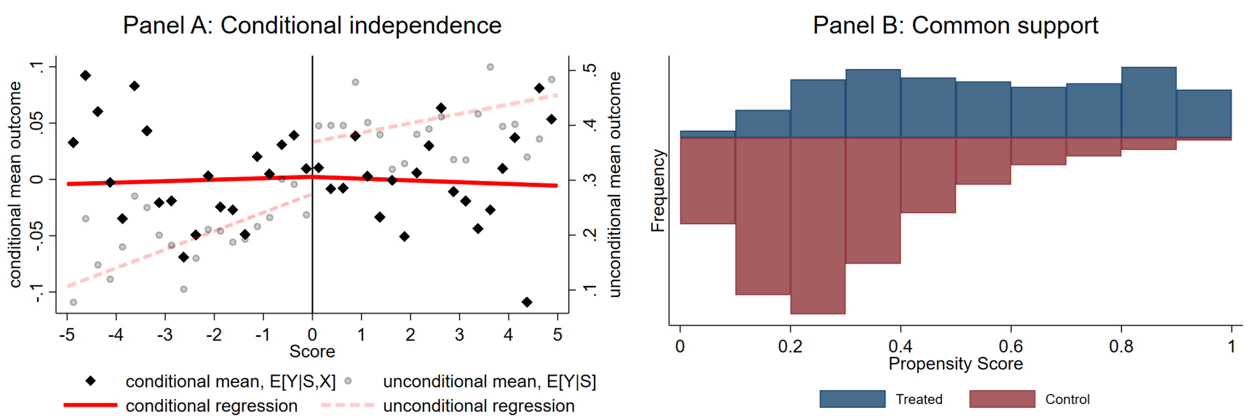
As discussed in Section 4, Angrist and Rokkanen (2015) invoke mean independence of the outcome on the running variable and common support between treated and control groups, conditional on a set of covariates X. We experiment with alternative predictors of firm growth, and we achieve conditional independence and common support for a vector $X^{\star}$ that includes the following covariates: firm age, which is inversely related to growth (Evans, 1987); lagged realizations of a firm's growth and 3-year forward growth of firms in the same market, as defined by the LLM and 3-digit sector; pre-treatment workers' skills, as measured by the average wage of white collar workers and indicators for having managers or apprentices in the payroll, and a measure of the size of the investment project, scaled by initial employment along with cell fixed-effects.[20] Importantly, all results are robust when selecting an alternative set of covariates based on a newly developed data-driven algorithm in the spirit of Imbens and Rubin (2015). This algorithm implements a *greedy approach* that selects, at each step, the variables making the ignorability condition most likely to hold. We discuss this alternative approach in Section S3 of the Supplementary Materials.

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

In Figure 6 we visualize the results of the tests for conditional independence - equation (4) - and common support - equation (5) - for the vector of covariates $X^{\star}$. Starting with the former condition, Panel A plots the binned residuals from a regression of 6-year employment growth on $X^{\star}$ against the applicant's score (black markers) together with the conditional regression line (solid line) separately on each side of the cutoff. The relationship is flat, as confirmed by the estimated coefficients reported in Online Appendix Table A3. The same graph also shows that, in line with the evidence in previous Figure 4, the unconditional relationship between the outcome and the running variable is positive (grey markers and dashed regression line). In other words, while on average higher-ranked applicants experience faster employment growth on both sides of the cutoff, this relationship is broken conditioning on the vector of firm characteristics in $X^{\star}$.[21] Therefore, the variability in the treatment status induced by S after conditioning on $X^{\star}$ is *as if* randomly determined.

Finally, Panel B of Figure 6 displays considerable common support between treated and controls in the distribution of the propensity score $\mathbb{P}(D = 1 \mid X^{\star})$.[22]

Fig. 6: Testing the conditional independence and common support



Notes: Panel A shows the test of conditional independence in equation (4) for the vector of covariates $X^{\star}$, described at the beginning of Section 6. Black markers represent the mean outcome Y (i.e., firm employment growth in the 6 years after applying for L488/92 funds) by equally-spaced bins of the running variable (i.e., the application score), conditional on the vector $X^{\star}$ of covariates, $E[Y|S, X^{\star}]$; the regression line is also reported (solid line). Grey circles and the dashed line represent the unconditional mean, $E[Y|S]$, and regression line. Panel B shows the density of treated and control firms by decile of the estimated propensity score of receiving the subsidy conditional on $X^{\star}$, $P(D = 1|X^{\star})$.

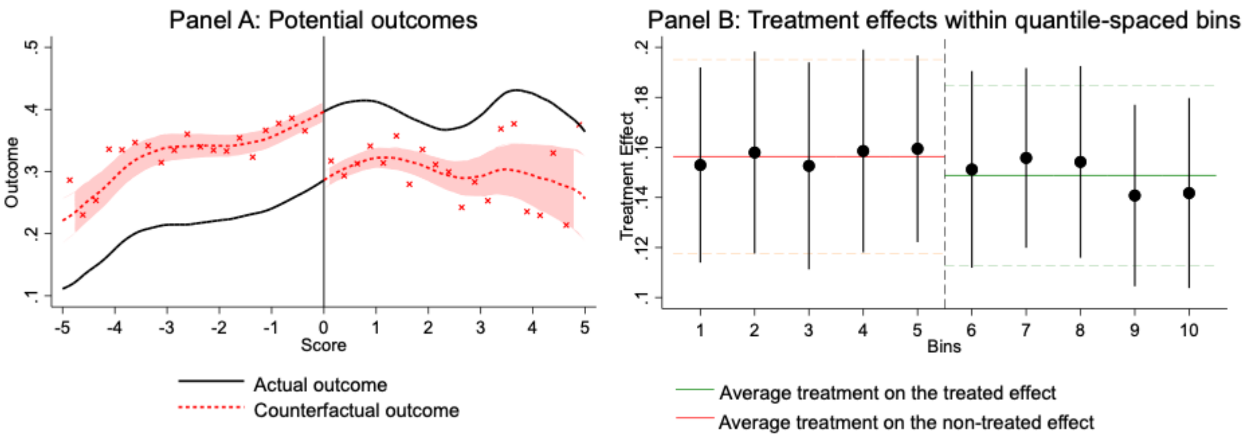
[21]The same result holds at any time-horizon between t+1 and t+6, for both employment growth and investment. These results are available upon request.

[22]Figure S8 in the Supplementary Materials provides additional evidence of common support over the joint distribution of the estimated propensity score and the running variable.

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

Under conditional independence and common support, we can estimate treatment effects across firms away from the cutoff by comparing the outcomes of treated and control firms keeping constant the covariates in $X^{\star}$. Specifically, we use the estimated parameters in equation (7) to predict the potential outcomes of control firms were they treated and to predict the potential outcomes of treated firms were they not treated. Panel A of Figure 7 plots fitted actual and extrapolated counterfactual outcomes along the distribution of the application score, together with bootstrapped confidence intervals. As it should be expected, both potential outcomes increase with the running variable, as higher-ranked applicants exhibit stronger employment growth both when they are treated and when they are untreated. The two lines are approximately parallel, implying that average treatment effects are constant along the application score. This is confirmed in Panel B of Figure 7, which plots treatment effects and confidence intervals across equally-spaced bins. This does not imply that the effects of subsidies can not vary along other dimensions other than the score, as we will see in Section 6.3.[23]

Fig. 7: Potential outcomes and treatment effects in $t+6$, along the distribution of the applicant score



Notes: Panel A plots actual and counterfactual potential outcomes six years after obtaining or not obtaining the subsidy, as estimated from kernel-weighted local polynomial smoothers, along the applicant score. Counterfactual outcomes are estimated by equations (7), and bootstrapped confidence intervals are reported. Panel B plots average treatment effects within quantile-spaced bins on either side of the cutoff, estimated using the linear reweighting estimator in (8). 95% confidence intervals are estimated using 2000 iterations of a non-parametric cluster bootstrap.

[23] Supplementary Materials Figure S9 shows that the results in Figure 7 are not sensitive to excluding observations with an estimated propensity score outside $[0.1, 0.9]$, as recommended by Crump, Hotz, Imbens and Mitnik (2009).

6.2 Estimated policy effect and comparison with previous work

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

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

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

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

Therefore, the cost-effectiveness of L488/92 subsidies was much lower in Southern regions, which received the largest share of funds; see also Figure A14 in the Online Appendix. This relationship is consistent with decreasing returns to the mobilization of new public subsidies, particularly in disadvantaged areas characterized by a scarcity of profitable investment opportunities.[24] We next ask whether alternative allocation rules could have improved on cost-effectiveness.

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

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

Notes: This table shows the cost of new jobs and investment generated by the L488/92 subsidies over a six-year period. All amounts are expressed in thousand € at constant 2010 prices. The estimates in columns labelled as "manual" employ the set of covariates listed at the beginning of Section 6, while the estimates in columns labelled as "data-driven" employ the set of covariates selected by the algorithm described in detail in Section S3 of the Supplementary Materials. 90% confidence intervals are reported in brackets and are computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level.

[24] The differential effectiveness of public subsidies is more generally likely to reflect the well-known territorial economic divide in Italy (which we find to persist despite, as mentioned in Section 2, many of the Northern firms eligible for the subsidy were located in distressed areas). A host of firm- and context-specific characteristics concur to determine this outcome. Online Appendix Table A5 shows that balancing out firm observable characteristics (by pairwise matching Southern to Northern firms on age and industry, size and employment composition, average wage, and past employment growth) absorbs part of the differential. However, the average estimated costs of both new jobs and new investment in the South are still twice as high as those of the North.

6.3 Rules vs. discretion

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

We next compare the effectiveness of projects selected on the basis of objective rules and political discretion, respectively, using the result in equation (10). To this purpose, we first show that the CIA holds for both scores jointly. In Table 5, we regress the log change in employment over 6 years (i.e., our main outcome of interest) on both subscores SR and SD separately for projects on either side of the cutoff, and include the additional set of covariates $X^{\star}$ (columns 2 and 4). In line with the CIA, the coefficients of SR and SD are no longer significantly different from zero after controlling for $X^{\star}$. In addition, Online Appendix Figure A11 plots the residuals of the estimated regression over the support of (SR, SD) – 25 bins, corresponding to the 5×5 quintiles of SR and SD – separately to the left and right of the cutoff, and Online Appendix Figure A12 shows their 95% confidence intervals. There is no systematic relationship between the residuals and either of the two sub-scores, and confidence intervals do not cross the zero line only in 5 cases out of 100, which is what we should expect under the null hypothesis that the CIA holds.

Turning to the estimation of treatment effects, Panel A of Figure 8 plots the effect on 6-year employment growth by quintiles of the sub-scores for objective rules (SR) and political discretion (SD).[25] Both the firms preferred by regional politicians and those scoring high on objective criteria generate larger employment growth compared to other applicants. The effect ranges between 10% for applicants scoring low on both SR and SD to almost 20% for applicants scoring high on both dimensions. Therefore, the (approximately) constant effect along the distribution of the overall application score S, shown in Figure 7, results from the fact that SR and SD are inversely correlated with each other (Figure 2), thus masking a positive relationship between job creation and both of the two sub-scores.

At the same time, the cost of creating new jobs, in terms of subsidies, varies dramatically between applicants ranking high on SR and applicants ranking high on SD. Panel B of Figure 8 shows that the number of new jobs created per €100,000 of subsidies received by the firm is highest in the south-east quadrant (high on rules and low on discretion) and it is lowest in the north-west

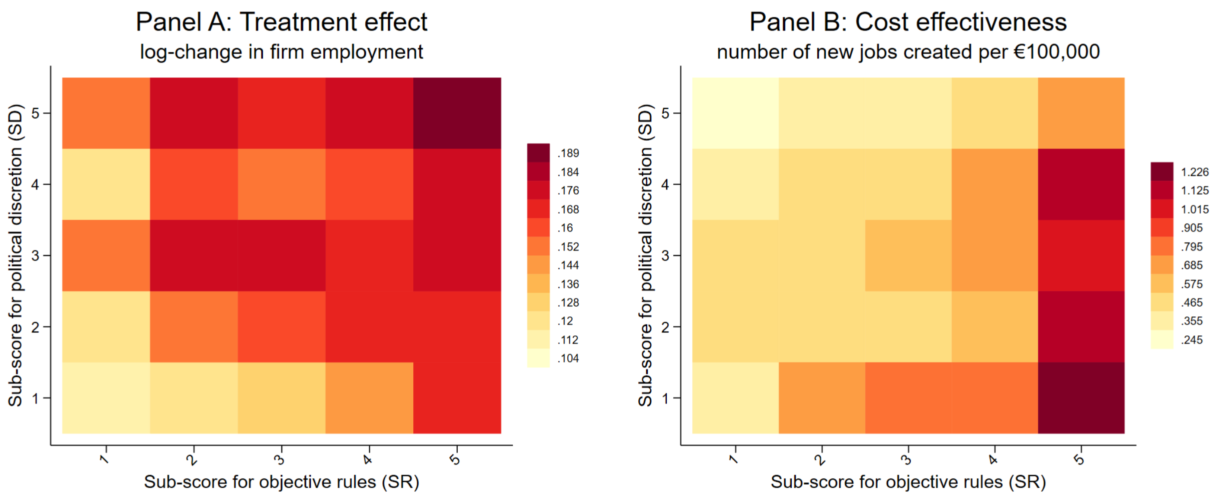
[25] Online Appendix Table A8 reports point estimates and confidence intervals for all entries of Figure 8.

Table 5: Conditional independence tests

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

Notes: The table reports regression-based tests of the conditional independence assumption in equation (9). We regressed employment growth and investment in the six years after the award of L488/92 subsidies on the two sub-scores for objective rules and political discretion (i.e., SR and SD). Regressions in even columns also includes all covariates in $X^{\star}$, listed at the beginning of Section 6. All regressions include cell fixed effects. The sample includes only applicant firms from the 3^{rd} call for projects onward, as the sub-score for political discretion was not present in the first two calls (see Section 2). Standard errors clustered by cell are reported in parenthesis.

Fig. 8: Treatment effect and new jobs created per €100,000, rules vs. discretion



Notes: This figure shows the heterogeneity in treatment effects on firm employment growth (Panel A) and the cost effectiveness of subsidies (Panel B), by quintiles of the sub-scores for objective rules (SR) and political discretion (SD). In Panel A, the treatment effect for each bin ($SR = r, SD = d$) is estimated as $\mathbb{E}[Y(1) - Y(0) \mid SR = r, SD = d] = (\beta_1 - \beta_0) \cdot \mathbb{E}[X^{\star} \mid SR = r, SD = d]$. The covariates included in $X^{\star}$ are listed at the beginning of Section 6. In Panel B, cost effectiveness is measured by the number of newly created per €100,000 of subsidies received by the firm. The number of newly created jobs in each bin is computed by multiplying the size of each firm by the treatment effect for its respective bin, as reported in Panel A, and aggregating across all firms in that bin. Online Appendix Table A8 reports the estimate for each bin together with 90% confidence intervals computed using 1,000 draws of a non-parametric cluster Efron bootstrap, where clusters are defined at the cell-level.

one (low on rules and high on discretion). On average, it takes just over €80,000 for high-on-rules, low-on-discretion applicant firms to create a new job, while the cost is five times as large for low-on-rules, high-on-discretion applicant firms.[26]

Therefore, the larger subsidies-per-worker demanded by applicants ranking high in terms of the political score, compared to applicants ranking high in terms of objective rules (Online Appendix Figure A4, bottom panels), translate into a higher cost-per-newly-created job. Figure A4 also made clear that applicant firms favored by political discretion are on average smaller than firms ranking high in terms of objective rules. Additional estimates reported in Online Appendix Table A7 reveal that smaller firms generate higher *percent* changes in employment but lower increases in the *number* of new jobs than large firms (250+ employees), as even smaller percent increases in employment in large firms translate into a high number of newly-created jobs.[27] As a consequence, the cost per new job is more than three times larger for small firms than for large firms – €253 thousand and €78 thousand, respectively, the two estimates being statistically different from each other.

These facts reconcile the findings in the two panels of Figure 8. In particular, both applicants favored by political discretion and those favored by objective rules generate large percent increases in employment, but the former are smaller in size and demand larger subsidies per worker. Hence, the same percent increase translates into a lower number of new jobs and a higher cost-per-new-job in small firms. Online Appendix Figure A15 shows that such differences are statistically significant.[28]

6.4 Counterfactual scenarios

To better understand the implications of allowing for political discretion in the selection of projects, we simulate the cost of new jobs under counterfactual policies. Specifically, we consider alternative criteria for ranking applications and compute the cutoffs obtained within each cell under the

[26]Figure S11 in the Supplementary Materials shows that the results in Figure 8 are not sensitive to excluding observations with an estimated propensity score outside $[0.1, 0.9]$, as recommended by Crump, Hotz, Imbens and Mitnik (2009). Supplementary Materials Figure S10 plots the fraction of such observations across quintiles of SD and SR.

[27]Heterogeneity in the effectiveness of the subsidy along the size and age dimensions is discussed in a companion paper based on the First Keynote Lecture held at the 34th Conference of the European Association of Labor Economists (Cingano, Palomba, Pinotti and Rettore, 2023).

[28]Since the risk of firm mortality may differ widely between small and large firms, in Online Appendix Figure A16 we investigate the heterogeneity of treatment effects on survival probability. The graph conveys two main results. First, the positive average treatment effect on survival probability over a 5-year period shown in Figure 5 (last panel) is pervasive across all groups of firms, ranging from +1.2 to +2.9 percentage point. Second, the effect increases with both the sub-scores SR and SD, similarly to the effect on employment growth.

counterfactual ranking. Some of the applicants funded under the actual policy would not be funded under the counterfactual policy, and vice-versa. We then compute the counterfactual cost of new jobs and investment by integrating the treatment effects over the subset of applicants funded under the counterfactual ranking.

This exercise maintains the policy invariance assumption (11): the distribution of applicants' characteristics as well as the average "objective quality" of the projects they submit, as measured by the objective scores, are not affected by a change of the selection rule. We provide evidence consistent with such assumption by comparing applicants' characteristics and projects' objective scores between the last call for projects before the policy change and the first call for projects after the policy change. Online Appendix Figure A13 shows a large degree of overlap in the distribution of such variables before and after the policy change, and Online Appendix Table A4 confirms that means are not significantly different between the two groups. We also implement a difference-in-differences version of this balance test by leveraging the fact that a few regions did not specify discretional priorities in all or part of the period after the policy change. In particular, the regional government of Lombardy always relied only on objective scores, while the government of Puglia did the same in the first two calls after the policy change but attributed discretionary points in the following calls. We thus compare average objective scores and applicants' characteristics between regions that used and did not use discretion, before and after the policy change, controlling for region and period fixed effects. These regressions, reported in Online Appendix Table A6, suggest that neither the average characteristics of applicant firms nor the average quality of submitted projects changed significantly after the policy change, providing strong support for the policy invariance assumption.

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

The costs of new jobs and new investment under these counterfactual policies are presented in Table 6, along with the costs under the actual policy (column 1).[29] Column (2) of Panel A shows that eliminating political discretion would reduce the cost of creating new jobs by 11 percent. Interestingly, the cost reduction would be more marked in southern than in northern regions (12 percent and 9 percent). The cost of investment, in Panel B, exhibits a similar reduction (13

[29]The costs reported in column (1) of Table 6 are slightly different from those in Table 4 because the latter is based on application in all calls for projects, while the former includes only applicant firms from the 3^{rd} call onward (the sub-score for political discretion was not present in the first two calls).

percent) but no clear gradient along the north-south dimension.

Column (3) of Table 6 shows the effect of an opposite policy, namely relying exclusively on political discretion for allocating subsidies. In this case, we cannot validate the policy invariance assumption in the same way as we did for the no-discretion counterfactual scenario. Moreover, politicians could in principle respond to the (expected) objective score SR obtained by different projects when assigning the discretionary score SD across municipality-industry-type of projects. In particular, they should attribute more points to projects that they favor and expect to be scoring lower on rules (SR). In this case, for given project characteristics SD should respond negatively to SR. In Appendix 4 we empirically estimate such response assuming, alternatively, that politicians form expectations about SR across municipality-industry-type of projects based on the average realizations of SR within such cells in the previous call ("adaptive expectations") or that they can anticipate average realizations in the current call ("perfect foresight"). In both cases, the estimated response remains very close to zero.[30] If we maintain the assumption of no response of SD to SR (consistent with the results discussed above), relying exclusively on politicians' discretion would greatly increase the cost of new jobs and investment – by 42 and 22 percent, respectively.

Overall, the evidence in columns (1)-(3) of Table 6 suggests that politicians' influence on the allocation of subsidies results in fewer new jobs generated for the same budget, particularly in disadvantaged (southern) regions commanding the largest share of the budget. Based on our previous results in Section 3.1, these results may partly reflect their targeting of the weaker sub-regional areas, where contextual factors hinder the impact of the policy above and beyond firm characteristics.

We also consider the possibility that the different effectiveness of projects selected on objective rules vs. political discretion may be biased by a different compliance with treatment assignment (i.e., a different probability of actually receiving the subsidy among applicants scoring above the cutoff). In Online Appendix Figure A17, we plot the relationship between non-compliance with treatment assignment and the sub-scores for objective rules and political discretion, respectively. While there is no relationship between non-compliance and the sub-score for political discretion, eligible applicants scoring higher on objective rules are less likely to receive the subsidy. A potential reason for this pattern is that such applicants may tend to over-promise in the application stage (e.g., in terms of new jobs to be created) and are eventually unable to deliver. In any event, the

[30] In Section S5 of the Supplementary Materials we obtain analogous results when employing a fully non-parametric approach that allows for a completely flexible relationship between SD and SR.

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

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

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

results in Online Appendix Figure A17 suggest that, if anything, we are under-estimating the cost-effectiveness of projects selected on objective rules relative to projects selected on political discretion.

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

7 Conclusions

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

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

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

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