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THE EFFECTS OF A LARGE PLACE-BASED REDUCTION OF SOCIAL SECURITY EMPLOYERS' CONTRIBUTIONS: THE CASE OF DECONTRIBUZIONE SUD

by Giuseppe Albanese*, Emanuele Ciani*, Gabriele Macci*,
Graziella Mendicino* and Andrea Petrella*

Abstract

We study the *Decontribuzione Sud* programme, introduced in 2020 to improve the competitiveness of firms operating in Southern Italy through a substantial reduction in social security contributions. Using administrative data from INPS and the National State Aid Registry, we document the characteristics of firms that joined the programme, investigate the sources of its lower-than-expected participation rate, and examine its interactions with other incentive schemes and State aid regulations. Exploiting a border discontinuity design along the boundary of the eligible Southern regions and balance sheet data for incorporated companies, we estimate the programme's impact at the firm level, both on employment and on key financial outcomes. For our benchmark sample of small and medium-sized enterprises (SMEs), we find that the reduction in labour costs raised sales and profitability on average across firms but had no measurable effect on employment or wages; the resulting profitability gains translated into improved firm liquidity rather than higher investment.

JEL Classification: H25, H32, J38, R58.

Keywords: place-based policies, employment subsidies, investment.

DOI: 10.32057/0.QEF.2026.1011

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1. Introduction*

In October 2020, the Italian government introduced "Decontribuzione Sud" (DS), a large-scale program aimed at enhancing firms' competitiveness in the lagging Southern regions. The measure provided a 30% cut in employers' social contributions for both incumbent workers and new hires in the private sector, provided they were located in the South, with some exceptions. There were no requirements in terms of additional investments or employment. The cut implied a projected 5.6% labor cost reduction for Southern companies from 2020 to 2025. The intervention was then set to be gradually phased out, with the cost reduction decreasing to 3.7% in 2026-27 and 1.9% in 2028-29.[1]

DS has faced significant regulatory uncertainty due to its reliance on the European Union Temporary Frameworks (TFs) for State aid. Although initially designed to run until 2029, its implementation has been subject to a series of short-term approvals by the European Commission (EC), typically granted in 6- to 12-month extensions.[2] In some instances, approval was delayed, allowing firms to retroactively claim benefits for previous months. More importantly, with the end of the TFs, the EC opposed further extensions, leading to the policy's termination five years earlier than planned, on December 31, 2024.[3]

Further complications arose from evolving aid caps across different TFs. The initial COVID-19 Temporary Framework (Section 3.1) imposed a €800,000 aid limit per firm, which was later increased to €2.3 million. Meanwhile, the Temporary Framework addressing the Ukraine crisis (Section 2.1) established a separate limit, ultimately reaching €2.25 million. These constant adjustments to aid ceilings, combined with uncertainty around short-term extensions, created a challenging regulatory environment, potentially disrupting long-term strategic planning and hiring stability.

The literature on the effect of place-based payroll cuts has mostly focused on their employment and wage effects. The evidence is mixed and suggests that the impact is context-dependent. Bennmarker, Mellander, and Öckert (2009), Korkeamäki and Uusitalo (2009), and Stokke (2021) study regional payroll tax reductions or social security cuts in Sweden, Finland, and Norway, respectively. They find no effect on employment and only a limited pass-through of employer tax cuts to wages. In contrast, Ku, Schönberg, and Schreiner (2020) find that the removal of geographically differentiated payroll taxes in Norway between 2004 and 2006 led to a significant decline in local employment. Benzarti and Harju (2021), exploiting two regional payroll tax cuts in Finland, find that effects tend to be stronger during downturns.

Apart from regional and cyclical differences, the broader empirical evidence on the impact of hiring subsidies suggests that they are more effective when applied to groups that face greater difficulty in finding a job, such as the long-term unemployed (see Card et al., 2018, for a meta-analysis, and Ciani, Grompone, and Olivieri, 2024, for Italy). For the DS, a preliminary evaluation of the policy limited to 2022 is contained in INPS (2023); comparing labor market dynamics across provinces located on either side of the administrative border of

* The views in this paper are those of the authors and do not necessarily reflect those of the Bank of Italy.

[1] Specifically, at the time of its establishment, DS provided for a reduction by 30% of social security contributions for employers for years 2020-25, declining to 20% for 2026-27 and 10% for 2028-29. Given that social security contributions represent about 24% of additional costs on top of gross wages, and assuming an extra 6% of other costs related to labor which are beyond the scope of DS (like for instance INAIL contributions), we have (30% x 24%) / (100% + 24% + 6%) ≈ 5.6% reduction of total labor costs for the years 2020-25.

[2] COVID-19 Temporary Framework was introduced in March 2020 (2020/C 91 I/01) and it was amended six times before expiring in June 2022. The next TF, designed after the Russian invasion of Ukraine, entered into force in March 2022 (2022/C 131/08) and, after several rollovers and changing name, lasted for two years until mid-2024.

[3] For the 2025–2029 period, the original scheme was replaced by a more narrowly defined policy framework that introduced a clear distinction between small and medium-sized enterprises (SMEs) and large firms. For SMEs, the policy was aligned with the constraints of the "de minimis" regime, a State aid framework that allows member states to grant limited amounts of aid - currently up to €300,000 per firm over three financial years - without prior authorization from the EC, on the grounds that such support is considered too small to distort competition within the internal market. By contrast, a more selective measure was envisaged for large firms, with eligibility restricted to permanent employment contracts and, crucially, to firms able to demonstrate a verifiable expansion of their workforce during the reference year; however, this measure did not enter into force due to the lack of authorization from the EC.

Southern regions, the report found smaller effects compared to other, more selective subsidies for youth and women[4].

There is less evidence about the effect of place-based payroll tax cuts on firm outcomes. One exception is Benzarti and Harju (2021), who also examine investment and sales, finding that firms that benefited from payroll tax cuts fared better on both outcomes during recessions. The broader literature on hiring subsidies suggests similar results. For instance, Saez, Schoefer, and Seim (2019) find that firms with a larger proportion of young workers increased not only employment but also capital, sales, and profits after a reform introduced a payroll tax cut for employees under the age of 26 in Sweden. However, there is also evidence showing that hiring subsidies lead to lower capital intensity, as the labor input becomes cheaper (Depalo and Viviano, 2024).

We contribute to this literature by analyzing the impact of DS at the firm level. First, we document its lower-than-expected take-up, describing the characteristics of firms that benefited from the measure and the interactions between DS, other incentives, and State aid regulations. We then employ a border regression discontinuity design, based on the firm's distance from the North-South administrative border, to assess the program's impact on firm outcomes, including both employment and key firm performance indicators. We use data on incorporated companies, for which detailed balance sheet information is available, and we limit the analysis to SMEs, which are less likely to operate multiple establishments, allowing us to identify their location more precisely.[5] The regression discontinuity design allows us to isolate the effect of the policy from broader macroeconomic trends that favoured Southern Italy during the post-pandemic period, in particular the increase in domestic demand, the large-scale incentives for the construction sector, and the increase in public-sector employment after a long hiring freeze (see Accetturo et al., 2025). Furthermore, conducting the analysis at the firm level also allows us to perform an extensive series of robustness checks, in particular by controlling for two-digit sectoral trends and for the receipt of other subsidies, using information from the National Aid Registry.

Our results show that the policy did not have a significant impact on employment or wages on average across firms. It did lower labor costs as intended, however, which resulted in increased firms' profitability. These gains were primarily used to strengthen liquidity rather than boost investment. Given uncertainty stemming from both the policy's design and the broader macroeconomic context, firms likely perceived this increase in profitability as temporary. As a result, they opted to reinforce their financial position rather than commit to new investments. This aligns with robust empirical evidence showing that uncertainty discourages investment (Guiso and Parigi, 1999; Kumar et al., 2023) and that firm subsidies or tax cuts are less effective in stimulating investment when policy duration is uncertain (Ábrahám et al., 2024) or when macroeconomic uncertainty is high (Guceri and Albinowski, 2021). We also find a positive effect of DS on revenues, concentrated among more labor-intensive firms, which are more likely to experience competitiveness gains from lower labor costs. These effects emerge only in 2023, a period of persistently high inflation, when the ability to reduce prices or to implement more moderate price increases translated into stronger competitive advantages.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 outlines the institutional framework and examines the reasons for the lower-than-expected take-up. Section 4 presents the empirical strategy to assess the impact of DS on firm-level outcomes and discusses the results. Section 5 concludes.

[4] See also the column by E. di Porto and P. Naticchioni "Occupazione e stipendi: che effetto fanno gli incentivi per le imprese", lavoce.info, November 2023.

[5] In our data, firms are geolocated based on their legal address; for companies with several operational sites, this may introduce measurement error. Furthermore, balance-sheet information is not available at the establishment level, limiting our ability to analyze the full set of firm outcomes. For employment and wages, however, we also conduct a robustness check including larger firms by using a smaller sample of data that allows us to identify individual plants.

2. Data on *Decontribuzione Sud*, other State aids, and firms' performance

As we lack direct access to the entire administrative archives covering all worker-employer relationships in Italy, we use two different samples drawn from the Social Security Institute (INPS) data, currently available until year 2023. Each sample has limitations, and therefore we combine them to analyze all relevant aspects:

- **INPS-BdI**: covers all employer-employee relationships for a selected sample of private firms. The sample includes all firms participating to the Bank of Italy's INVIND survey, which is focused on larger companies with at least 20 employees. It also includes a sample of other firms (of any size), drawn to ensure representativeness. The dataset encompasses nearly 115,000 enterprises, representing about 25 per cent of the economy in terms of value added. We use this dataset for analyses that require information on the whole workforce at the firm level, bearing in mind that it is more representative for larger firms. To preserve complete employment histories, the dataset also includes employment spells in non-sampled firms. In the relevant period for our analyses (October 2020 to December 2023) it contains approximately 9 million workers.[6] For each individual, we observe whether they received DS and in which months.
- **INPS-Imp**: encompasses the universe of Italian firms, but provides only aggregate information at the firm level about the number of employees and their compensation, by job position. It does not envisage additional information on DS.

Both datasets identify incorporated businesses with their fiscal code and therefore can be matched with other administrative archives. Non-incorporated businesses (individual firms) cannot be matched as their identifier is encrypted.

The other main data source we use is the National Registry of State Aids (**RNA**), which contains the universe of State aids – as defined by EU legislation – received by Italian firms. Since the DS measure is selective (as it only applies to firms operating in Southern Italy), it is classified as a State aid and is therefore covered by this dataset. Using RNA we can identify all the companies that have benefited from DS at some point between 2020 and 2023, but we cannot determine the yearly level of a firm's take-up, nor whether the firm applied DS only to a fraction of its workforce.[7]

Finally, to evaluate the impact on firms' performance, we use **Cerved,** a dataset on the universe of incorporated firms periodically compiled by *Cerved group*, a service company specialized in business data. This archive is linked to INPS-Imp to recover firm-level information on employment levels and wages. The dataset contains no information on plants, as data is collected at the firm level.

3. Total costs and beneficiary firms

Law 178/2020 allocated €41.7 billion to DS over eleven years. Planned spending followed the time profile shown in Figure 3.1, remaining at an average of €5.5 billion per year until 2025, before gradually declining in line with the reduction in tax cuts.[8] Due to the EU's decision not to extend the Temporary Frameworks for the COVID-19 and Ukrainian crises, DS in its original form ended in 2024. The Budget Law 2025 re-introduced DS, with a different progression of tax rate cuts and specific provisions explicitly targeting SMEs.[9]

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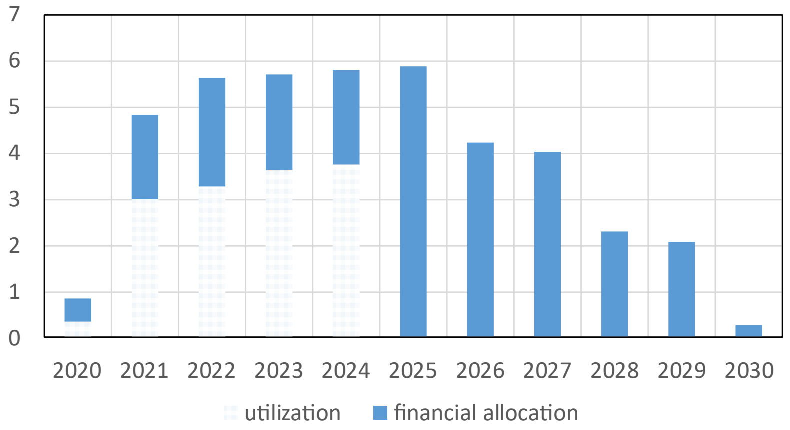
[6] Some workers have been eliminated from the dataset, as the firms for which they were working were not eligible for DS. In particular, we have dropped the employees of: (i) the firms operating in sectors 1-3, 64-66, 97 and 99 of the NACE Rev. 2 classification; (ii) a small set of very large companies created as a result of a privatization process.

[7] Fruition date is imperfectly coded in RNA. We can only determine whether the firm had benefited from a certain wave of DS (corresponding to subsequent extensions of the measure under different Temporary Frameworks), as identified by specific administrative codes.

[8] This is just above 3% of total compensation of employees in the South of Italy, which is 162, 173 and 182 billion euros for 2021, 2022 and 2023, respectively (Conti Territoriali, ISTAT).

[9] Starting in 2025, the new DS measure offers a 25% reduction in employers' social security contributions, capped at €145 per month per employee, for SMEs in Southern regions. The relief decreases over time: 20% in 2026 and 2027, and

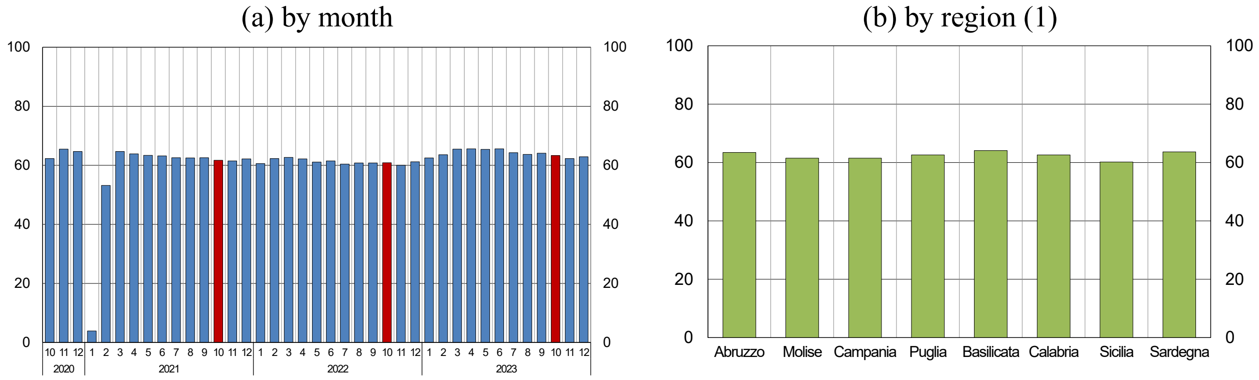
Fig. 3.1 – Available funds and utilization by year, billion euros



Source: Law 178/2020 and INPS.
Note. Utilization data is available only up to 2024

In the four full years of implementation, actual utilization of DS funds ranged from €3 billion in 2021 to €3.7 billion in 2024, substantially lower than the available resources (around 60%).[10] The level of expenditure closely reflects the take-up rate of the measure. In its 2023 Annual Report, INPS estimated that as of October 2022, approximately 64% of eligible employment relationships benefited from the DS tax cut.

Fig. 3.2 – DS take-up



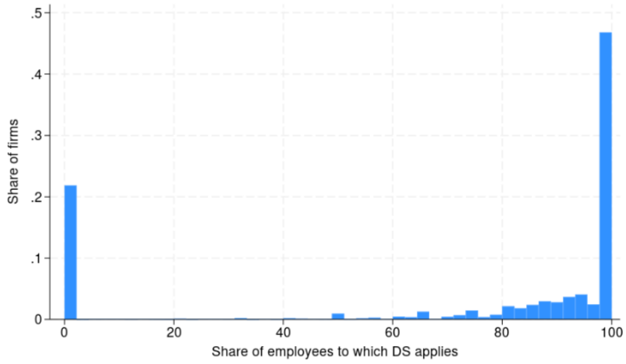
Source: INPS-BdI.
(1) Data representing the average monthly take-up for October, calculated over the period from 2020 to 2023.

Our estimates broadly confirm this figure: according to INPS-BdI data, the DS measure was applied to 60% of workers in the sample over the period October 2020–December 2023. The policy excluded jobs in agriculture, finance, and private household employment of domestic staff, so these sectors are also excluded from our analysis.

15% by 2029. The measure applies to all permanent employees in SMEs, hired by the end of the previous year. Large companies can also benefit from the measure, but only if they demonstrate an increase in the total number of permanent employees; at the moment, this provision for large companies is pending and will become effective only upon authorization from the European Commission (see Law 207/2024, art. 1 cc 406-426).
[10] In 2020 DS was introduced during the year. The reduced budget and some initial administrative difficulties translated into a low utilization of DS.

The take-up rate remained fairly stable over time (Figure 3.2.a), except in January and February 2021, when utilization was lower due to delays in TF authorization by the European Commission. Looking at October 2022 alone, the take-up rate was 61%, roughly in line with the INPS Annual Report estimate.[11] DS utilization also varied only slightly across regions (Figure 3.2.b), with Abruzzo, Basilicata and Sardegna recording the highest average take-up rate (nearly 64% on average from 2020 to 2023) and Sicily the lowest (approximately 60%).

Fig. 3.3 – Share of firms per DS usage



Source: INPS-BdI. The data refer to October 2023 and include exclusively firms within the representative sample with employees located in Southern regions, limited to sectors eligible for the application of DS.

The reasons behind the lower-than-expected take-up should be examined primarily at the firm level, as most of the underutilization stems from companies that did not take advantage of the DS benefit at all. The majority of firms in the sample fall into two distinct groups: those that fully applied DS to their entire workforce and those that did not use it at all. Only a small subset of companies applied DS to only part of their employees (Figure 3.3). On average, among beneficiary firms, DS covered 89% of the workforce.

Beyond the exclusion of specific sectors, firms were also ineligible for DS if they had not been regularly paying social contributions. This was a key requirement that significantly limited take-up. Compliance had to be verified through an attestation (DURC), issued by INPS, the same institution responsible for approving DS applications. As a result, the rule was strictly and automatically enforced. Although our microdata do not include information on DURC or social-security compliance, aggregate figures from INPS yearly Social Reports indicate that 23% of firms in the South were non-compliant with social security payments in 2023 and therefore lacked a DURC. This could potentially explain a large portion of the 22% of firms (unweighted estimate) that did not apply for DS.

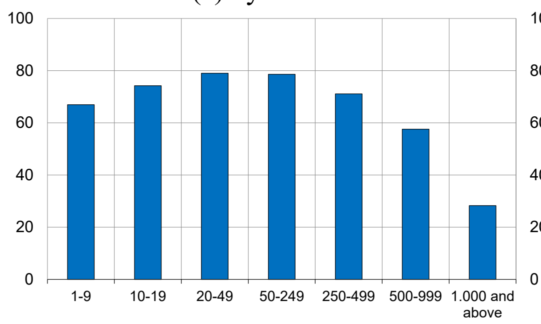
The relevance of DURC is confirmed by the lower take-up among micro-firms (Figure 3.4.a), which are generally less likely to be compliant with social-security payments. DS take-up is also lower in sectors such as tourism (Figure 3.4.b), where the informal economy is more widespread, leading to fewer firms – especially SMEs – having a DURC.[12] In these sectors and among micro-firms, it is also more common not to implement the most representative collective bargaining agreements, pay workers below the prescribed minimums, or adopt minor collective contracts with lower wages. These were additional statutory requirements for being eligible for DS.

[11] Throughout the paper we often take October as our month of interest, to directly compare with the figures released by INPS. Take-up rate was 63% in October 2023.

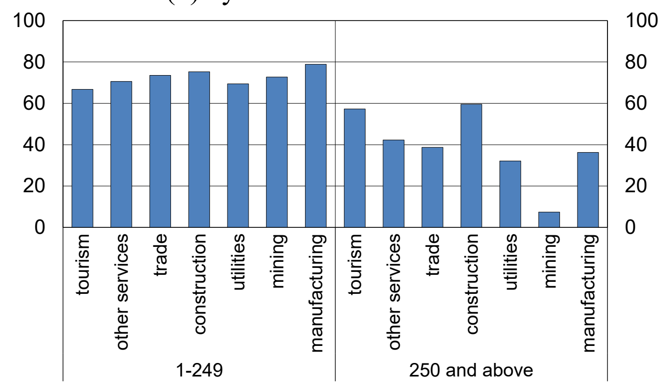
[12] According to National Accounts, in 2023 the share of irregular workers in the hotels and restaurants sector was 14.4%, above the national average of 10%. The construction sector, despite employing an above average share of irregular workers as well, likely was not constrained by the DURC requirement because of other large and concurrent policies (S*uperbonus*) that also required to be DURC compliant.

Fig. 3.4 – DS take-up by firm characteristics (1)

(a) by firm size



(b) by sector and SME status



Source: INPS-BdI.
(1) Data representing the average monthly take-up for October, calculated over the period from 2021 to 2023.

Take-up was also lower among firms in the largest size brackets. A contributing factor may be the size of the aid ceilings, which for companies with many employees can be reached quickly if the contribution relief is applied across the entire workforce.[13] In such cases, the measure may offer only limited scope for application, reducing its overall appeal to largest firms. The appeal would be even lower for firms that were already approaching ceilings, having received other forms of State aid. However, this constraint likely applied only to very large firms, as the State aid limits were set at historically high levels under the TFs.[14] According to our estimates, this limitation applied to 3.7% of workers who did not receive the DS tax cut, despite being in eligible sectors. If we also consider firms that were close to reaching the State aid limit (less than 20% below it), the share of affected workers rises to almost 11%. These figures are notably higher for firms with 250 or more employees.

Economic conditions also played a significant role. The law specified that firms in financial distress could not apply for DS.[15] Table 3.1 presents a regression of a dummy for DS use on firm-level characteristics, using the Cerved dataset. The explanatory variables are arranged in the columns. In addition to confirming the U-shaped relation with employment size, the table shows that take-up was higher among less risky[16] and more profitable firms, as well as those with higher ex-ante investment and labor productivity. Take-up was also higher among those with higher leverage, but this result is conditional on their riskiness as captured by the rating. Overall, these results show that the lower-than-expected take-up is also partly due to firms with struggling economic performance.

Tab. 3.1 – Characteristics of firms using DS

	log empl.	(log empl.)^2	log VA p.w.	log investment	log rating	ROA	leverage
DS use	0.0586***	-0.0059***	0.0290***	0.0031***	-0.1014***	0.00016***	0.00002*
	(0.00171)	(0.00037)	(0.00167)	(0.00070)	(0.00238)	(0.00006)	(0.00001)

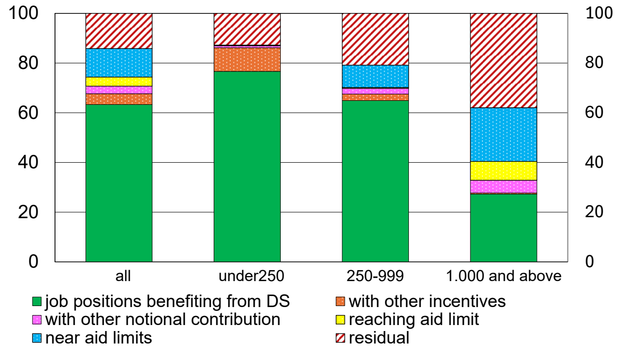
Source: own elaborations on RNA, INPS and Cerved data for the years 2017-19.
Notes: only firms eligible for DS are considered in the sample. Year, sector (2-digit Nace Rev. 2) and region fixed effects included. 188,560 observations included. * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

[13] Assuming an average gross wage of €1,800 per month (based on INPS data from the Observatory on non-agricultural employees) and a contribution rate of 27%, the 30% reduction under the DS scheme corresponds to approximately 8.1% of gross wages, or about €150 per worker per month. Over the duration of the two Temporary Frameworks (COVID-19 from October 2020 and June 2022 and Ukraine from March 2022 and December 2023), which provided aid ceilings of €2.3 million and €2 million respectively, a firm that did not access any other support could have applied the DS relief to an estimated range of 600 to 750 employees. For firms exceeding these workforce sizes, the ceilings could therefore be reached quickly, limiting the scope of the measure.
[14] The State aid limit varied through time and across TFs, hitting 2.3 million euros per firm at its peak.
[15] As per Article 2(18) of Regulation (EU) No 651/2014, an undertaking is in difficulty if it has significant capital losses, is insolvent or close to insolvency, or has received unrepaid rescue or restructuring aid. Micro and small enterprises, however, may still qualify for aid if not under insolvency proceedings and without prior rescue or restructuring aid.
[16] The 'rating' variable is increasing in the riskiness of a firm.

Another possible explanation relates to displacement effects from other incentives. Our analyses suggest that these effects are present but relatively limited in size. The use of other forms of incentives was much lower than DS, covering only about 2% of the workers in the sample over the observation period. This is consistent with the fact that these incentives targeted specific categories (such as young and female workers) and were limited to new hires. The use of other incentives increased over time and was associated with a reduction in the concurrent use of DS. However, overall, only about 4% of the workers who did not benefit from DS received another type of incentive.

Apart from incentives, workers may have not been eligible for DS because they were on sickness leave. However, these factors were very marginal, affecting less than 1% of positions not covered by DS. A more significant factor relates to the use of job retention schemes (Cassa Integrazione Guadagni, CIG), as workers in these schemes could not access DS. The relevance of this factor was more dependent on the economic cycle, as it affected 6.9%, 3.2% and 3.0% of workers not covered by DS in October 2021, 2022 and 2023, respectively.

Fig. 3.5 – Share of eligible workers to which the DS was not applied by potential reasons



Source: INPS-BdI.
Note: "Other notional contributions" refers to the use of CIG or to long absences due to illness or injury. "Near aid limits" means that the firm in which the worker was employed had accumulated State aids totaling 80% or more of the EU limit. Data referring to October 2023.

Figure 3.5 summarizes all the potential reasons for low take-up that we have been able to quantify. For bigger firms, missing take-up is largely explained by State aid limits, while for smaller firms, the use of other incentives is more prevalent in relative terms. However, there remains a significant share of missing take-up (around 14% for the entire sample) that remains unexplained.[17] DURC compliance, which we cannot directly observe, likely accounts for a large part of this, especially for smaller firms. Instead, for the largest ones (particularly those with more than 1,000 employees) the limited size of the aid ceilings may also have played a role in discouraging firms from accessing the policy.[18]

4. The impact on firms

In this section, we investigate the economic effects of DS on firms located in Southern Italy. We focus on firm-level outcomes such as employment, wages, labor costs, revenues, profitability, and investment decisions. To isolate the effect of the policy, we use a geographic-based identification strategy, concentrating on firms located near the administrative border between Southern and Northern Italy, where eligibility for DS was determined.

4.1 Descriptive evidence and empirical strategy

A simple comparison of the economic performance between users and non-users of DS would likely return a biased estimate of DS impact.

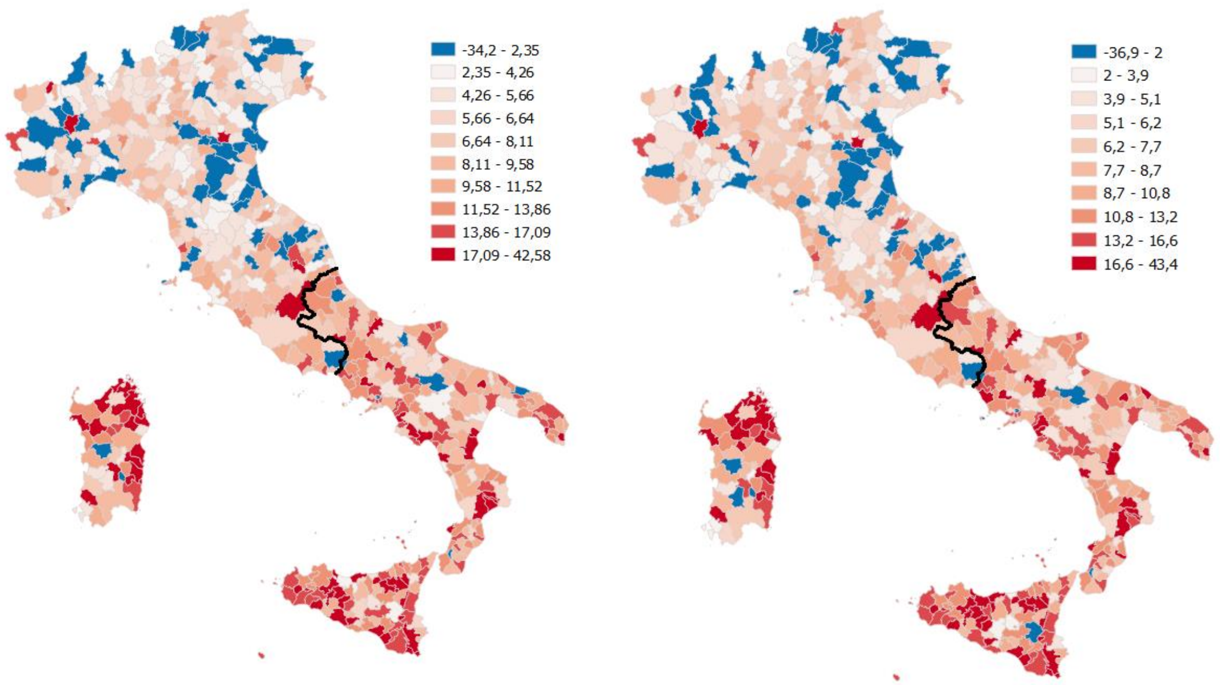
Firstly, the key difference between users and non-users is whether they are located in the South. An area-wide comparison between firms in the South and the Centre-North would be biased by other macroeconomic factors that unevenly influenced these two areas. Indeed, between 2019 and 2023, Southern Italy showed a better macroeconomic performance compared to the Centre-North (5.9% vs. 3.4% GDP growth; 4.5% vs. 1.7% employment growth). The higher growth was widespread across Southern local labor markets (LLMs) (Figure

[17] We can complement these estimates using Bank of Italy's INVIND survey which features a sample of 1,330 firms that were likely to be eligible for the policy in 2023-24, on the basis of their location and sector. About 5 per cent of respondents declared to lack some requirement to obtain the relief, 3 per cent was deterred by regulatory uncertainty and another 6 per cent was just not interested (rising to 17 per cent among firms with more than four fifth of the workforce based in the Centre-North).
[18] See footnote 11 for a discussion of the limited scope of the policy for larger firms.

4.1), but no clear pattern emerges along the North-South administrative border, which also marks eligibility for DS. Moreover, the better macroeconomic performance of Southern regions was largely explained by stronger growth in the construction sector, driven by substantial public subsidies, and by the lifting of hiring restrictions in the public sector. Excluding these two sectors, the growth gap shrinks, though it remains more favorable for the South.

Fig. 4.1: Employment growth by LLM in 2019-23

(a) total economy (b) only eligible sectors



Source: Istat ASIA.
Note: The solid black line represents the Southern regions' border.

Secondly, in the firm-level data we cannot observe some key eligibility conditions, particularly whether firms were regularly paying social contributions and met the, albeit loose, condition of "not being in financial distress".[19] This means that firms that used DS are more likely to have better economic and financial standing than those that did not. Therefore, a simple comparison with other firms would likely overestimate the true effects of DS.

Considering these two issues, it is not surprising that a simple regression of firm outcomes on the use of DS (which covered a large fraction of firms in the South), conducted on the entire national sample of Cerved firms, reveals significant positive associations with changes in economic indicators (Table 4.1).

[19] The regulation on DS establishes that the contribution relief is not applicable to firms classified as being in financial difficulty. To identify such companies, we applied the definition set out in EU Regulation No. 651/2014, excluding from the analysis those firms that, in 2019, suffered losses exceeding half of their subscribed share capital as a result of accumulated deficits.

Table 4.1 – Economic performance of firms that used the DS

	(1)	(2)	(3)	(4)
	Change in logs over 2019-23			
	Revenues	Employment	Total labor cost (per-cap.)	Fixed assets
DS use	0.102***	0.085***	-0.010***	0.119***
	(0.002)	(0.002)	(0.001)	(0.004)
Obs.	467,848	408,378	406,726	434,358

Source: own elaborations on RNA, INPS-Imp and Cerved data.
Notes: OLS estimates. Only firms in sector eligible for DS considered in the sample. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Firms that used the DS are identified by means of the RNA archive and are those who used the DS over the entire period.

To minimize these sources of bias, in the rest of the analysis we use a spatial regression discontinuity design, focusing on firms located near the North-South administrative border. Although this border is also used to determine eligibility for other place-based policies, we argue that the main change that occurred during 2019-23 was the introduction of DS. Had this policy not been introduced, there would have been no other reasons to expect differential trends for firms located on either side of the border.

We therefore compare the economic performance of firms located just South of the border with those located just North of it. Given that not all firms located in the South took up the incentive, we use a fuzzy-RDD strategy, where the receipt of DS is instrumented by the sharp jump in eligibility at the administrative border. This amounts to rescaling the reduced form RDD by a first stage where the outcome of the RDD is the treatment variable.

We use the RDD locally linear estimator with second-order-polynomial bias correction and robust standard errors proposed by Calonico et al. (2014).[20] The bandwidth is also chosen using the optimal method suggested by Calonico et al. (2020), which can vary from outcome to outcome.[21] The fuzzy RDD estimates two equations:

$$\Delta y_i = \beta^R South_i + f^R(d_i) \times South_i + g^R(d_i) \times (1 - South_i) + \epsilon_i^R \qquad \text{(1) - Reduced form}$$

$$DS_{\mathrm{i}} = \beta^F South_i + f^F(d_i) \times South_i + g^F(d_i) \times (1 - South_i) + \epsilon_i^F \qquad \text{(2) - First stage}$$

where Δy_i is the change of a certain outcome for firm *i* over years 2019-23; $South_i$ is a dummy for the firm being located in the South; d_i is the distance from the border, calculated using geolocated data for the universe of Italian firms in 2019; DS_{i} is a dummy for having used the DS over the entire period 2020-23.[22] The fuzzy RDD estimator amounts at dividing the jump in the outcome at the threshold estimated using the reduced form (β^R) by the jump in the use of the policy instrument estimated using the first stage (β^F). As in any RDD strategy, ϵ_i^R needs to be mean independent from $South_i$: this requirement is satisfied if we assume that, in the absence of the policy, firms located at either side of the border would have had, on average, parallel trends in outcomes (Δy_i).

[20] We also followed Pei et al. (2022) to evaluate whether we should use a locally linear estimator or a higher-order one. We always find that a locally linear estimator is preferable, but results are virtually unchanged (Table A.1). Furthermore, Table A.2, panels a-b, shows that our results remain unchanged when using a conventional local polynomial RDD estimator instead of the bias-corrected one introduced by Calonico et al. (2014), or when applying a conventional heteroskedasticity-robust standard error estimator rather than the robust version proposed by Calonico et al. (2014). Finally, Table A.2, panel c, demonstrates that our estimates remain similar when adopting a global linear polynomial.

[21] The bandwidth selected using this method typically ranges between 40 and 100 kilometers. As a result, the procedure excludes more distant regions—beyond those already excluded by design, such as Sicily and Sardinia (see Figure A.1). At the same time, it does not necessarily select areas that lie exactly along the North-South border for comparison.

[22] Some firms made use of DS only for part of the period, accounting for approximately 25% of all DS users. Although they are excluded from the baseline analysis, the results remain qualitatively unchanged when these firms are included.

In most cases, Δy_i is calculated as log-differences, hence the effect can be interpreted in terms of log-points or, under certain assumptions, as the approximate percentage change in the outcome due to the policy.[23] For indices, such as the return-on-equity (ROE), the difference is taken in levels. All outcomes have been winsorized at 5% (i.e. bottom- and top-censored) to avoid strong outliers, but results are similar without this correction.

As discussed, in our data the geolocalization of firms refers to their "legal" address. We therefore limit the analysis to Small and Medium-Sized Enterprises (SMEs), as they are the least likely to have multiple establishments. Multi-plant firms could introduce measurement error in our estimates, as companies registered in the North might still partially benefit from DS if they have establishments in the South.

We further exclude firms located in regions far from the administrative border.[24] Finally, we restrict the sample to those firms that had at least one employee in 2019 and are present in the archive over the entire 2019-23 period. Firms with missing values in the explanatory variables are excluded.

The main disadvantage of this approach is that RDD estimates should be interpreted as local estimates, meaning they capture the effect of the policy on the compliers located near the border. Additionally, due to the sample restrictions, we are not accounting for the effects on large firms or on firm creation and destruction.

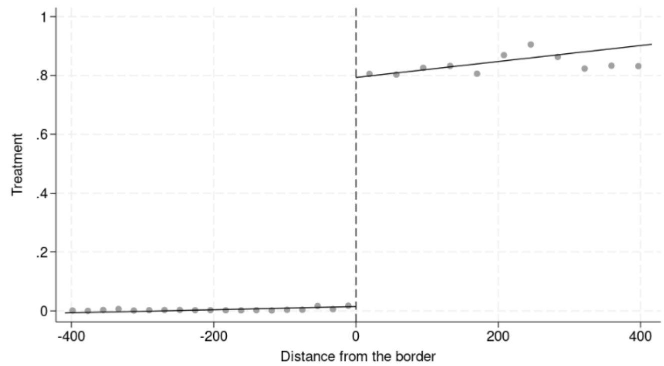
4.2 Main results

Figure 4.2 displays the first-stage relationship between the running variable and the probability of using DS. The graph shows a clear increase in the likelihood of receiving the treatment at the cutoff, consistent with the fuzzy RDD framework. As expected from the discussion in Section 3, the discontinuity is not sharp, but the jump at the threshold confirms a significant first-stage effect, which is crucial for the validity of the instrumental variable strategy. The smoothness of the fitted lines on both sides of the cutoff also provides visual evidence against manipulation of the running variable.

Table 4.2, Panel A, shows the main results. The baseline estimates, which do not include any covariates, highlight a zero effect on employment and average wages (net of employers' social contributions). Conversely, total labor costs per employee decrease; the effect, equal to 4.2%, substantially aligns with expectations.[25] There are also positive effects on revenues and value added per worker, which – as we further argue in Section 4.4 – are likely driven by an increased price competitiveness of treated firms.

The reduction in costs and the increase in revenues leads to higher firms' profitability: both EBITDA and return on equity increase among beneficiaries. The higher profitability is mostly used to increase liquidity, while investments and fixed assets remain unchanged.

Fig. 4.2 – Probability of treatment around the border



Note: Only firms in sector eligible for DS are considered in the sample. Firms that used the DS are identified by means of the RNA archive and are those who used the DS over the entire period.

[23] The expected difference in logarithms is directly related with the multiplicative effect on the expected outcome in levels only if ϵ_i^R is statistically independent from $South_i$, while for the estimates to be consistent for the effect in log-points we only need ϵ_i^R and $South_i$ to be mean independent.

[24] In particular, regions more than 400 km away from the border are excluded (see Figure A.1).

[25] If a firm uses DS for its entire workforce and there is no change in quantities and prices, labor costs should decrease by 5.6% (see footnote 1). However, a slightly lower value could be due to some firms not using it for their entire workforce.

Introducing sectoral dummies to account for sector-specific trends (Panel B) leaves the results largely unchanged; an exception concerns revenues, where the estimated coefficient is attenuated and its statistical significance is slightly reduced.

The key identifying assumption in our spatial RDD strategy is that, absent the policy, firms located on either side of the border would have exhibited similar economic performance. In support of this argument, Table 4.3 shows no significant differential trends in the pre-policy period (2016-19). Nevertheless, if firms located south of the border in 2019 differ somewhat from those to the north, identification could still be threatened insofar as such differences translate into divergent trends. Table 4.4, however, indicates that there are no statistically significant discontinuities at the border (at the 5% level) for 10 out of the 11 characteristics measured for firms operating in that year. A significant coefficient is observed only for total assets, which could be related to pre-existing incentive policies that were historically more widespread in the South and continued throughout the period of analysis without major changes. To address the possible concern that this difference might confound the result, Table 4.5 Panel A shows that results are virtually unchanged when controlling for all lagged outcomes in 2019.

Table 4.5 includes further robustness checks. Panel B includes segment fixed effects to account for the uneven distribution of treated and control firms along the border. Results are consistent with our main specification. Panel C implements a donut-hole RDD specification, excluding firms located closest to the border to mitigate potential spillover effects and geolocation measurement error. Only the result on revenues becomes weaker, indicating that the firms located closest to the border might be those who have mostly benefitted from the advantages stemming from the reduction in labor costs.

As a further validation exercise, Table 4.6 presents estimates based on different time windows starting from 2019. The results are broadly consistent with the policy's timing. In 2020 no relevant effects are observed for treated firms on labor costs, profitability, or liquidity, reflecting the fact that DS was introduced in October. These effects become significant (at 5% level) only in the 2021–22 period, when the policy becomes fully operative. Finally, the effects on revenues and productivity only materialize from 2023, when the persistence of elevated inflation may have provided treated firms with greater scope for price competitiveness (see also Section 4.4).

Tab. 4.2 – Effect of the *Decontribuzione Sud* on firms' performance over 2019-23

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. BASELINE = FUZZY RDD - LOCAL LINEAR CALONICO - NO COVARIATES - OPTIMAL BW											
DS use	0.051**	-0.023	0.001	-0.042***	-0.061	0.047**	-0.021***	0.018***	0.040***	0.032**	0.000
	(0.023)	(0.021)	(0.008)	(0.009)	(0.046)	(0.018)	(0.005)	(0.004)	(0.013)	(0.013)	(0.005)
Panel B. FUZZY RDD - LOCAL LINEAR CALONICO - CTRLS ATECO2D - OPTIMAL BW BASELINE											
DS use	0.043*	-0.026	-0.000	-0.045***	-0.068	0.038**	-0.020***	0.017***	0.036***	0.029**	0.000
	(0.022)	(0.021)	(0.008)	(0.008)	(0.045)	(0.018)	(0.004)	(0.004)	(0.013)	(0.013)	(0.005)
Total obs.	139,531	131,953	131,953	131,953	132,039	131,953	139,531	139,531	139,531	139,152	139,531
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3
Effective obs.	51,388	44,808	61,101	64,434	46,089	54,514	31,945	64,481	56,906	46,333	63,335

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The optimal bandwidth is calculated using Calonico et al (2020) in the baseline specification. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Only firms in sector eligible for DS considered in the sample. Firms that used the DS are identified by means of the RNA archive and are those who used the DS over the entire period.

Tab. 4.3 – Pre-trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2016-19					Change in levels over 2016-19					
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
South	-0.018	-0.023	0.007	-0.009	0.026	0.003	-0.003	0.003	-0.013	0.006	-0.002
	(0.014)	(0.015)	(0.006)	(0.007)	(0.028)	(0.013)	(0.003)	(0.003)	(0.011)	(0.008)	(0.003)
Total obs.	143,428	134,848	134,848	134,848	136,223	134,848	143,428	143,428	143,428	143,022	143,428
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3
Effective obs.	53,932	46,796	63,761	67,243	48,746	56,816	32,834	67,678	59,695	48,560	66,505

Note: Sharp RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The bandwidth is those obtained using Calonico et al (2020) in the baseline specification of Table 4.2. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Tab. 4.4: Balancing outcomes in 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
South	-72.8	0.758	0.569	-0.660	350.4***	0.609	0.009	-0.001	-0.049*	-0.011	-0.008
	(307.5)	(0.482)	(0.360)	(0.851)	(91.2)	(3.717)	(0.008)	(0.004)	(0.028)	(0.051)	(0.006)
Total obs.	208,202	208,202	208,202	194,246	208,202	194,246	208,202	208,202	208,202	207,682	208,202
Mean	2,366.8	9.290	17.917	30.350	594.7	60.682	0.240	0.129	0.167	0.353	0.699
Sd	18,685.7	17.896	10.198	68.481	6,155.1	462.925	0.279	0.181	1.502	3.533	0.226
Bandwidth	93.8	59.9	36.0	37.8	54.8	31.0	71.7	92.0	62.6	62.1	85.4
Effective obs.	103,711	77,694	17,120	17,103	63,077	12,734	87,784	103,157	80,495	79,762	98,736

Note: Sharp RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). Optimal Bandwidth calculated using Calonico et al (2020). Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All monetary amounts are reported in thousands of euros.

Tab. 4.5 – Other robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. CTRLS FIRM CHARACTERISTICS											
DS use	0.068***	-0.007	0.010	-0.036***	-0.066	0.050***	-0.016***	0.017***	0.034***	0.023*	0.000
	(0.023)	(0.020)	(0.007)	(0.008)	(0.045)	(0.018)	(0.004)	(0.004)	(0.012)	(0.012)	(0.005)
Effective obs.	49,012	44,744	61,011	64,342	44,253	54,430	30,412	61,609	54,312	44,251	60,514
Panel B. CTRLS SEGMENT FIXED-EFFECTS											
DS use	0.054**	-0.018	0.005	-0.035***	-0.050	0.043**	-0.017***	0.016***	0.026*	0.021	-0.001
	(0.024)	(0.021)	(0.008)	(0.009)	(0.046)	(0.019)	(0.005)	(0.004)	(0.013)	(0.013)	(0.006)
Effective obs.	51,388	44,808	61,101	64,434	46,089	54,514	31,945	64,481	56,906	46,333	63,335
Panel C. DONUT HOLE RDD											
DS use	0.049*	-0.030	-0.004	-0.056***	-0.027	0.055**	-0.026***	0.023***	0.060***	0.035**	-0.005
	(0.028)	(0.026)	(0.009)	(0.010)	(0.055)	(0.022)	(0.006)	(0.005)	(0.015)	(0.015)	(0.006)
Effective obs.	50,030	43,518	59,811	63,144	44,797	53,224	30,587	63,123	55,548	44,983	61,977
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The bandwidth is those obtained using Calonico et al (2020) in the baseline specification of Table 4.2. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Panel (a): The full set of lagged firm outcomes. Panel (b): Five border segment fixed effects are included as controls. Panel (c): Firms located within 5 km of the border are excluded.

Tab. 4.6 – Different time-windows

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs						Change in levels				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. PERIOD 2019-2020											
DS use	-0.025	-0.014	0.014*	-0.012	-0.041*	-0.010	-0.007*	0.001	0.012	-0.001	0.004
	(0.017)	(0.011)	(0.008)	(0.014)	(0.022)	(0.018)	(0.004)	(0.005)	(0.015)	(0.016)	(0.005)
Total obs.	171,445	160,765	160,765	160,765	162,638	160,765	171,445	171,445	171,445	170,847	171,445
Bandwidth	50.4	64.7	70.2	36.4	47.2	61.5	49.9	49.9	68.4	37.8	38.2
Effective obs.	42,444	61,114	64,075	13,587	30,504	58,839	41,149	41,022	68,138	15,325	15,550
Panel B. PERIOD 2019-2021											
DS use	-0.029	-0.018	-0.001	-0.037**	-0.051	-0.012	-0.008**	0.004	0.051***	0.025**	0.009*
	(0.021)	(0.016)	(0.012)	(0.015)	(0.033)	(0.023)	(0.004)	(0.005)	(0.014)	(0.012)	(0.005)
Total obs.	159,629	150,150	150,150	150,150	151,176	150,150	169,364	169,364	169,364	168,811	169,364
Bandwidth	48.2	52.1	36.1	34.8	52.3	41.4	55.3	50.9	70.2	67.6	49.2
Effective obs.	32,150	39,101	12,446	11,792	39,361	16,309	51,830	43,580	68,438	66,718	38,432
Panel C. PERIOD 2019-2022											
DS use	0.002	-0.018	0.002	-0.045***	-0.042	0.012	-0.017***	0.013***	0.039***	0.022*	0.004
	(0.021)	(0.018)	(0.007)	(0.009)	(0.040)	(0.018)	(0.004)	(0.005)	(0.014)	(0.012)	(0.005)
Total obs.	149,517	141,110	141,110	141,110	141,397	141,110	166,572	166,572	166,572	166,013	166,572
Bandwidth	55.1	56.5	84.4	77.0	56.9	62.6	49.3	77.4	69.2	73.7	92.3
Effective obs.	43,981	43,350	62,301	58,072	43,544	51,156	38,298	71,264	66,777	69,326	79,849

Note: Fuzzy RDD estimates. Optimal Bandwidth calculated using Calonico et al (2020). Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4.3 Accounting for other policies

We also conducted an additional sensitivity check using information from all subsidies recorded in the National Aid Registry. For each subsidy, we define a dummy equal to one if the firm received the subsidy during 2019-23. It is important to stress that these subsidies might have been available before this period and/or do not necessarily differ along the North-South border. For each subsidy dummy, we first check for the presence of discontinuity at the border using a sharp RDD regression, as in equation 2. Out of 338 subsidies, only 51 show some significant difference (at 5% level).[27] To understand whether these discontinuities bias our results, we add a dummy for the receipt of these subsidies in our main regressions (Table 4.7, Panel A). The results are largely unchanged, with the partial exception of revenues, for which the estimated coefficient decreases and loses statistical significance. It should be noted, however, that the estimated effect remains positive and close to the specification including sectoral dummies (Table 4.2, panel B).

Another potential confounder is represented by Covid-related restrictions, which hit different municipalities with heterogeneous intensities according to the spread of the pandemic. In Table 4.7, Panel B, we control for the number of days the municipality where the firm is located was subject to full mobility restrictions (the so-called "red zone"). Again, the results are virtually unchanged.

Lastly, EU Cohesion Policy funds flow to Southern municipalities more intensely than to those in the Centre-North. Although the allocation of these funds did not change over the period of interest, and the boundary separating "less developed" regions (which receive a larger share of EU funds) has not coincided with the North-South border since 1996, we also checked whether introducing the amount received per capita as a control leads to different results. Table 4.7, Panel C shows that this is not the case.

4.4 Labor intensity and wage levels

By reducing overall labor costs, the policy may have benefited firms with higher labor intensity. For these firms, lower labor costs could significantly enhance competitiveness, allowing them to either reduce prices or implement more moderate price increases during the inflationary period that started in the second half of 2021 and peaked at the end of 2022.

Our results support this hypothesis. When labor intensity is measured by the ratio of capital to employment in 2019 (Table 4.8), we find that DS had a positive impact on revenues only for firms with lower capital deepening. Analogously, when labor intensity is measured as the ratio of labor costs to total revenues in 2019 (Table 4.9), a positive impact on revenues appears only for firms with a higher labor cost ratio.

On average, our main results indicate that savings did not translate into higher wages. However, Table 4.10 shows that firms starting from a relatively low wage compared to the sector average did translate part of the savings to their workers through higher wages. These firms also increased their revenues more and, as a result, the impact of DS on the ratio between labor costs and revenues does not differ across firms paying lower or higher wages.

[27] Among these 51 measures, only one program (the Covid-19 grants, received between October 2020 and June 2022) recorded a higher number of beneficiaries than DS, whereas all the others reached substantially fewer recipients. However, for this program, we estimate a small negative difference in the probability of obtaining the incentive for Southern firms at the threshold (−5.0%; the average probability is about 60%).

Tab. 4.7: Accounting for other policies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. CTRLS CONCURRENT POLICIES FROM THE NATIONAL AID REGISTRY											
DS use	0.038	-0.036	-0.000	-0.044***	-0.077	0.047**	-0.023***	0.018***	0.041***	0.024*	0.004
	(0.024)	(0.022)	(0.008)	(0.009)	(0.048)	(0.020)	(0.005)	(0.005)	(0.014)	(0.014)	(0.006)
Panel B. CTRLS FOR COVID RESTRICTIONS (“RED ZONE”)											
DS use	0.051**	-0.023	0.001	-0.042***	-0.061	0.047**	-0.021***	0.018***	0.040***	0.032**	0.000
	(0.023)	(0.021)	(0.008)	(0.009)	(0.046)	(0.018)	(0.005)	(0.004)	(0.013)	(0.013)	(0.005)
Panel C. CTRLS FOR COHESION POLICY											
DS use	0.046*	-0.021	0.001	-0.042***	-0.052	0.046**	-0.021***	0.019***	0.039***	0.034***	-0.000
	(0.023)	(0.021)	(0.008)	(0.009)	(0.045)	(0.018)	(0.005)	(0.004)	(0.013)	(0.013)	(0.006)
Total obs.	139,531	131,953	131,953	131,953	132,039	131,953	139,531	139,531	139,531	139,152	139,531
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3
Effective obs.	51,388	44,808	61,101	64,434	46,089	54,514	31,945	64,481	56,906	46,333	63,335

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The bandwidth is those obtained using Calonico et al (2020) in the baseline specification of Table 4.2. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Tab. 4.8: Heterogeneity results, by capital deepening tertile

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. LOW CAPITAL TO EMPLOYMENT RATIO (FIRST TERTILE)											
DS use	0.122***	-0.025	0.007	-0.026*	0.004	0.077**	-0.029***	0.023***	0.038	0.034	-0.001
	(0.043)	(0.038)	(0.013)	(0.015)	(0.095)	(0.032)	(0.009)	(0.009)	(0.027)	(0.023)	(0.010)
Effective obs.	19,107	17,563	23,294	24,426	15,867	21,052	11,688	23,520	20,971	17,312	23,166
Panel B. MEDIUM CAPITAL TO EMPLOYMENT RATIO (SECOND TERTILE)											
DS use	0.033	-0.029	0.002	-0.035**	-0.132*	0.024	-0.022***	0.007	0.009	0.023	-0.002
	(0.038)	(0.035)	(0.012)	(0.014)	(0.072)	(0.030)	(0.008)	(0.007)	(0.021)	(0.021)	(0.009)
Effective obs.	16,110	14,770	20,130	21,210	15,145	17,957	10,017	20,251	17,842	14,521	19,887
Panel C. HIGH CAPITAL TO EMPLOYMENT RATIO (THIRD TERTILE)											
DS use	0.015	-0.018	-0.001	-0.059***	-0.032	0.046	-0.013*	0.022***	0.056***	0.026	0.008
	(0.039)	(0.036)	(0.013)	(0.015)	(0.059)	(0.034)	(0.007)	(0.007)	(0.019)	(0.022)	(0.009)
Effective obs.	13,704	12,327	17,485	18,596	13,146	15,333	8,653	17,739	15,415	12,271	17,362
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The bandwidth is those obtained using Calonico et al (2020) in the baseline specification of Table 4.2. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Tab. 4.9: Heterogeneity results, by labor cost to revenue ratio tertile

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. LOW LABOR COST TO REVENUE RATIO (FIRST TERTILE)											
DS use	-0.010	-0.054	-0.007	-0.062***	-0.162*	0.011	-0.016***	0.007	-0.006	0.013	-0.018*
	(0.045)	(0.044)	(0.017)	(0.018)	(0.097)	(0.041)	(0.006)	(0.008)	(0.024)	(0.024)	(0.010)
Effective obs.	17,140	13,374	18,533	19,528	15,276	16,420	10,444	21,683	19,060	15,415	21,295
Panel B. MEDIUM LABOR COST TO REVENUE RATIO (SECOND TERTILE)											
DS use	0.059	-0.007	0.013	-0.034**	-0.049	0.032	-0.015**	0.022***	0.037*	0.010	0.005
	(0.039)	(0.034)	(0.013)	(0.014)	(0.075)	(0.031)	(0.007)	(0.007)	(0.021)	(0.022)	(0.009)
Effective obs.	16,306	14,938	20,410	21,570	14,999	18,213	10,194	20,530	18,110	14,696	20,175
Panel C. HIGH LABOR COST TO REVENUE RATIO (THIRD TERTILE)											
DS use	0.085**	0.000	-0.002	-0.035***	0.000	0.081***	-0.023***	0.024***	0.072***	0.065***	0.008
	(0.038)	(0.032)	(0.011)	(0.013)	(0.071)	(0.026)	(0.008)	(0.007)	(0.022)	(0.021)	(0.009)
Effective obs.	17,942	16,496	22,158	23,336	15,814	19,881	11,307	22,268	19,736	16,222	21,865
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The bandwidth is those obtained using Calonico et al (2020) in the baseline specification of Table 4.2. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Tab. 4.10: Heterogeneity results, by wage level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel a. LOW WAGE FIRMS											
DS use	0.082***	-0.004	0.020*	-0.017	-0.060	0.053**	-0.017***	0.014**	0.023	0.020	-0.007
	(0.030)	(0.029)	(0.010)	(0.012)	(0.061)	(0.025)	(0.006)	(0.006)	(0.017)	(0.017)	(0.007)
Effective obs.	30,089	27,233	37,995	40,152	26,814	33,713	18,237	38,478	33,720	26,916	37,779
Panel b. HIGH WAGE FIRMS											
DS use	0.041	-0.030	-0.003	-0.067***	-0.056	0.048*	-0.020***	0.023***	0.057***	0.038**	0.014*
	(0.035)	(0.029)	(0.009)	(0.011)	(0.067)	(0.028)	(0.007)	(0.007)	(0.019)	(0.019)	(0.008)
Effective obs.	18,996	17,575	23,106	24,282	17,490	20,801	12,230	23,226	20,678	17,335	22,828
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The bandwidth is those obtained using Calonico et al (2020) in the baseline specification of Table 4.2. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4.5 Plant level evidence

Given that we do not have balance-sheet data at the plant level, our main analysis is conducted on data covering the universe of incorporated SMEs. Although this choice aims to minimize possible issues, we might miss the impact on plants that could benefit from the policy but are not included in the analysis because they belong to a firm located elsewhere. Furthermore, firms' geolocation information refers to the legal address, which may not correspond with the actual location of their activity.

For firms observed in the INPS-BdI sample, however, we can also recover some information about employment and wages at the plant level. This is because we observe all employee-employer job relations and the municipality where the job is located. This allows us to reconstruct total employment and average wage at the plant level, assuming firms have only one plant in each municipality and using the centroid of the municipality as geolocation (since we do not have the detailed address). In this case, the use of DS is also recovered directly from INPS archives, as we observe when DS was used for some workers.

In Table 4.11, Panel A reproduces the main analysis on this subsample, initially focusing on the same selection, i.e. SMEs geolocated using their legal address. The results are consistent with our main findings. Panel B conducts the analysis at the plant level, still restricting the sample to SMEs. The estimated effects are small and not statistically distinguishable from zero at conventional levels. Finally, given the different characteristics of this dataset, we can also consider plants owned by large firms to assess whether our earlier results extend to this group as well; Panel C confirms that they do.

Tab. 4.11: Effect of the *Decontribuzione Sud* on plants' employment over 2019-23

	(1)	(2)	(3)	(4)
	Change in logs over 2019-23			
	Employment		Average wage	
Panel a. SME FIRMS				
DS use	0.007	0.012	-0.006	-0.005
	(0.023)	(0.023)	(0.013)	(0.013)
Total obs.	51,503	51,503	51,495	51,495
Bandwidth	86.5	86.5	98.6	98.6
Effective obs.	19,198	19,198	20,734	20,734
Panel b. SME PLANTS				
DS use	-0.021	-0.013	0.016	0.017
	(0.025)	(0.025)	(0.014)	(0.014)
Total obs.	59,940	59,940	59,928	59,928
Bandwidth	54.4	54.4	69.9	69.9
Effective obs.	13,899	13,899	18,858	18,858
Panel c. OTHER PLANTS				
DS use	0.045	-0.030	-0.011	-0.019
	(0.094)	(0.093)	(0.050)	(0.049)
Total obs.	14,553	14,553	14,546	14,546
Bandwidth	108.7	108.7	112.4	112.4
Effective obs.	5,292	5,292	5,346	5,346
Covariates	NO	ATECO2D	NO	ATECO2D

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The optimal bandwidth is calculated using Calonico et al (2020) in the baseline specification of Columns 1 and 3. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

4.6 Survival

Finally, we examine whether the availability of DS increased firm survival. In this case, we rely on the reduced-form specification (eq. 1), since firms that ceased operations would not have used DS throughout the entire period, making any causal interpretation mechanically biased. The sample consists of the universe of SMEs with employees in 2019, constructed by merging the INPS-Imp database with Infocamere administrative records, which provide information on firm entry and exit.

Table 4.12 shows that there are no significant differences in the probability that Southern firms have survived in 2023. A possible explanation is that, while the policy effectively increased profitability, access restrictions for firms in financial distress limited its impact on the marginal firms.

Tab. 4.12: Survival

	(1)	(2)
	Survival	
South	0.0030	0.0050
	(0.0044)	(0.0044)
Total obs.	455,097	455,097
Bw	62.4	62.4
Effective obs.	167,574	167,574
	NO	ATECO2D

Note: Sharp RDD estimates. The sample includes all SME firms observed in 2019. The dependent variable is a dummy equal to one if the firm is present in 2023. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). The optimal bandwidth is calculated using Calonico et al (2020) in the specification of Column 1. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

5. Conclusions

In this paper, we examine the Decontribuzione Sud (DS) program, a policy introduced in 2020 featuring a reduction in social security contributions for all firms operating in the Southern regions of Italy. We document a take-up rate below expectations, with just above 60% of eligible workers benefiting from the policy and provide explanations for this relatively low take-up. For large firms (with more than 250 employees), the low take-up was mainly explained by firms being close to or already exceeding the maximum amount of State aid permitted under EU legislation. This constraint likely led to a significant share of these firms not participating in the program. In contrast, for small and medium-sized enterprises (SMEs), although some evidence of displacement by other concurrent incentives could be identified, a large fraction of missing take-up remains unexplained. We suggest that a key factor behind this low take-up is the compliance with social contribution payments, a requirement for accessing the DS benefit. Although this factor is not directly observable in our data, the fact that many SMEs may not meet the compliance requirement – due to administrative or financial difficulties – likely plays a major role in explaining the missing take-up.

Using the discontinuity at the border of the Southern regions as an identification strategy, we further provide evidence on the effects of DS on firms' employment and balance-sheet variables. We find that DS reduces labor costs by an amount consistent with the tax cuts introduced by the policy; however, we detect no impact on employment or wages on average across firms. Regarding firm performance, we find a positive effect on revenues, which emerges only in 2023 and is concentrated among firms with higher labor intensity. For these firms, lower labor costs may substantially enhance competitiveness, allowing them either to reduce prices or – during an inflationary period – to implement more moderate price increases. We also find that the policy had a positive and broader effect on profitability and liquidity, as firms experienced improved financial conditions. By contrast, we detect no impact on investment. This outcome could be a consequence of the severe uncertainty surrounding the DS program, particularly due to its dependence on the European Union TFs for State aid, which required repeated short-term EU approvals. The lack of a stable and long-term regulatory framework likely led firms to use the DS savings to improve short-term financial health rather than making long-term investments.

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Appendix

Figure A.1: Sample regions for RDD analysis

Table A.1: Effect of the *Decontribuzione* on firms' performance over 2019-23 (optimal polynomial choice)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
					Panel A. Polynomial order 1						
DS use	0.051**	-0.023	0.001	-0.042***	-0.061	0.047**	-0.021***	0.018***	0.040***	0.032**	0.000
	(0.023)	(0.021)	(0.008)	(0.009)	(0.046)	(0.018)	(0.005)	(0.004)	(0.013)	(0.013)	(0.005)
Optimal BW	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3
Estimated MSE	0.00054	0.00045	0.00006	0.00007	0.00210	0.00034	0.00002	0.00002	0.00017	0.00016	0.00003
					Panel B. Polynomial order 2						
DS use	0.045*	-0.035	-0.002	-0.044***	-0.075	0.049**	-0.024***	0.019***	0.036**	0.032**	0.011
	(0.026)	(0.024)	(0.009)	(0.010)	(0.049)	(0.022)	(0.005)	(0.007)	(0.016)	(0.016)	(0.009)
Optimal BW	104.6	106.8	125.7	160.8	108.4	108.9	83.3	82.0	95.7	86.5	75.1
Estimated MSE	0.00068	0.00056	0.00008	0.00010	0.00255	0.00050	0.00003	0.00004	0.00024	0.00024	0.00008
					Panel C. Polynomial order 3						
DS use	0.031	-0.032	-0.016	-0.056***	-0.134**	0.042	-0.023***	0.020***	0.006	0.035**	0.009
	(0.030)	(0.024)	(0.012)	(0.014)	(0.065)	(0.027)	(0.007)	(0.007)	(0.022)	(0.017)	(0.008)
Optimal BW	129.2	170.5	116.2	127.2	113.3	123.6	97.1	133.3	103.6	135.2	130.3
Estimated MSE	0.00089	0.00059	0.00014	0.00018	0.00420	0.00073	0.00005	0.00005	0.00048	0.00028	0.00007

Note: Fuzzy RDD estimates. Non-parametric robust bias-corrected estimates (Calonico et al., 2014). Optimal Bandwidth calculated using Calonico et al (2020). MSE estimates based on Pei et al (2022). b*** p<0.01; ** p<0.05; * p<0.10.

Table A.2: Effect of the *Decontribuzione* on firms' performance over 2019-23 (different procedures)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Change in logs over 2019-23						Change in levels over 2019-23				
	Revenues	Employment	Average wage	Total labor cost (per-cap.)	Fixed assets	Per-cap. value added	Labor cost to revenue (ratio)	EBITDA to total assets	Return on equity	Cash flow to debt	Leverage
Panel A. LOCAL LINEAR - CONVENTIONAL ESTIMATES											
DS use	0.065***	-0.008	0.001	-0.041***	-0.025	0.044***	-0.021***	0.017***	0.037***	0.033***	-0.003
	(0.019)	(0.018)	(0.006)	(0.007)	(0.039)	(0.016)	(0.004)	(0.004)	(0.012)	(0.012)	(0.005)
Panel B. LOCAL LINEAR - BIAS-CORRECTED ESTIMATES											
DS use	0.051***	-0.023	0.001	-0.042***	-0.061	0.047***	-0.021***	0.018***	0.040***	0.032***	0.000
	(0.019)	(0.018)	(0.006)	(0.007)	(0.039)	(0.016)	(0.004)	(0.004)	(0.012)	(0.012)	(0.005)
Panel C. GLOBAL LINEAR											
DS use	0.100***	0.022	-0.002	-0.036***	0.037	0.042***	-0.022***	0.014***	0.032***	0.034***	-0.009**
	(0.018)	(0.017)	(0.006)	(0.006)	(0.037)	(0.015)	(0.004)	(0.003)	(0.011)	(0.010)	(0.004)
Total obs.	139,531	131,953	131,953	131,953	132,039	131,953	139,531	139,531	139,531	139,152	139,531
Bandwidth	64.3	59.0	93.8	111.9	61.4	78.9	49.8	91.5	76.2	58.0	88.3
Effective obs.	51,388	44,808	61,101	64,434	46,089	54,514	31,945	64,481	56,906	46,333	63,335

Note: Fuzzy RDD estimates. Optimal Bandwidth calculated using Calonico et al (2020). Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Panel (a): Conventional RD local linear estimates with conventional variance estimator. Panel (b): Bias-corrected RD local linear estimates with conventional variance estimator. Panel (c): Conventional RD global linear estimates with conventional variance estimator.