The Labor Market Incidence of Place-Based Policies*

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March, 2026

Abstract. Do place-based subsidies generate broad labor-market gains or simply redistribute rents within firms? We study one of Europe's largest regional development programs- Italy's *Incentives for Disadvantaged Areas* (IDA), 1996–2007- by linking application-level rankings and subsidy awards to the universe of matched employer-employee administrative records. Exploiting quasi-random assignment of projects near within-ranking funding cutoffs, we find that marginally funded firms expand both employment (by roughly 7-13%) and wages (by 1-2%) over the following years. But these average effects mask sharp within-firm heterogeneity: employment rises through both higher retention and increased hiring, while wage gains accrue almost entirely to incumbent workers who remain with the firm. This pattern is hard to square with standard frictionless models, but fits an environment in which wage-setting institutions cause firm-level shocks to pass through differently to incumbent workers and new hires. Finally, we show that the effects of the policy extend beyond recipient firms. Combining the program's budget-allocation rules with simulation-based exposure measures, we trace spillovers through local labor markets and find that policy-driven local employment growth raises wages at competing non-treated firms.

*The opinions expressed are solely those of the authors and do not represent the views of INPS, the Bank of Italy or the Eurosystem. We thank Matilde Bombardini, Kirill Borusyak, Sydnee Caldwell, Patrick Kline, Mathilde Munoz, Filippo Palomba, Cailin Slattery, as well as participants in the Berkeley Labor lunch, Trade Lunch, and OEW seminar, for helpful comments and discussions. Motta gratefully acknowledges support from OLab, the Institute of Business Innovation, and John L. Simpson ABD Research Fellowship.

1 Introduction

Across countries, place remains a powerful determinant of economic opportunity (Blanchard and Katz, 1992; Chetty et al., 2013). To address these spatial disparities, governments devote substantial resources to place-based policies, among which firm-specific subsidies: grants or tax incentives aimed at inducing firms to locate and expand in targeted areas (Bartik, 2020; Slattery and Zidar, 2020). The rationale is that subsidized investment may stimulate local labor demand, creating benefits not only within recipient firms but also for workers and other firms in the surrounding economy. Whether such gains materialize, however, depends critically on how local labor markets respond to firm-level shocks.

Evaluating these policies therefore raises two central questions: efficiency and incidence. In a benchmark model with competitive labor markets, subsidies merely distort the spatial allocation of activity. But if labor markets depart from perfect competition-for instance, because unemployment is partly involuntary, or because frictions and institutions prevent wages from adjusting through market clearing-then demand-side interventions may be justified on efficiency grounds (Kline and Moretti, 2014b). Even then, the efficiency case need not align with the distributional one: when local demand expands, the resulting gains may accrue to firms, incumbent workers, or other workers through spillovers. Evaluating place-based subsidies therefore requires understanding both whether they act on pre-existing distortions and who ultimately benefits when local investment expands.

We study these questions in one of Europe's largest place-based subsidy programs: Italy's *Incentives for Disadvantaged Areas* (IDA). Implemented through repeated competitions between 1996 and 2007, the program was substantial in scale, allocating about €26 billion in capital grants, mobilizing roughly €87 billion in total investment across a wide range of manufacturing sectors. The program explicitly targeted projects with both *private* and *social* value: firms applied for subsidies to finance new plants or expand existing establishments, while committing to create jobs as part of the subsidized investment.

We combine the universe of subsidy applications with matched employer-employee data and study three dimensions along which place-based subsidies interact with labor markets: their effects on firm-level employment and wages, the within-firm distribution of gains across worker types, and the spillovers to non-participating local competitors. To our knowledge, these dimensions have not been examined jointly in the place-based policy literature, nor in a setting that links matched employer-employee records to the full universe of subsidy applications. Together, they capture distinct channels through which policy gains reach-or fail to reach-workers: transmitted to average firm-level wages; distributed unequally within the firm; or diffused to the surrounding firms through changes in scale. Studying these channels jointly matters because they can point in different directions: a policy that raises average wages at treated firms may simultaneously leave new hires unaffected while benefiting workers at untreated competitors. Only by tracing the full incidence chain-from the firm, through its workers, to the broader market-can we assess whether a place-based subsidy achieves its distributional goals.

Italy is an especially informative setting because it allows us to combine rich, granular data on labor markets and wage-setting institutions with a context that features two characteristics central to many European place-based policies: large regional disparities together with relatively limited migration, so

local slack does not quickly dissipate through worker reallocation; and strong wage-setting institutions that, through collective bargaining agreements, shape how local labor demand shocks pass through to workers.[1] This combination is crucial for incidence. When wages at entry are constrained by collective agreements, a subsidy-induced expansion may affect incumbent and newly hired workers very differently. The program thus offers a rare opportunity to study not only whether place-based subsidies create jobs, but also how labor market institutions shape who captures the gains when they do.

We begin by studying firms that were already active before applying to the program. This lets us trace the response of incumbent firms and track how their employment and wage growth evolve as a result of subsidy receipt. Exploiting quasi-random assignment of grants around eligibility thresholds within funding competitions, we compare marginal winners and marginal losers in a regression discontinuity design. Our first finding is that subsidized firms grow substantially: employment rises by about 8% within three years, while average wages increase by roughly 1%. The effects are stronger for projects involving the construction of new plants than for the expansion of an already existing plant.[2] Wage gains are also heterogeneous across skill groups and are more pronounced for white-collar workers. Combining the reduced-form estimates in a 2SLS regression discontinuity design, we recover a firm-level inverse labor supply elasticity consistent with meaningful departures from perfectly competitive labor markets (Card, 2022).

We then move to our second set of findings regarding worker-level incidence and ask: who within the firm benefits from the expansion induced by the subsidy? We begin by unpacking the employment response itself. An increase in employment can reflect two very different margins- firms may hire more new workers, or they may retain more of the workers they already employ- and these margins have very different implications for how the gains from the policy are distributed. Using matched employer-employee data, we estimate event-study specifications that compare firms applying in the same local labor market but falling on opposite sides of the funding cutoff. We find that both margins are important: retention rises sharply in the first two years after treatment, suggesting that subsidized firms initially use the transfer to stabilize and hold on to their workforce. Hiring, in contrast, builds more gradually over time and accounts for most of the employment response by year four. This pattern is visible for both blue-collar and white-collar workers.

We then examine the wage response, where the incidence of the policy is much more uneven within the firm. We begin with incumbent workers employed at applicant firms in the year of application. For this cohort, we estimate a worker-level event-study design that exploits the longitudinal structure of the data and includes worker fixed effects, allowing us to net out time-invariant worker heterogeneity and trace how the outcomes of the same workers evolve after subsidy receipt. Relative to workers at losing firms in the same local labor market, workers at winning firms display parallel trends prior to application but experience wage gains afterward. These gains, however, are concentrated entirely among workers who remain at the subsidized firm, and are especially pronounced for white-collar stayers. By contrast, we find no wage effect for workers who leave the firm. We also investigate whether this pattern reflects selective

[1] Despite a long-run decline in union membership across the OECD, collective bargaining coverage remains high in much of Europe-exceeding 80% in France, Italy, Belgium, and the Nordic economies (Jäger et al., 2024).

[2] As we explain in detail in Section 3, we identify an establishment as the combination of a firm ID and a labor market (defined as a geographical unit,i.e., province, times a 2-digit industrial sector).

retention and find no evidence that separating workers differ in predicted quality across winning and losing firms.

Turning to the effects of the subsidy on wages for newly hired workers, the empirical challenge is different: composition is a first-order concern, since subsidized firms may recruit a different mix of workers after treatment, and worker fixed effects are no longer available to absorb persistent heterogeneity. To address this, we construct a firm-year measure of residualized starting wages and study its evolution in a difference-in-differences design.[3] This yields a composition-adjusted measure of the wage paid at entry and allows us to test whether winning firms respond to the subsidy by raising the wages offered to new recruits, as they do for incumbent workers. We find that they do not. The effect of subsidy receipt on entrants' starting wages is essentially zero, with little heterogeneity by skill group. As a result, the subsidy increases within-firm wage inequality.

We explain this unequal incidence by studying the role of wage-setting institutions. After ruling out alternative explanations based on changing worker composition, these findings are difficult to reconcile with a standard, frictionless, competitive model of the labor market. They are instead more consistent with an environment in which wage-setting institutions make adjustments asymmetrically across workers, so that firms respond to a positive shock by raising pay for incumbent workers to increase retention, while relying on entry wages that remain comparatively rigid for new hires. We then bring in new data on collective bargaining agreements and show that this asymmetry maps closely into the institutional structure of wage setting: starting wages of new hires are tightly anchored to contractual minima, while incumbent pay is adjusted more flexibly through bargaining within the firm. This is the main margin through which subsidized firms share rents and retain workers. The broader implication is that job creation and wage incidence need not coincide: while the policy creates jobs, wage gains accrue primarily to incumbents already attached to the firm.

Finally, we study the market-level effects of the policy, and in particular its spillovers onto other firms' wages and employment. To do so, we develop a novel instrument for market scale[4] in the spirit of Borusyak and Hull (2023). The key source of variation comes from random fluctuations in the total number of jobs promised by firms that happen to receive funding around the eligibility threshold. Even within the same funding pool, local labor markets differ in how many near-cutoff firms they contain and, crucially, in the total number of promised jobs attached to those marginal winners. We exploit this quasi-random variation while recentering the instrument to account for each market's expected exposure, given the number and characteristics of the applicant firms it contains. Identification therefore comes from whether a market receives more or less subsidy-induced employment than would ordinarily be expected, rather than from systematic differences in applicant composition across markets.

Using this variation, we estimate how policy-driven local employment growth affects non-treated firms in the same market. We find that a policy-induced 10% increase in local employment raises wages at competing firms by 0.84 to 1.23 percentage points. These spillovers imply that the effects of the program extend well beyond recipient firms. By increasing local employment density, the policy improved workers'

[3] Specifically, we first purge individual entry wages of variation explained by observable worker characteristics and then average the residualized wages across new hires within each firm-year.

[4] We define market scale as total employment in a market (following Glaeser et al. (1992) and Bartelme et al. (2025)).

outside options and generated upward wage pressure even among firms that did not themselves receive support.

Contribution to the literature. The paper contributes to several strands of literature.

First, the paper contributes to the empirical literature on place-based policies and their effects on firm investment, employment, and local economic activity; for a comprehensive survey, see Neumark and Simpson (2015). Within this broad literature, our setting is closest to work on firm-specific subsidies in a variety of institutional contexts (Devereux et al., 2007; Criscuolo et al., 2019; Slattery and Zidar, 2020; Siegloch et al., 2024).[5] Much of the existing evidence, however, relies either on firm-level averages or on area-level aggregates and therefore remains largely silent on how the gains from these policies are distributed within local labor markets. An exception is the contemporaneous work of Grunau et al. (2024), who study worker-level effects of a similar policy in Germany. Relative to this literature, our paper makes three main advances. First, because we observe the full set of applicants together with the institutional rules governing eligibility, we can implement a particularly sharp design that compares treated and untreated firms within the same local labor market, rather than relying on across-market variation in exposure or eligibility. Second, we move beyond average firm-level responses and study the specific margins through which subsidized firms grow, emphasizing the role of both hiring and retention, and the importance of wage-setting to explain these effects. Third, we show that the effects of the policy extend beyond treated firms by constructing a market-level instrument that identifies spillovers onto other firms in the same local labor market.[6] More broadly, the paper also speaks directly to the economic rationale for place-based policy by providing empirical evidence on the labor-market distortions through which such policies may operate. Place-based subsidies can raise welfare only insofar as they interact with pre-existing distortions (Kline and Moretti, 2014b), yet there is remarkably little direct evidence on what those distortions are or how they shape policy incidence. While subsequent work has substantially advanced the theory (Bilal, 2023; Lindenlaub et al., 2023; Oh, 2024), its empirical content remains limited. Our setting allows us to make progress on this question. Combining quasi-experimental variation in subsidy receipt with novel data on wage-setting institutions, we show that the incidence of the policy is highly asymmetric across workers: rent-sharing with an increase in wages and retention for incumbents, employment expansion for entrants. The paper therefore gives empirical content to the welfare calculus of place-based capital subsidies by identifying the institutional frictions through which the gains from the policy are transmitted within the labor market.

Second, the paper contributes to the literature on wage-setting in imperfect labor markets (Card et al., 2018; Jäger et al., 2024). A large body of evidence shows that workers' wages often move with firm success -for example following innovations or patent awards (Van Reenen, 1996; Kline et al., 2019), increases in profits (Bertrand and Mullainathan, 1999), grant-induced cash windfalls (Howell and Brown, 2023), or trade shocks (Caldwell and Harmon, 2019; Garin and Silvério, 2024). At the same time, contractual wage floors may constrain how firm-level shocks pass through to workers, especially in rigid labor markets (Card et al., 2014). We contribute to this literature by showing that the transmission of firm-level shocks to

[5] Bronzini and De Blasio (2006); Cerqua and Pellegrini (2014); Cingano et al. (2023) also study IDA, focusing primarily on firm-level investment and employment effects.

[6] In this respect, the paper is closer in spirit to Greenstone et al. (2010), but exploits quasi-experimental variation in policy exposure across markets rather than comparing locations before and after large plant openings.

workers depends on the structure of wage-setting institutions: the same firm-level shock can generate very different wage responses across workers within the same firm, depending on the institutional margins along which wages can adjust.[7] In our setting, nationally negotiated contractual minima anchor starting wages for new hires, generating upward rigidity at entry[8] while firms use discretion over wage premia above those floors for incumbent workers. Subsidized firms appear to use this flexible component of pay to retain incumbents, rather than to raise entry wages for new hires. This has direct implications for within-firm inequality and for the incidence of place-based policy.[9]

Finally, the paper contributes to the literature on how large and plausibly exogenous local labor-demand shocks propagate across firms and workers (Blanchard and Katz, 1992; Moretti, 2010). This literature exploits a wide range of shocks, including natural-resource booms (Feyrer et al., 2017), international trade shocks (Autor et al., 2013), and various forms of government spending.[10] We contribute to this literature by studying a different type of local demand shock: a place-based subsidy targeted at firms in distressed areas, where we can observe the number of jobs firms commit to create as part of the subsidized investment, rather than only the amount of money channeled to a place. Taken together, our results show that local investment subsidies affect not only the directly treated firms, but also the broader organization of production and employment within the local economy.

The paper proceeds as follows. Section 2 describes the context: the IDA program, wage-setting institutions, the main data used in the analysis, as well as some descriptive facts. We then present firm-level RDD estimates (Section 3), worker-level incidence (Section 4), and market-level spillover results (Section 5). We conclude with our next steps.

2 Institutional Framework and Data

This section sets up the empirical analysis. We begin with the IDA program-its design, eligibility rules, and ranking mechanism-because understanding the program's structure is necessary before interpreting our identification strategy. We then describe the wage-setting institutions that govern how subsidies translate into earnings, and finally present the administrative data sources we combine.

[7] This sharp asymmetry also highlights an important limitation of firm-level average wages as a measure of rent sharing: average effects can mask strongly unequal incidence across worker cohorts.

[8] Although firms might be slow to update posted wages, one would expect adjustments by year four. We find no such evidence. Our results suggest that the nominal rigidity in new-hire wages identified by Hazell and Taska (2025) is not just downward; there is evidence of upward rigidity as well.

[9] In this respect, we also relate to recent work that ties rising wage inequality to growing disparities across firms (Card et al., 2013, 2018; **?**).

[10] Existing work studies increases or contractions in federal procurement (Wilson, 2012), spending programs whose allocation depends mechanically on local population thresholds (Suárez Serrato and Wingender, 2016), financial shocks to state pension funds (Shoag, 2013), temporary stimulus payments (Pennings, 2021), and military spending (Nakamura and Steinsson, 2014).

2.1 The Italian Place-Based Policy: Incentives for Disadvantaged Areas

Historical context. Italy's lagging South[11] has been at the center of regional policy debates for more than a century. Following the Second World War, the Italian government consistently directed resources toward the southern regions through infrastructure investment and industrial support (Cerrato and Filippucci, 2024; Incoronato and Lattanzio, 2024). Despite these sustained efforts, the territorial divide has persisted.[12] With the introduction of the European Union's unified Cohesion Policy framework in 1990s, support for lagging areas became more formally institutionalized. Under the revised state-aid regime, disadvantaged regions became eligible for substantial EU transfers, and Italy's southernmost regions qualified throughout our sample period. IDA grew out of this longstanding tradition of place-based industrial policy, but in a new form: it replaced a largely discretionary allocation system with a competitive grant mechanism.

Program design. IDA was an investment subsidy program active between 1996 and 2006, with a total budget of €26 billion, triggering €87 billion in total private investment. The law disbursed capital grants to firms pursuing investment projects tied to job creation in European Commission-designated eligible areas, primarily Objective 1 regions (GDP per capita below 75% of the EU average) and Objective 2 areas (regions with declining industries and above-average unemployment).[13] Together, these covered 48% of the national population, concentrated overwhelmingly in the South, with manufacturing as the dominant subsidized sector.

The program operated through yearly *calls for tenders*: each of the eligible Italian regions opened its own call, inviting firms to submit investment project bids in *eligible* areas. [14] In a bid, the firm had to specify three components. First, the *type of project*: for instance, whether the firm proposed to construct a new plant or expand its current facilities at an existing one.[15] Second, the *project location*, which determined the maximum applicable subsidy rate.[16] Third, the *investment plan*: total investment size, own-capital share, promised jobs, and the requested grant amount.

Ranking and identification. Projects were evaluated on five criteria: (1) the ratio of own capital to total investment, $i_1 =$ Own K/Total Inv., which rewarded firms' own financial commitment; (2) the ratio of promised jobs to total investment, $i_2 =$ Jobs/Total Inv., which rewarded employment intensity; (3) the ratio of the maximum allowable grant to the requested grant, $i_3 =$ Max Grant/Requested Grant, which rewarded fiscal restraint; (4) regional priorities related to project location, type, and sector, set ex ante

[11] The South (Mezzogiorno) includes 8 regions: Abruzzo, Molise, Campania, Apulia, Basilicata, Calabria, Sicily, and Sardinia. The Center-North includes: Valle d'Aosta, Piedmont, Lombardy, Liguria, Veneto, Trentino-Alto Adige, Friuli-Venezia Giulia, Emilia-Romagna, Tuscany, Umbria, Marche, and Lazio. A region is comparable, relatively, to a US state: in 1995, Italy had a total population of approximately 56.84 million people, and the average population of a region was approximately 2.84 million inhabitants.

[12] In the early 1990s, Southern GDP per capita stood at roughly 60% of the Northern level, unemployment rates exceeded 20% in Campania, Sicily, and Calabria, and labor force participation was among the lowest in the OECD.

[13] For a detailed discussion of the EU Cohesion Policy framework and comparisons with analogous US programs, see Appendix A.2.

[14] For our analysis, we only focus on Southern regions, and firms submitting bids there, as they were allocated the vast majority of the budget. While also some territories in the Center-North were eligible for the subsidy, they were very few and were devoted a small share of the total policy budget.

[15] See Appendix A.3 for a detailed classification of the type of projects that could be financed.

[16] The subsidy rate would range from up to 65% of investment for small firms in Calabria to 7.5% for large firms in the North, with distinct schedules applying to the 1995-1999 and 2000-2005 periods. Subsidy rates by region and period are reported in Appendix A.4.

by regional councils but not disclosed to applicants; and (5) an environmental impact assessment. Each project received a composite score equal to the sum of these components after standardization within competition. Formally, firm f applies in call c (a region-year pair) and receives

$$\text{ProjectScore}_{fc(f)} = \sum_{d=1}^{D} \frac{i_{d,fc(f)} - \mu_{d,c(f)}}{\sigma_{d,c(f)}},$$

where criterion d is standardized within call c using mean $\mu_{d,c(f)}$ and standard deviation $\sigma_{d,c(f)}$.

Within each call, projects were not ranked in a single list. Instead, they were partitioned into distinct *funding pools* defined by characteristics: firm size (small versus medium or large), manufacturing dummy (whether manufacturing sector or servcies), EU co-financing eligibility, as well as broad EU objective area. Denoting a funding pool by p, all projects in p competed for a group-specific budget B_p and were ranked from highest to lowest score.[17] Funding was then assigned sequentially until that budget was exhausted, generating a cutoff score $\bar{S}_p$. Project i in group p received a grant if and only if $S_{ip} \geq \bar{S}_p$.

This institutional structure is central for identification. Our regression discontinuity design compares firms located just above and just below the funding cutoff within the same *funding pool*. These pools, however, were defined by administrative eligibility criteria, not by the boundaries of local labor markets. We follow the literature and we define a local labor market as a province (roughly analogous to a U.S. county) crossed with a 2-digit NACE industry (approximately corresponding to a 3-digit NAICS industry in the North American classification).[18] As a result, firms may compete for funding within the same administrative pool while operating in distinct local labor markets. This feature allows quasi-random variation in subsidy receipt within pools, while also allowing us to include market fixed effects in our analysis.

To fix ideas, Table 1 presents two applications from the second 1996 competition, both submitted by firms bidding for projects in Sicily. The two applications belong to the same funding pool-group 1-covering small manufacturing firms in Objective 1 areas with EU co-financing. Yet, even within this narrowly defined pool, firms belong to two different *markets*: they are in different manufacturing sectors (automobiles and wood products) and different provinces (Palermo and Catania). Project 1000, a marginal winner with a standardized score of $+0.018$, had a high own-capital share (0.925) but promised no additional jobs. Project 22245, a marginal loser with a score of -0.003, had a lower own-capital share (0.728).

2.2 Wage Setting

Two-tier bargaining. Understanding who captures the gains from a firm subsidy requires understanding how wages are set. In Italy, wages are governed by a two-tier collective bargaining agreements (*CBA*) system. At the first tier, industry federations and the main union confederations negotiate sector-wide agreements, *contratti collettivi nazionali di lavoro* (CCNL), that establish legally binding minimum pay rates

[17] Three additional allocation rules further shaped the distribution of funds: at least 50% of each regional budget was reserved for small and medium enterprises, at most 5% could be allocated to service-sector projects, and EU-co-financed projects were prioritized.

[18] For a discussion on labor market definition, please refer to C.1.

Table 1

Competition n. 2: Sicily x 1996								
Project	**Pool**	**Province**	**Sector**	**Specialty**	$i_1 = \frac{OwnK}{Inv}$	$i_2 = \frac{Jobs}{Inv}$	$i_3 = \frac{100}{Subsidy}$	**ProjectScore**
1000	1	Palermo	Manuf.	Auto	.925	0	1.33	2.26
22245	1	Catania	Manuf.	Wood	.728	.006	1.50	2.234

$\underline{w}_{\ell s}$ for each occupational level ℓ within sector s, renewed every two to three years.[19] Crucially, these floors apply *uniformly* to all covered firms regardless of size, local productivity, or geographic location. As a direct consequence, low-productivity Southern provinces might face much higher non-employment rates, as employers cannot reduce wages below the nationally negotiated floor to clear the market (Boeri et al., 2021).

At the second tier, companies may negotiate additional supplements above the national floor, reflecting firm-specific productivity, profitability, or retention pressures. In principle, a firm-wide additional agreements is permitted, but it can *only* increase wages above the floor, not reduce them.[20] In practice, this firm-wide second tier was limited in reach.[21] However, there is nothing preventing firms from paying workers more than the established first tier minima, bargaining individually with workers. This implies that for worker i at occupational level ℓ employed at firm f in sector s, earnings thus decompose as:

$$\text{Wage}_{i\ell} = \underbrace{\underline{w}_{\ell s(i)}}_{\substack{\text{CBA floor} \\ \text{(national, binding)}}} + \underbrace{\text{top-up}_{if(i)}}_{\substack{\text{firm-worker specific} \\ \text{(discretionary)}}} \tag{1}$$

Consequences for place-based policies. The geographic uniformity of the CBA floors and the architecture of wage setting has a sharp implication for the incidence of a subsidy. When a subsidy increases a firm's marginal revenue product of labor, employment might rise, but if the CBA floor is set too high, it might prevent entry wages from adjusting.[22] New hires are recruited at $\underline{w}_{\ell s(i)}$, the going national minimum for their occupational grade, as the firm faces a competitive external labor market at that rate and has no incentive to offer more *ex ante*: the bargaining surplus of any individual new hire is zero. Incumbents are different. Workers who have already accumulated firm-specific human capital are costly to replace; when the capital shock raises the value of each existing match, the firm responds by raising the discretionary top-up to secure retention. The prediction is therefore sharp: wage gains should be concentrated entirely

[19] In 2021, Italy had around 346 distinct national agreements covering 97.7% of dependent employment and 99.3% of firms (Boeri et al., 2021), making the system among the most encompassing in Europe.

[20] The *favorability* principle prevents decentralized agreements from undercutting national minima.

[21] Survey evidence from the mid-1990s indicates that only about 10% of firms engaged in any firm-level bargaining, overwhelmingly large manufacturing establishments in Northern industrial districts (Boeri et al., 2021).

[22] Southern Italian firms pay the same wage floor as Northern firms in the same industry, despite operating in labor markets with substantially lower productivity and far higher unemployment. For the typical IDA applicant-a small manufacturing firm in a high-unemployment Southern province-the national floor might be *binding*: absent the contract, the market-clearing wage would lie below it. The IDA subsidy therefore arrives in an institutional environment where any upward wage pressure on new hires might be blocked, making the stayer/new-hire distinction the central distributional divide in the incidence of place-based capital grants.

among stayers, with zero pass-through to new hires-a pattern invisible in firm-level average wages but clearly visible in the worker-level decomposition we present in Section 4.

2.3 Data

The first is the universe of Law 488 subsidy applications, covering all 21 calls for tenders issued across Italian regions between 1996 and 2006. This dataset records 62,631 project bids submitted by 47,200 firms, of which 21,271 projects-linked to 18,545 firms-crossed the funding threshold and received grants. For each application, we observe the project score, the call-specific cutoff, the requested and awarded grant amounts, the project type (new plant versus expansion), the target location, and the firm identifier used to match the applications to employment records.

We link these data to the Italian firm registry, a longitudinal dataset covering the universe of non-agricultural firms with at least one employee in Italy since 1983. This source provides establishment-level information on employment by occupation, average earnings, 6-digit industry codes, and province identifiers. We use it primarily to study the effects of subsidy receipt on firm-level outcomes such as employment and wage growth, and to construct local labor-market aggregates that allow us to analyze spillovers onto other firms.

Finally, we link the applications to the INPS matched employer-employee administrative data[23] For each worker-year observation, these data record gross earnings, weeks worked in full-time-equivalent units, part-time status, contract type (permanent versus temporary), and occupational classification, including apprentices, blue-collar workers, high-skilled blue-collar workers, white-collar workers, middle managers, and managers. The data also include worker demographics such as gender, year of birth, province of birth, year of labor-market entry, and year of retirement.

For descritpive statistics on the policy, as well as for additional planned market-level analysis and robustness, we further draw on two auxiliary sources. Decennial census data from 1981 onward supply local population counts, employment levels, housing stock, and housing conditions that we use to characterize baseline conditions across local labor markets. Province-level migration and value-added data-migration from population registries following Bonifazi and Heins (2000), and value-added from *Istituto Tagliacarne*-allow us to assess local economic conditions.

2.4 Subsidy Allocation Patterns

We highlight three facts about the size and allocation of IDA subsidies that motivate the analysis below. A fuller description of which projects were subsidized, what types of firms applied, how subsidies varied by region, firm size, and project type, and which local labor markets attracted the most new-plant applications is provided in Appendix B.

First, IDA was explicitly geared toward job creation. Although the program could also finance projects with

[23]We accessed this dataset through the VisitINPS program. Due to privacy constraints, the linkage could only be performed using establishment identifiers, year of application, and eligibility status. We could not recover the exact application score, and for this reason the worker-level analysis relies on a difference-in-differences design rather than a regression discontinuity design.

no employment commitment, such as restructuring existing establishments, 98% of subsidized projects involved either the construction of a new plant or the expansion of existing production capacity, together with a pledge to create jobs.

Second, the program was entrepreneurial in design: new-plant projects accounted for more than half of total subsidy spending, with an average grant of about €617,000. Yet these were rarely cases of outside firms entering a new market. About two-thirds were undertaken by firms already operating in the same local labor market, about one-third by newly created firms, and only 2.5% by established firms entering from elsewhere. This matters for Section 3, where we study incumbent firms already active in a market and track how subsidies affect their employment and wage-setting.

Third, the geography of subsidies closely followed local economic disadvantage: Southern regions with higher pre-policy unemployment received both more plants and larger grants per plant, with funding heavily concentrated in manufacturing. Because subsidy exposure is therefore not randomly distributed across markets, the market-level analysis relies on a recentered design, following Borusyak and Hull (2023), to isolate the quasi-random component of local policy exposure.

3 Effects on applicant firms

We start by studying the effects of subsidy applications for *incumbent* firms that chose either to open a new plant in a labor market where they already operated or to expand an existing establishment. In the full firm archive, however, we cannot distinguish between these two types of projects, since the data only identify the firm within a given market and not the specific establishment affected by the application.

3.1 Identification and Empirical Design

Near each pool's funding cutoff, grant assignment is as good as random. Firms just above and just below the threshold submitted nearly identical applications, differing only in which side of the score cutoff they fell on. Because grants are typically disbursed within two years of application, any subsequent divergence in employment or wages between marginal winners and marginal losers can be attributed to subsidy receipt rather than to pre-existing differences. Figure 1 confirms the key validity condition: the density of the running variable is smooth through zero, with no detectable bunching just above the cutoff, indicating that firms cannot precisely manipulate their composite scores to secure a grant.

Our main specification is a regression discontinuity estimated separately at each post-application horizon t:

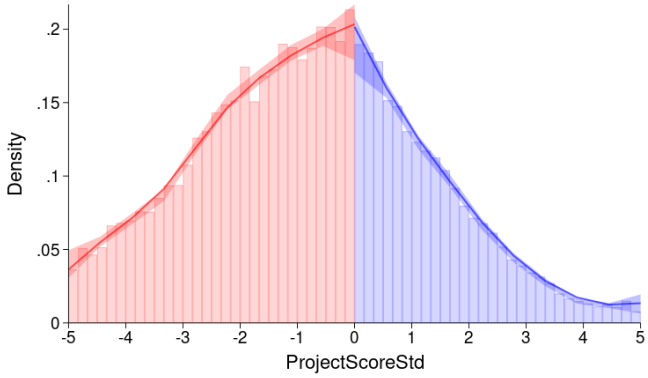
$$\Delta \log Y_{fp}^{(t)} = \beta_0 + \beta_1 \text{Win}_{fp} + \beta_2 \text{ProjectScoreStd}_{fp} + \beta_3 \text{Win}_{fp} \times \text{ProjectScoreStd}_{fp} + \alpha_p + \alpha_{m(f)} + \varepsilon_{fp}^{(t)} \quad (2)$$

where $\Delta \log Y_{fp}^{(t)}$ is the change in log employment or log average wages at firm f in pool p, measured t years after application; $\text{ProjectScoreStd}_{fp} = \text{ProjectScore}_{fp} - \text{Cutoff}_p$ is the running variable centered at the pool-specific funding cutoff; and $\text{Win}_{fp} = \mathbf{1}[\text{ProjectScoreStd}_{fp} > 0]$ is the treatment indicator. Pool

fixed effects α_p absorb all competition-level heterogeneity in baseline conditions and budget availability. The market fixed effects $\alpha_{m(f)}$, defined at the industry-province level, are the key innovation relative to prior work: they ensure that identification comes from comparing winners and losers operating within the *same* local labor market, purging any confounding from local demand shocks or area-specific trends.

Eligibility for the subsidy is determined within a set of project rankings established separately for each call c, which correspond to distinct combinations of region and year. Importantly, multiple parallel rankings exist within each call, based on four pre-determined dimensions: firm size (SMEs vs. large firms), whether the project receives EU co-financing, EU Objective Area classification (three levels), and sector (services vs. non-services). This institutional feature generates multiple discontinuities in eligibility across otherwise comparable firms. Our identification strategy exploits these within-call discontinuities, comparing firms just above and below the relevant cutoff within each ranking group. Under the assumption that firms cannot precisely manipulate their project scores, this setting provides a credible source of quasi-experimental variation in subsidy receipt.

Fig. 1: No bunching at the funding threshold.

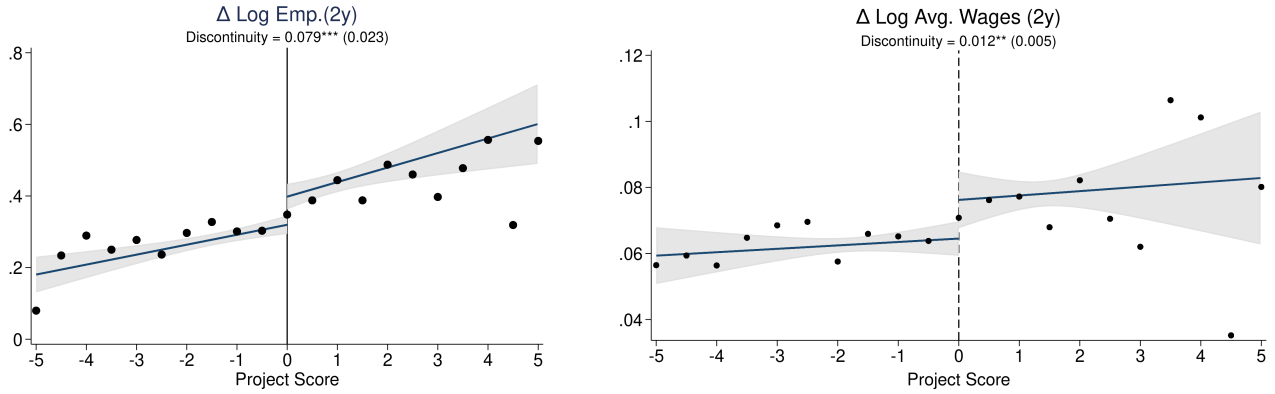


Notes: McCrary (2008) density test for the standardized project score running variable. The smooth density through zero-with no detectable jump-indicates that firms cannot precisely manipulate their scores to just cross the cutoff, supporting the key identifying assumption of the regression discontinuity design.

3.2 Results

We focus on outcomes two years after application $(t+2)$, when the effects are already clearly visible; estimates for $t+1$ and $t+3$ are similar and reported in Appendix D. Figure 2 presents the RD evidence at this horizon. Firms just above the funding cutoff experience a sharp increase in employment, on the order of 7-10%, relative to firms just below the threshold. Average wages also rise discontinuously at the cutoff. Figure 3 reports the RD coefficient for different time horizons following application and also reports placebo estimates for employment and wage growth in the three years prior to the call for projects. Both employment and wages rise gradually as subsidized investment is implemented, with no evidence of differential pre-trends. The employment response is larger and accumulates more quickly, while the wage response is smaller in magnitude but remains persistent over time.

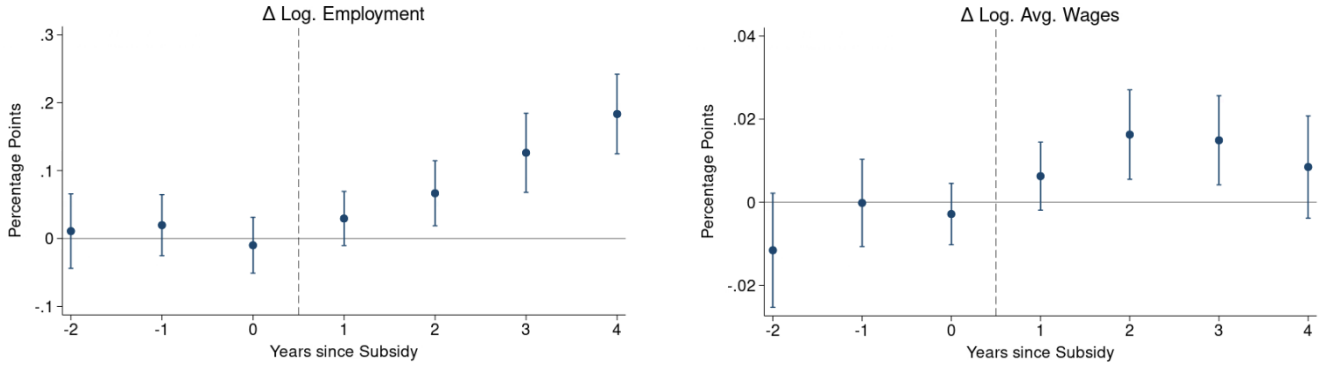
Fig. 2: Subsidized firms grow and pay more.



(i) Log employment change **(ii) Log wage change**

Notes: RD plots of the change in log employment (left) and log average wages (right) two years after the grant application (t+2), plotted against the standardized project score. Each dot is a local average of outcomes within a score bin; the vertical dashed line marks the funding cutoff at zero. Both panels show a sharp upward jump at the threshold. Effects grow further at t+3; see Appendix D.

Fig. 3: RD firm-level estimates over time.



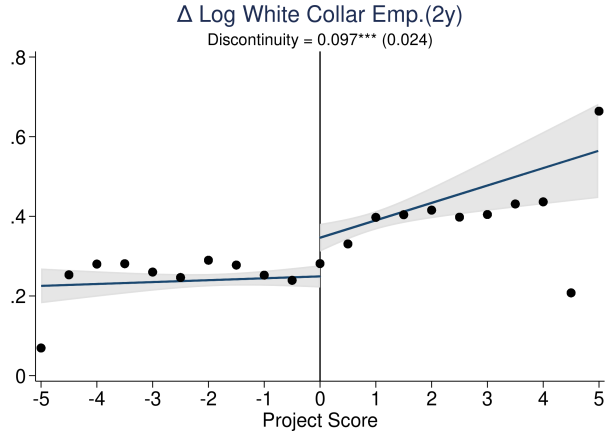
(i) Employment **(ii) Wages**

Notes: Dynamic RD estimates of the treatment effect on log employment (left) and log average wages (right) at horizons t+1 through t+4, with 95% confidence intervals. Each coefficient is estimated from a separate cross-section. The absence of pre-trends (not shown) and the smooth post-application growth are consistent with the program's design: grants fund multi-year investment projects, so employment and wages ramp up gradually as plants are built and equipped.

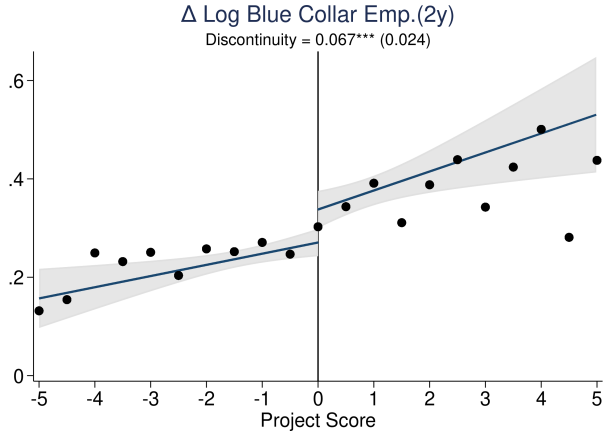
Heterogeneity by skill group. Figure 4 decomposes both employment and wages by skill group (white-collar and blue-collar) at t+2. Employment rises for both groups, with the effect somewhat larger for white-collar workers. Wage gains are concentrated among white-collar stayers; the blue-collar wage response is positive but smaller. We investigate how wage-setting institutions are responsible for this difference in Section 4.

Heterogeneity by project type. In Section 2.4, we described how the IDA program financed two main types of investment: the creation of new plants and the expansion of existing establishments. Although most new-plant subsidies still went to firms already operating in the same local labor market, these projects may nevertheless generate different effects from standard expansions. Opening a new plant typically requires creating a new set of employer-worker matches from scratch and may therefore involve stronger

Fig. 4: Wage gains are concentrated among white-collar workers; employment rises for both skill groups.



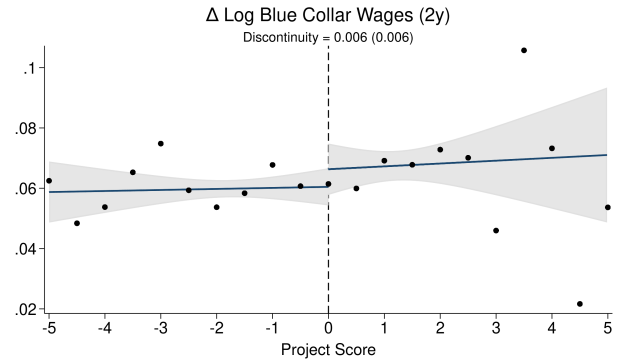
(i) White-collar employment



(ii) Blue-collar employment



(iii) White-collar wages



(iv) Blue-collar wages

Notes: RD plots at $t+2$ by skill group. Left column: change in log employment for white-collar (top) and blue-collar (bottom) workers. Right column: corresponding log wage changes. Both skill groups expand employment at winning firms, but wage gains are substantially larger for white-collar workers.

hiring needs and greater upward pressure on wages.

Appendix Figure D.18 shows that both the employment and wage effects are driven almost entirely by new-plant projects, while expansion projects generate little detectable response at the cutoff. This suggests that the main mechanism behind the policy was the creation of new production units and new employer-worker matches, rather than the simple scaling-up of existing establishments.

Retrieving firm-level (inverse) labor supply elasticity. The fact that subsidized firms increase both employment and wages implies that firm-level labor supply is not perfectly elastic. Under perfect competition, a firm-level labor demand shock would raise employment with no effect on wages. We therefore estimate the firm-level inverse labor supply elasticity from

$$\Delta \log W_f^{(t)} = \frac{1}{\eta} \Delta \log \text{Emp}_f^{(t)} + \varepsilon_f^{(t)} \tag{3}$$

Because employment growth is endogenous to firm-level demand shocks, we instrument it with the

funding discontinuity in the RD design. Near the cutoff, subsidy receipt shifts labor demand quasi-exogenously, so the resulting 2SLS estimate traces out the slope of the firm's labor supply curve.

Table 2 reports positive and statistically significant estimates at all horizons, ranging from 0.147 at $t{+}1$ to 0.070 at $t{+}4$. These imply a labor supply elasticity of approximately 6, rejecting the benchmark of perfect competition and aligning with recent evidence on firms having some monopsony power over workers (Sokolova and Sorensen, 2021). In Appendix, we show that the estimates are broadly stable across horizons (Figure D.16), and they are qualitatively similar for blue and white collar workers (Tables D.14-D.15).

Table 2: Firms face upward-sloping labor supply curves.

	$\Delta \log$ Wage $(t{+}1)$	$\Delta \log$ Wage $(t{+}2)$	$\Delta \log$ Wage $(t{+}3)$	$\Delta \log$ Wage $(t{+}4)$
$\Delta \log$ Employment	0.1472**	0.1685***	0.1109***	0.0701**
	(0.0727)	(0.0588)	(0.0427)	(0.0299)
Observations	21,280	20,967	20,470	19,934
First-stage F-stat	3.71	6.62	12.99	19.95
Overid p-val	0.968	0.667	0.646	0.061

Notes: 2SLS estimates of the inverse firm-level labor supply elasticity $1/\hat{\eta}_f$ from equation (3), computed as the Wald ratio of the wage reduced form to the employment first stage. Each column corresponds to a post-application horizon ($t{+}1$ through $t{+}4$). The inverse elasticity is positive and statistically significant at all horizons, implying $\hat{\eta}_f \approx 6$ and rejecting perfect competition. First-stage Kleibergen-Paap F-statistics and overidentification p-values are reported below the main estimates. Standard errors clustered at the funding-pool level.

4 Effects on workers

Firm-level average wages conflate compositional changes with changes in pay for given workers, and cannot reveal *who* captures the wage gains from a capital subsidy. To answer this question, we link the INPS employer-employee data to IDA applicant firms.

We construct a person-year panel in which each worker is assigned a unique *dominant employer* in each year, denoted $F_{it} \in \mathcal{F} \cup \{\emptyset\}$. For each worker-year, the dominant employer is defined as the establishment associated with the worker's longest employment spell during that year. Here $F_{it} = f$ means that worker i's dominant job in year t is at firm f. This yields a person-year panel of 2,770,905 workers who transitioned through an applicant firm. We anchor each worker cohort at the application year t_f^a and track three groups: *stayers* (employed at the firm at t_f^a and remaining), *leavers* (employed at t_f^a but departing afterward), and *new hires* (joining after t_f^a).

Throughout this section we use a *difference-in-differences event-study* design. We pool workers (or firms) at near-cutoff applicants-those just above and just below the funding threshold-and compare outcomes between treated (above-cutoff, subsidized) and control (below-cutoff, non-subsidized) units over event time $k = t - t_f^a$, using $k = -1$ as the reference period. The design is validated by comparing its estimates to the firm-level RDD results from Section 3; the two approaches yield closely aligned estimates. Where the outcome is a firm-level margin (employment decomposition, workforce composition, average entry wages), we use a firm-level specification. Where the outcome is an individual wage, we use a worker-level specification with worker and year fixed effects. Each subsection below describes the specific regression

used.

4.1 Employment decomposition: how do firms grow?

A policy-induced increase in firm employment can in principle be driven by higher new hiring, higher retention of existing workers, or both. We decompose the employment margin using the accounting identity:

$$\frac{\Delta \text{Emp}_{f,t}}{\text{Emp}_f^0} = \frac{\text{Hire}_{f,t}}{\text{Emp}_f^0} + \frac{\text{Retentions}_{f,t}}{\text{Emp}_f^0} - 1, \tag{4}$$

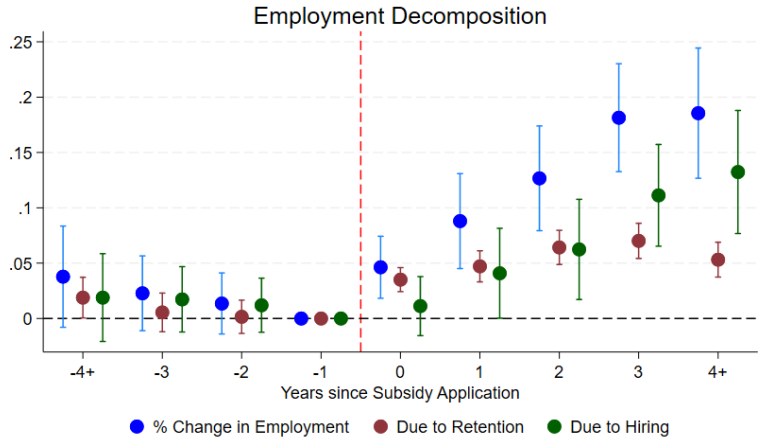
where $\text{Hire}_{f,t}$ denotes the number of new hires in period t and $\text{Retentions}_{f,t}$ the number of incumbents who remain. We estimate each margin using the *firm-level event study*:

$$Y_{ft} = \alpha_f + \lambda_t + \sum_{k \neq -1} \beta_k \, \mathbf{1}\{t - t_f^a = k\} \cdot \text{Treated}_f + \delta \mathbf{X}_{m(f)t} + \varepsilon_{ft}, \tag{5}$$

where α_f are firm fixed effects, λ_t are year fixed effects, and $\mathbf{X}_{m(f)t}$ are market-level controls. Figure 5 plots the event-study coefficients $\hat{\beta}_k$ for each margin separately. Both hiring and retention rise at treated firms following the subsidy, with the hiring margin quantitatively larger by year 4. This dual expansion is important: the simultaneous increase in both margins means that the firm is growing from within (retaining more incumbents) *and* from outside (recruiting new workers), suggesting that capital investment creates genuine additional labor demand but also decreases separations.

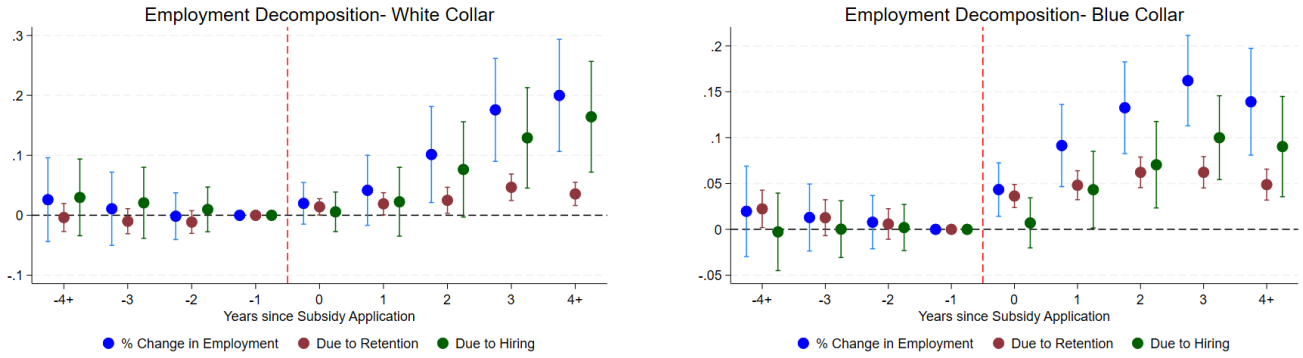
Figure 6 shows the same decomposition separately for white-collar and blue-collar workers. Both skill groups contribute to the employment expansion through both hiring and retention, with the hiring margin quantitatively more important for both groups. Notably, the white-collar hiring response is larger in relative terms, possibly consistent with the complementarity between new capital and skilled workers in manufacturing.

Fig. 5: Subsidized firms grow through both more hiring and better retention.



Notes: Event-study estimates of the firm-level DiD specification for the hiring margin (new workers joining) and the retention margin (incumbents remaining), each weighted by baseline employment. Both margins rise significantly at treated firms in the post-application years with no pre-trend.

Fig. 6: Both skill groups expand through hiring and retention.



(i) White-collar **(ii) Blue-collar**

Notes: Same decomposition as Figure 5, estimated separately for white-collar (left) and blue-collar (right) workers.

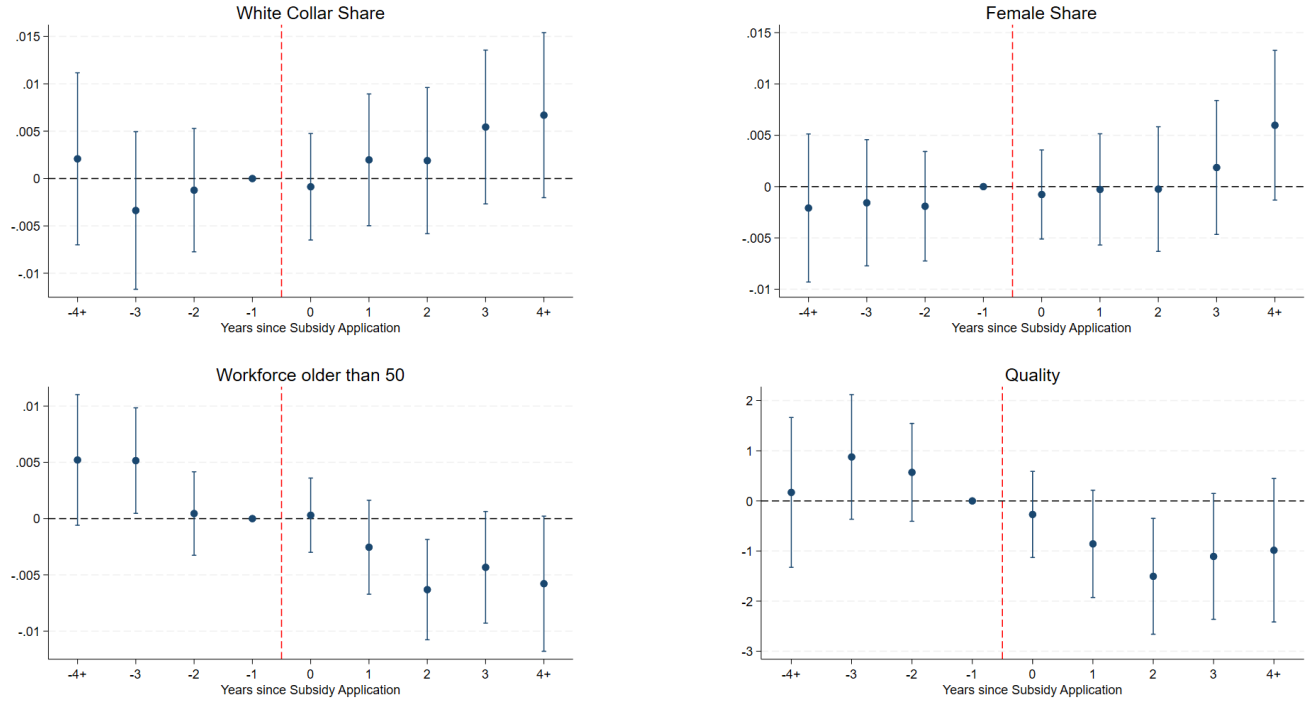
Workforce composition: stability amid expansion. A natural concern when interpreting wage changes at expanding firms is that composition shifts could drive average wage movements. If, for instance, the firm disproportionately retains high-wage incumbents or recruits higher-skill new workers, average wages would rise through a pure composition effect. We directly test for this possibility. Workforce composition-the white-collar share, female share, share of workers over age 50, and an index of average worker quality[24]-does not change significantly following the subsidy (Figure 7). The white-collar share shows no systematic trend; the female share and the age-50+ share are similarly flat across all event-study horizons. If anything, there is a slight decrease in the average worker quality at the firm, which we will show is mostly driven by the new hires. This stability is informative in two ways. First, it validates our decomposition approach: wage changes at stayers reflect genuine within-worker pay adjustments, not a selection bias. Second, it suggests that firms expand primarily within their existing occupational and demographic structure rather than upgrading the skill mix-consistent with the view that subsidies at this scale augment production capacity rather than fundamentally restructure the workforce. This finding stands in contrast to the "skill-upgrading" hypothesis that predicts a compositional shift toward more skilled workers following capital-intensive investment.

4.2 Effects on incumbent workers

We next study how subsidy receipt affects the workers who were already employed at applicant firms before treatment. To do so, we estimate a worker-level event-study specification that follows incumbent workers over time and compares outcomes at subsidized and non-subsidized firms near the funding cutoff. The analysis is restricted to workers employed at firm f before the application year, and since we exploit the longitudinal nature of our worker panel, the estimates capture the response of incumbent workers rather than compositional changes.

[24] As proxied by their predicted earnings, following Kline et al. (2019).

Fig. 7: The workforce does not change in observable composition at treated firms.



Notes: Event-study estimates for four composition measures at subsidized firms: white-collar share (top left), female share (top right), share of workers over age 50 (bottom left), and an index of average pre-application wages (bottom right). None of the four measures exhibits a significant post-application trend, ruling out composition effects as an explanation for the wage gains documented in the text.

For each worker i observed in calendar year t, we estimate

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq -1} \beta_k \, \mathbf{1}\{t - t_f^a = k\} \cdot \text{Treated}_f + \eta \mathbf{X}_{it} + \delta \mathbf{X}_{m(f)t} + \varepsilon_{it}, \tag{6}$$

where α_i are worker fixed effects, λ_t are calendar-year fixed effects, and Treated_f indicates whether firm f received funding. The worker fixed effects absorb all time-invariant worker characteristics, such as permanent differences in ability, attachment, or occupation-specific wage levels, while year fixed effects absorb aggregate shocks common to all workers. The vector $\mathbf{X}_{it}$ includes time-varying worker controls, such as age and gender (as well as their interactions, following Kline et al. (2019)[25]), and $\mathbf{X}_{m(f)t}$ includes time-varying controls at the relevant market level.

Identification comes from comparing incumbent workers employed at firms just above and just below the funding threshold within the same near-cutoff applicant pool. The omitted event-time coefficient is $k = -1$, meaning that each β_k measures the difference in outcomes between workers at winning and losing firms in event year k, relative to the year immediately before the application.

We consider two main outcomes. In the wage analysis, Y_{it} is log average weekly wages or average weekly wages, depending on the specification. In the employment-retention analysis, Y_{it} is an indicator or measure of days worked, capturing whether incumbent workers remain attached to the firm after treatment and

[25]We use a polynomial function, quartic in age fully interacted with gender.

how intensely they are employed. This design allows us to trace the full dynamic response of incumbent workers, separately from firm-wide employment effects, and to test whether subsidies primarily benefit existing employees through higher pay, stronger retention, or both.

A central advantage of this specification is that it isolates within-worker changes over time. Because the same worker is followed before and after the firm's application, the estimates are not driven by cross-sectional differences in workforce composition between subsidized and non-subsidized firms. Instead, they capture how the outcomes of incumbent workers evolve when their employer receives a subsidy relative to otherwise similar workers employed at near-cutoff firms that do not receive funding.

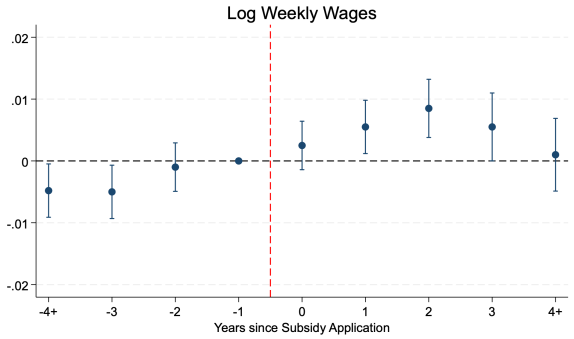
Figure 8 reports the event-study coefficients for the incumbent cohort, pooling together workers who stay at the firm and those who later leave. The growth of average weekly wages for incumbent workers at treated firms rise gradually in the years following the application, with no evidence of differential pre-trends. The same is true for the outcome in levels. Exploiting the longitudinal structure of the data, we further estimate worker retention directly and find that retention increases among workers at winning firms; these results are robust to the inclusion of detailed worker controls (see Table E.17 and E.18). Taken together, these patterns complement the earlier firm-level evidence in Section 3 and indicate that treated firms both pay more and retain workers longer after receiving the subsidy.

Separating incumbents into stayers and leavers reveals a much sharper pattern. Figure 9 shows that workers who remain at treated firms experience clear and statistically significant gains in both log wages and earnings. Figure 10 further decomposes these effects by occupation and shows that they are especially strong for white-collar stayers, while the response for blue-collar stayers is positive but smaller and estimated less precisely. This mirrors the firm-level evidence in Section 3.[26]

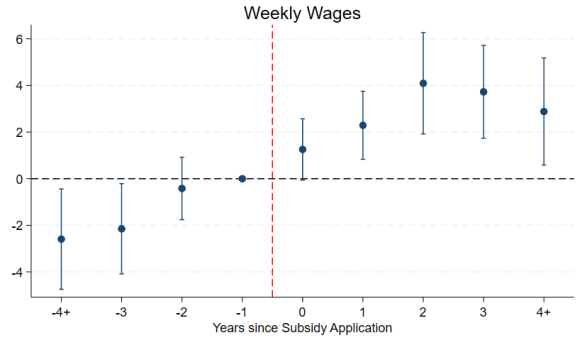
By contrast, workers who leave treated firms show no significant wage response (Appendix Figure E.20). Nor is there evidence that treated firms selectively retain higher-quality workers. Figure 11 reports event-study estimates for four characteristics of leavers: severance pay, future log wages at the next employer, log wages at separation for all leavers, and log wages at separation for job-to-job movers only. None of these outcomes displays a significant post-application change. This rules out a selection-based explanation in which treated firms increase average wages simply by retaining their highest-paid or highest-quality workers. Instead, the evidence points to genuine wage gains for incumbent stayers.

[26] Our results differ from the heterogeneity documented by Card et al. (2014), who find that blue-collar workers are more likely to benefit from rent sharing. Although their study is also set in Italy, it focuses on workers in Veneto, a Northern region widely regarded as one of the country's most entrepreneurial areas. It is therefore plausible that wage-setting institutions and firm-level bargaining operate differently in that context.

Fig. 8: Cohort analysis: incumbent workers (pooled stayers and leavers) at subsidized firms



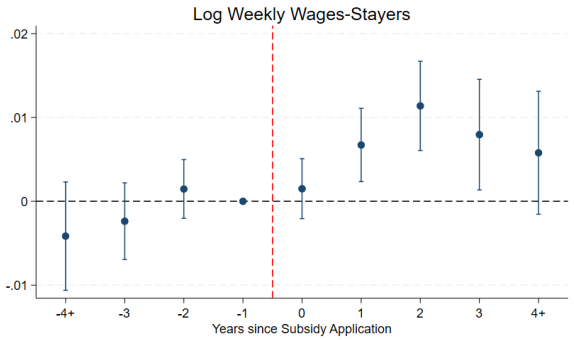
(i) Log wages



(ii) Days worked

Notes: Worker-level event-study estimates (equation (6)) for the incumbent cohort-all workers employed at the firm at the application year-pooling stayers and leavers. Log wages (left) and earnings (right) rise gradually and significantly in the post-application years with no pre-trend. The gains are driven entirely by stayers; see Figure 9.

Fig. 9: Wage estimates for incumbent workers who remain at the applicant firm.



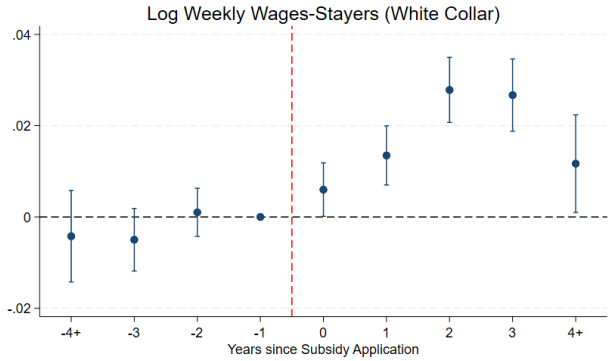
(i) Log wages



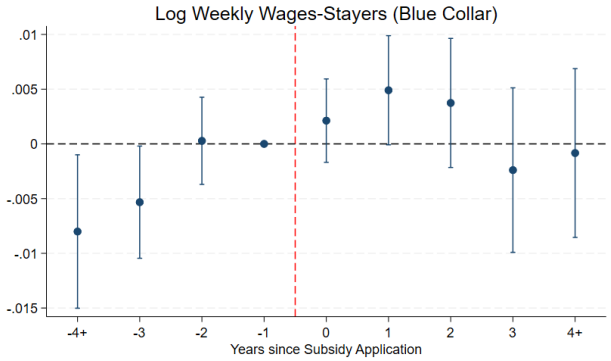
(ii) Days worked

Notes: Worker-level event-study estimates for the stayer subsample. Log average weekly wages (left) and weekly earnings (right) rise by approximately 3-5% over the post-application period at treated firms relative to control firms, with flat pre-trends.

Fig. 10: Wage gains for stayers are concentrated among white-collar workers.



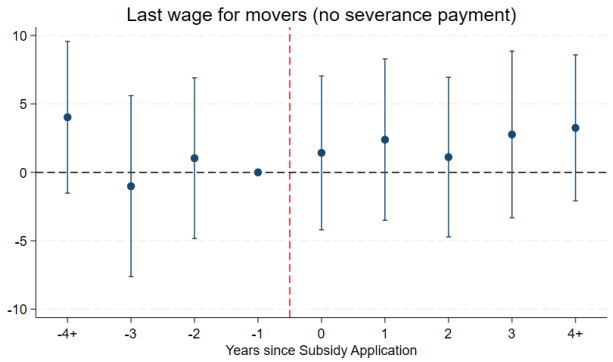
(i) White-collar stayers



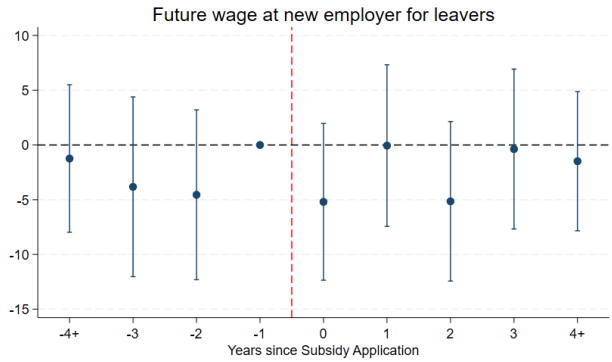
(ii) Blue-collar stayers

Notes: Worker-level event-study estimates for stayers, separately for white-collar (left) and blue-collar (right) workers. White-collar stayers experience substantial and precisely estimated wage gains, while blue-collar stayers show a positive but smaller and less precise response.

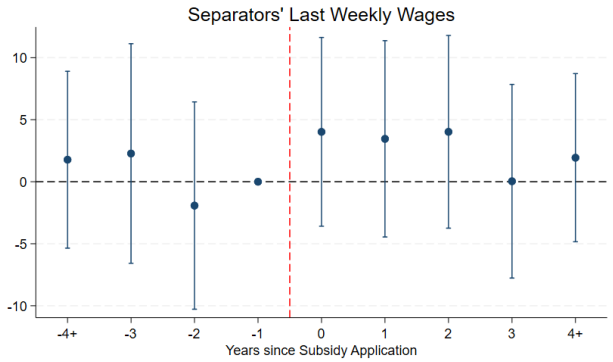
Fig. 11: The composition of workers leaving treated firms does not change.



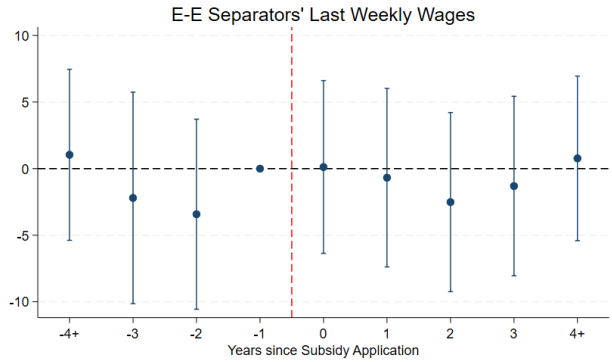
(i) Severance pay



(ii) Future wage at next employer



(iii) Wage at separation (all leavers)



(iv) Wage at separation (job-to-job)

Notes: Event-study estimates for four leaver characteristics: severance pay (top left), future log wage at the next employer (top right), log wage at separation for all leavers (bottom left), and for job-to-job movers only (bottom right). None of the four outcomes shows a significant post-application trend, ruling out the hypothesis that treated firms selectively retain high-wage workers.

4.3 Effects on newly hired workers and within-firm inequality

The previous subsection showed that incumbent workers who remain at subsidized firms experience clear wage gains after treatment. But those results do not tell us whether the policy also improves the pay of workers who are recruited as a result of the subsidy. If instead subsidized firms recruit more workers while leaving starting wages unchanged, then job creation and wage incidence come apart in an important way.

Studying entry wages (i.e., the first wage of a given worker in the firm) requires a different empirical approach from the one we used for incumbents. While for incumbent workers, the longitudinal dimension of the matched employer-employee data allows us to absorb all time-invariant worker heterogeneity, this strategy is no longer available for starting wages.[27] Moreover, the composition of those hires may itself respond to the subsidy: treated firms may recruit workers who differ systematically from those hired by control firms, or hired before the subsidy. Hence, raw average entry wages are therefore difficult to

[27] We only observe starting wages once per worker.

interpret.

To address this concern, we construct a firm-level measure of average entry wages based on residualized individual log wages. Specifically, we first partial out a quartic in age, gender, and their interactions from individual log wages, and then compute, for each firm-year, the mean residualized log wage among workers newly hired in that year. The resulting outcome captures variation in entry pay net of changes in observable composition. Formally, the object of interest is

$$\widetilde{\log w}_{ft} \equiv \hat{E}\Big[\widetilde{\log w}_{it} \mid i \text{ is a new hire at } f \text{ in } t\Big] = \alpha_f + \lambda_t + \sum_{k \neq -1} \beta_k \, \mathbf{1}\{t - t_f^a = k\} \cdot \text{Treated}_f + \delta \text{X}_{m(f)t} + \varepsilon_{ft}, \tag{7}$$

where Y_{ft} is the mean residualized log wage of entrants at firm f in year t. Because this is a firm-level outcome, we estimate it using the firm-level event-study specification in equation (5). This design compares firms that apply in the same local labor market and fall on opposite sides of the funding cutoff, while tracing the dynamic evolution of entry wages before and after the application year.

Figure 12 reports the estimated event-study coefficients. The picture shows a flat coefficient: we find no systematic post-application increase in the years that follow for firms winning the subsidy. Entry wages remain essentially unchanged at all horizons. In other words, although subsidized firms expand and recruit more workers over time, they do not appear to do so by raising the wage paid to new hires.

The absence of an entry-wage response is not driven by the use of residualized wages. If anything, the result is even stronger when we consider unresidualized average entry wages (Figure E.22), suggesting that treated firms, if anything, slightly downskill their inflow of recruits. We test this directly in Figure E.23, which shows event-study estimates for average age, the female share, and average pre-hire log wages among new hires. None changes significantly after treatment. Figure E.24 delivers the same message for workers' origin: there is no significant change in the share of job-to-job hires, entrants from non-employment, workers from the same province, or workers from the same industry.

Fig. 12: New hires' wages do not go up.

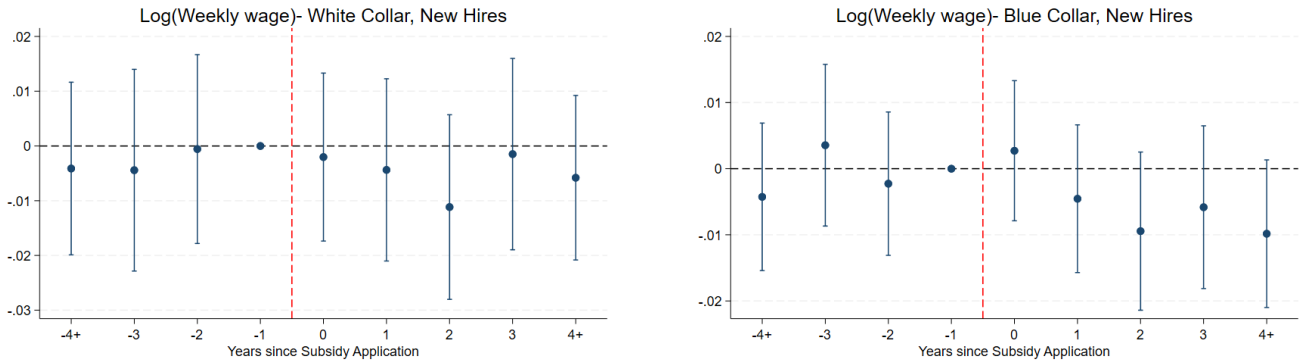


Notes: Firm-level event-study estimates for average entry wages (mean log wage of newly hired workers in year t, residualized for age, gender, and their interactions). This result holds at all post-application horizons.

We next ask whether this flat entry-wage profile conceals offsetting responses across occupations. One

possibility is that subsidized firms raise starting wages for one group of workers but not for another, so that the average masks meaningful heterogeneity. Figure 13 addresses this directly by estimating the same firm-level event study separately for white-collar and blue-collar entrants. The results show that the absence of an entry-wage effect is broad-based. Neither white-collar nor blue-collar new hires experience a statistically significant increase in starting wages at subsidized firms relative to comparable firms just below the cutoff. The point estimates remain small throughout the post-application period, and there is again no evidence of pre-existing differential trends. The null result for entrants is therefore not confined to a single segment of the workforce; it characterizes both major occupational groups.

Fig. 13: The flat entry wage result holds for both occupational groups.



(i) White-collar **(ii) Blue-collar**

Notes: Firm-level event-study estimates for average entry wages, separately for white-collar (left) and blue-collar (right) new hires. Neither group earns more at subsidized than at non-subsidized firms. Residualization controls for a quartic in age, gender, and their interactions.

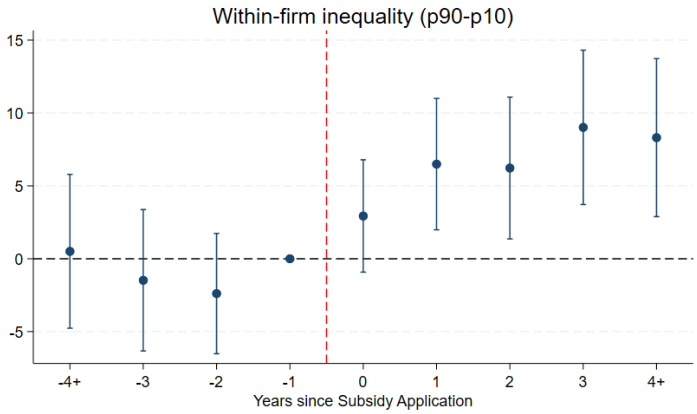
Taken together, these findings imply a sharp asymmetry in the wage incidence of the subsidy within the firm. On the one hand, the program clearly expands employment, and part of that expansion occurs through new recruitment. On the other hand, the workers brought in through that expansion do not receive higher starting wages. By contrast, incumbent workers-and especially incumbent stayers-do experience wage gains. The policy therefore changes not only the level of employment, but also the internal distribution of wages within subsidized firms.

We document this directly in Figure 14, which plots event-study estimates for the within-firm 90-10 log-wage ratio. The figure shows a clear post-treatment increase in wage dispersion at subsidized firms relative to firms just below the cutoff.

4.4 Explaining unequal wage response

We have shown substantial evidence that wage gains are concentrated among incumbent workers-especially white-collar incumbents-while entry wages for new hires show little or no response. We have also already ruled out the possibility that this pattern is driven simply by changes in workforce composition, such as selective retention, differential hiring quality, or broader compositional upgrading, rather than genuine within-worker pay adjustment. We now turn to the mechanisms behind this unequal wage response.

Fig. 14: Within-firm wage inequality rises at subsidized firms



Notes: Event-study estimates for the within-firm 90-10 log-wage ratio at treated vs. control firms. The ratio rises significantly in the post-application years.

Standard benchmark models. This asymmetry is difficult to reconcile with standard benchmark models of the labor market. A static monopsony framework (Manning, 2011; Card et al., 2018) can accommodate heterogeneity across worker groups-for example, if separation elasticities differ between blue-collar and white-collar workers-but it is less well suited to generate large wage responses for incumbents together with muted starting-wage effects for new hires. In those models, a positive firm-level shock that raises surplus or improves outside options would typically spill over to the wages of new hires as well, unless bargaining power or outside options vary sharply with tenure or incumbency status.

Likewise, efficiency-wage mechanisms (Shapiro and Stiglitz, 1984; Akerlof and Yellen, 1990) would tend to predict wage increases that apply more generally, including to new recruits, rather than a pattern concentrated among incumbent stayers. An incentive-contracting story where workers involved in preparing the subsidy application are later rewarded through higher wages is also difficult to square with the evidence. The wage effects we document are indeed persistent rather than short-lived, and although they are stronger for white-collar workers, they are present for workers both below and above the median of the earnings distribution.

Another possible interpretation is an implicit-contracting environment in which financially constrained firms effectively borrow from workers by paying low wages today in exchange for higher future wages once financial constraints relax. However, we do not find evidence that the longest-tenured workers have the strongest claims on the resulting wage gains (Figure E.21), as one would expect under a backloaded compensation or equity-like arrangement. While this mechanism is central in Howell and Brown (2023), the difference in setting is likely crucial. Their framework is well suited to high-tech, small firms that typically rely on external finance in the form of equity rather than debt, whereas our firms are established companies operating in economically depressed areas. Moreover, we do not find that new hires receive higher wage increases relative to their previous jobs, which further weakens the case for this interpretation. Moreover, the absence of a strong tenure gradient in incumbent wage gains also makes it difficult to interpret these effects as disguised dividends to owners paid through wages.[28]

[28]Given the social security contribution nature of the dataset, this channel is unlikely to be quantitatively important in our

Finally, we consider the role of unions. This institutional structure allows us to rule out union efficient-bargaining models (Brown and Ashenfelter, 1986; Abowd and Lemieux, 1993), in which unions can effectively bargain over the entire firm surplus and jointly determine both wages and employment. In those models, the union trades off higher rents per worker against the number of workers who share in the surplus. In our context and period, there is no role for establishment-level unionization in shaping bargaining. Instead, workers within a given sector are covered by collective bargaining agreements negotiated at the national level between the main union confederations, employer representatives, and government actors. As a result, firm-specific shocks do not lead individual firms to renegotiate their own contracts with a plant-level union.

More broadly, however, unions still matter because they shape the wage-setting rules under which firms operate. We therefore turn next to how collective bargaining institutions influence the transmission of the subsidy to workers' wages.

Collective bargaining and the asymmetric wage response. We next bring in new data on collective bargaining agreements to study whether the unequal wage response documented above maps into the institutional structure of wage setting. The key advantage of these data is that they allow us to observe, for each worker-contract cell, the nationally negotiated contractual minimum wage implied by the relevant collective agreement. Merged to the INPS employer-employee archive, they make it possible to decompose observed earnings into two conceptually distinct components: the contractual floor set outside the firm, and the residual component above that floor, which captures the more discretionary part of pay.

The descriptive evidence reported in Table 3 points to a stark asymmetry between incumbents and entrants (for more results, please refer to Appendix Table E.19 and E.20). For incumbent workers at IDA applicant firms, the CBA base wage accounts for 78% of total earnings on average, with a median of 75%. This implies substantial room for bargaining between firm and worker, allowing them substantial premia above the contractual minimum. For newly hired workers, by contrast, the CBA base wage accounts for 100% of earnings on average, with a median of 89%, indicating that starting wages are in practice tightly anchored to the nationally negotiated minimum.

This contrast is also economically meaningful across occupations. Among blue-collar workers, the contractual floor is close to binding for most employees, leaving relatively little room for top-ups. Among white-collar workers, instead, observed pay exhibits substantially greater dispersion around the contractual minimum, together with a higher incidence of firm-level supplements. This mirrors the reduced-form evidence above: wage effects are strongest precisely for the workers whose compensation includes a larger margin above the national floor.[29]

Taken together, these facts point to a simple mechanism: wage-setting institutions do not eliminate firm-level wage adjustment, but they shape where within the firm that adjustment occurs. Entry wages remain closely tied to nationally negotiated minima, while incumbent pay contains the more flexible component through which firms can share rents and strengthen retention after treatment.

dataset. Moreover, we remove firm managers to decrease the risk of capturing this effect.

[29] In unreported evidence from a Bank of Italy's survey on manufacturing firms, also firms report that wage drift above the national contract is common for incumbent workers, especially white-collar workers, but much less prevalent for new hires across occupational categories.

Table 3: Contractual minima and actual pay

	Incumbents		New hires	
	Mean	Median	Mean	Median
All workers	0.784	0.752	1.054	0.887
White-collar	0.744	0.687	1.003	0.888
Blue-collar	0.797	0.767	1.094	0.886

Notes: Entries report the share of total earnings accounted for by the contractual minimum wage implied by the relevant collective bargaining agreement. Full distributions and complementary measures of pay above the contractual floor are reported in Appendix Tables E.19 and E.20.

Pending disclosure approval, we will report additional evidence that makes this mechanism even more transparent. In particular, we plan to show a more detailed decomposition of pay into contractual and above-contract components by worker type, as well as complementary evidence on the distribution of firm-level premia across incumbents and entrants. Those results will allow us to illustrate more directly that the subsidy primarily shifts the flexible component of incumbent pay, rather than the wage floor faced by newly hired workers.

5 Effects on markets: spillovers to non-participating firms

5.1 Motivation

The firm-level results show that the IDA policy increased employment and wages at subsidized firms, with wage gains concentrated among incumbent workers. We now ask whether these direct effects spilled over to *other* firms operating in the same local labor market. This question is central to understanding the incidence and welfare consequences of place-based policies. If subsidies benefit only treated firms, the policy may simply reallocate activity across firms or locations. By contrast, if non-treated local firms also benefit, this points to broader market-level effects and suggests that the policy generated positive externalities beyond the targeted firms themselves.

There are at least two reasons why such effects may arise. First, treated firms may affect the local labor market through standard labor demand channels. If subsidized firms expand and raise pay for their incumbent workforce, competing firms may need to raise wages as well in order to retain workers. In addition, subsidized firms may increase demand for the inputs produced by other local firms. Through these input-output linkages, part of the subsidy-induced gains may be transmitted to other firms and, in turn, to their workers. Second, the policy may generate broader local spillovers in the spirit of Greenstone et al. (2010). By increasing economic activity in a market, subsidies may raise local demand more than proportionally, strengthen agglomeration economies, and improve outcomes even for firms that did not receive support.

At this stage, we do not attempt to separately identify the precise mechanism behind these effects. Rather, the contribution of this section is to show that the incidence of the policy extends beyond the firms that

directly receive subsidies and affects the broader local labor market.[30]

5.2 Empirical specification and endogeneity concern

Within each market m (province $\times$ 2-digit sector cell), we distinguish between *applicant firms*, which participated in the IDA program, and *non-participating firms*, which did not apply. The focus of this section is entirely on the latter group. We ask whether outcomes at non-participating firms respond to policy-induced changes in total market employment.

Our baseline specification is

$$\Delta \log Y_{j(m)} = \gamma \, \Delta \log E_m + \eta \mathbf{X}_m + \alpha_p + \alpha_k + \varepsilon_j, \tag{8}$$

where j indexes non-participating firms in market m. The dependent variable $\Delta \log Y_{j(m)}$ is either log wage growth or log employment growth at non-participating firm j between 1995 and 2001.[31] The key regressor, $\Delta \log E_m$, is total employment growth in market m over the same period, computed across all firms in the market, including both IDA applicants and non-applicants. The vector $\mathbf{X}_m$ includes market-level controls, while α_p and α_k denote province and sector fixed effects. The parameter of interest, γ, measures how an increase in total market employment affects outcomes at non-participating firms.

The main empirical challenge is that local employment growth is endogenous. Markets with weaker fundamentals may both be more heavily targeted by the policy (growing much faster in scale) and have firms that are on a negative wage growth trend, so that a simple cross-market comparisons would confound policy effects with underlying differences in market conditions. To address this problem, we construct an instrument that isolates the quasi-random component of policy-induced market growth.

5.3 Non-random exposure to policy shock

As explained in Section A, subsidies were allocated within administrative funding pools: within a given region and competition year, firms only competed with similar firms.[32] Within each funding pools, applications were ordered by score and funded sequentially until the available budget was exhausted.

Among firms far from the funding threshold, treatment status is essentially deterministic: very strong applications are funded, and very weak ones are not. By contrast, among firms close to the cutoff, subsidy receipt depends on small differences in rank and on the composition of requested subsidy amounts relative to the funding pool's total budget. This creates quasi-random variation in *which* near-cutoff firms receive funding, even holding fixed the number of applicants close to the cutoff and the overall budget of the funding pool.

We exploit this feature to construct exogenous cross-market variation in policy exposure. Intuitively, some

[30] A structural decomposition of the underlying channels will be developed in a future version of the paper.

[31] The reason we start by focusing on this time difference is because the bulk of money was disbursed in the first 4 years of the policy (1996-1999), and since the employment effect is already significant after 2 years, we expect this to be a good approximation of the time horizon when static labor demand effects should have materialized.

[32] Funding pools were a function of firm size, manufacturing dummy, and European co-financing/objective area.

markets contain near-cutoff firms that, by chance, end up receiving more funding than one would expect given the number and type of applicants in that market. Other markets contain near-cutoff firms that receive less. This quasi-random variation in realized subsidy allocation translates into quasi-random variation in market-level employment growth.

Fix a funding pool p, and let $i(p)$ index applicant firms in that group. For each firm, we observe its requested subsidy amount, denoted $a_{i(p)}^{\text{request}}$, and its actual treatment status,

$$z_{i(p)} = \mathbb{1}\{\text{firm } i \text{ is funded in funding pool } p\}.$$

Because funding is assigned in score order until the budget is exhausted, treatment depends not only on a firm's own score but also on the subsidy amounts requested by higher-ranked firms. Let

$$b_p = \sum_{i \in p : z_{i(p)}=1} a_{i(p)}^{\text{awarded}}$$

denote the total budget allocated in funding pool p. We define the set of near-cutoff firms infunding pool p as

$$\mathcal{C}_p = \left\{ i \in p : \left| \text{ProjectScoreStd}_{i(p)} \right| \leq \delta \right\},$$

where $\text{ProjectScoreStd}_{i(p)}$ is the standardized distance between the firm's project score and the funding cutoff in that funding pool as defined in Section 3, and δ is the bandwidth used to define the near-cutoff sample.

We then simulate S counterfactual subsidy allocations within each ranking group. In each simulation s, we randomly reshuffle the firms in $\mathcal{C}_p$ and assign funding sequentially, respecting the same budget constraint b_p and using the observed requested subsidy amounts. Firms outside $\mathcal{C}_p$ keep their actual treatment status in every simulation. Let $z_{i(p)}^{(s)}$ denote simulated treatment status:

$$z_{i(p)}^{(s)} = \begin{cases} \text{simulated assignment within } \mathcal{C}_p & \text{if } i \in \mathcal{C}_p, \\ z_{i(p)} & \text{if } i \notin \mathcal{C}_p. \end{cases}$$

Each simulation therefore represents an alternative, budget-feasible realization of who could have won among firms near the cutoff, holding fixed the structure of the competition and the treatment of firms far from the margin.

To translate these simulated assignments into market-level exposure, we use promised jobs to construct counterfactual employment changes for subsidized applicant firms. For all firms, whether they participated or not in the policy, we predict a level of employment in 2001 based on a parsimonious Pseudo Maximum Likelihood procedure (described in Appendix F). For firms that did participate in the program, and that won close to the cutoff in simulation s, we also add their promised jobs. More details are provided in Section F.2.

We then aggregate across firms to obtain simulated market-level employment growth. Averaging across

simulations gives the expected policy-induced employment growth for market m,

$$\mu_m^{\text{Scale}} = \frac{1}{S} \sum_{s=1}^{S} Z_m^{\text{Scale},(s)}$$

Our instrument is the recentered shock

$$\tilde{Z}_m^{\text{Scale}} = Z_m^{\text{Scale, obs}} - \mu_m^{\text{Scale}}$$

This recentering step is crucial. Markets with more applicant firms, or with applicants requesting more jobs on average, will mechanically have higher expected exposure to the policy. By subtracting the simulated expectation, we net out this predictable component and isolate only the quasi-random part coming from which near-cutoff firms happened to win. The instrument therefore captures whether a market received more or less policy-induced employment growth than would be expected based on its underlying applicant pool alone.

5.4 Validating the design

First, we check there is substantial cross-market variation in employment growth over the policy period. In the Appendix, Table F.21 reports summary statistics for the province-sector markets in the estimation sample. Between 1995 and 2001, average employment growth was positive, but dispersion across markets was large: some markets expanded sharply, while others contracted.

Figure 15 reports the first-stage relationship between the recentered instrument $\tilde{Z}_m^{\text{Scale}}$ and realized market employment growth $\Delta \log E_m$, once we residualize for fixed effects for province and industry, and we bin the resulting estimates in 30 bins[33]The relationship is strong and positive: markets in which near-cutoff firms happened to receive larger-than-expected subsidy exposure experienced faster employment growth. The corresponding first-stage F-statistics range from roughly 29 to 31, comfortably above conventional thresholds.

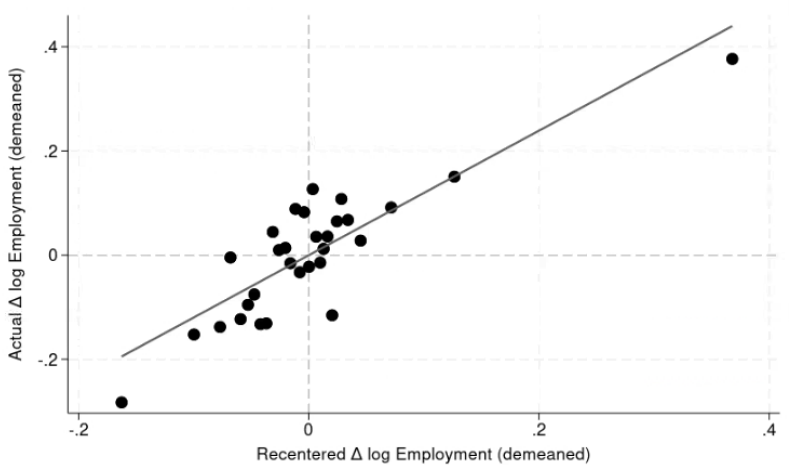
It is also important that the instrument not simply proxy for pre-existing market characteristics. For this reason, Appendix Table F.22 shows that the recentered shock is uncorrelated with baseline market observables, supporting the interpretation that it captures quasi-random variation in subsidy exposure rather than underlying differences in local trends.

5.5 Results

Wage spillovers to non-participating firms. Table 4 presents the main results for wage growth at non-participating firms. Odd-numbered columns report OLS estimates, while even-numbered columns report 2SLS estimates using $\tilde{Z}_m^{\text{Scale}}$ as an instrument for market employment growth. The specifications differ in the set of market-level controls included, but the qualitative pattern is stable across columns.

[33]This mirrors the exercise in Nakamura and Steinsson (2014).

Fig. 15: The recentered instrument strongly predicts market employment growth



Notes: Binned scatter plot of market log employment growth $\Delta \log E_m$ (1995-2001) against the recentered instrument $\tilde{Z}_m^{\text{Scale}}$, residualized with respect to province and sector fixed effects.

The OLS coefficients are small and slightly negative. This is consistent with downward bias from endogenous market growth: firms in markets with unfavorable fundamentals and declining wage growth may also be the ones growing more due to the influx of funds. Once we isolate the quasi-random component of market expansion using the instrument, the estimates become positive, economically meaningful, and statistically significant. Across specifications, a 10% increase in market-wide employment raises wages at non-participating firms by about 0.8 to 1.2 percentage points.

Table 5 repeats the analysis using employment growth at non-participating firms as the dependent variable. The OLS coefficients are strongly positive, reflecting the fact that firms in expanding markets also tend to grow themselves. The IV estimates remain positive, but they are smaller and imprecisely estimated.

Table 4: Policy-induced market expansion raises wages at non-participating firms

	(1)	(2)	(3)	(4)
Dep. variable: Δ log competitor wages	OLS	IV	OLS	IV
Scale: $\Delta \log E_m$	-0.007	0.123***	-0.011	0.084**
	(0.008)	(0.042)	(0.008)	(0.040)
Observations	138,774	138,774	138,774	138,774
F-stat		31.61		29.08
FE (Industry)	Yes	Yes	Yes	Yes
Specialization controls	Yes	Yes	No	No
Other regional controls	No	No	Yes	Yes
Clustering	Market	Market	Market	Market
Mean Δ log wage	0.226	0.226	0.226	0.226
Mean baseline log wage	6.625	6.625	6.625	6.625

Notes: OLS and 2SLS estimates of equation (8), where the dependent variable is wage growth at non-participating firms between 1995 and 2001. Even-numbered columns instrument market employment growth with the recentered shock $\tilde{Z}_m^{\text{Scale}}$. Standard errors are clustered at the market level.

A central implication of this result is that the incidence of the subsidy extends beyond the firms that receive it directly. The pattern of results also provides suggestive evidence on the main equilibrium adjustment

margin. When the policy expands employment in a local labor market, non-participating firms in the same market also raise wages; while wage spillovers are sizable, employment spillovers are much less precisely estimated.

This asymmetry suggests that competing firms respond to policy-induced market expansion primarily through wage-setting rather than through large employment adjustments. In other words, when local labor demand rises, the main response of non-treated firms appears to be to pay more in order to retain workers, consistent with firms facing upward-sloping labor supply and wages respond to changes in workers' outside opportunities, as in Manning (2003, 2021). The absence of an employment response makes it less compatible with broader local productivity or agglomeration channel (Greenstone et al., 2010; Moretti, 2011).

In the next version of the paper, we will look at the effect on other firms' TFP (using balance sheet data), as well as estimating this exercise at the worker-level (for workers employed at non-participating firms), in order to account for changes in composition of the workforce.

Table 5: Employment spillovers to non-participating firms are positive but imprecisely estimated

	(1)	(2)	(3)	(4)
Dep. variable: Δ log competitor employment	OLS	IV	OLS	IV
Scale: $\Delta \log E_m$	0.164***	0.118	0.163***	0.084
	(0.019)	(0.079)	(0.019)	(0.082)
Observations	138,776	138,776	138,776	138,776
F-stat		31.61		29.08
FE (Industry)	Yes	Yes	Yes	Yes
Specialization controls	Yes	Yes	No	No
Other regional controls	No	No	Yes	Yes
Clustering	Market	Market	Market	Market
Mean Δ log employment	0.112	0.112	0.112	0.112
Mean baseline employment	0.502	0.502	0.502	0.502

Notes: OLS and 2SLS estimates of equation (8), where the dependent variable is employment growth at non-participating firms between 1995 and 2001. Even-numbered columns instrument market employment growth with the recentered shock $\tilde{Z}_m^{\text{Scale}}$. Standard errors are clustered at the market level.

6 Next steps and conclusion

This paper traces the incidence of one of the largest place-based entrepreneurship subsidies in modern history-Italy's IDA program-from subsidized firms, to their workers, and further to competing firms in the surrounding local labor market. Three main findings emerge.

First, subsidized firms expand substantially. Employment rises by 7-13% within two to three years, and average wages increase as well. The joint rise in employment and wages implies that local labor supply to the firm is upward sloping: treated firms do not simply hire more workers at unchanged wages, but must also pay more to attract and retain labor.

Second, the worker-level incidence of these gains is highly uneven. The wage effects are concentrated entirely among incumbent workers who remain at the subsidized firm, while new hires and incumbent workers who separate receive essentially no wage premium. This pattern is not driven by compositional

change; it reflects the institutional structure of wage setting in Italy. The result is a striking distributional implication: although the policy was designed to stimulate employment and improve local economic conditions, the wage gains accrue primarily to insiders rather than to the newly hired workers the program was partly meant to help.

Third, the effects of the policy extend beyond directly treated firms. We document positive wage spillovers to non-participating firms operating in the same local labor market. Using our recentered instrument-which isolates the quasi-random component of which near-threshold firms happened to be funded in each market-we find that a policy-induced increase in local employment raises wages at competing firms, while the effects on employment are not precisely estimated. This implies that the a sizable share of the incidence operates through equilibrium effects in the broader labor market.

Taken together, these results paint a nuanced picture of place-based entrepreneurship subsidies. In aggregate, the policy works: it creates jobs, raises wages, and generates positive spillovers in the local economy. But who benefits, and through which margins, depends critically on labor-market institutions. In a setting where labor markets are frictional and wage-setting is not governed by market clearing, the rents generated by the subsidy are shared mainly with incumbent workers, while new hires are largely excluded from wage gains. More broadly, the paper shows that the incidence of place-based policy is shaped not only by firm responses, but also by the institutional architecture through which firms recruit, retain, and pay workers.

A natural next step we plan on including is to quantify these distributional and equilibrium effects in a unified welfare framework. Doing so would make it possible to compare the gains accruing to firms, incumbent workers, new hires, and competing employers, and to evaluate how alternative policy designs might change this allocation. More generally, the findings suggest that place-based policies should be understood not simply as firm subsidies, but as interventions that interact deeply with local labor market power, wage-setting institutions, and the broader economic geography of the places they target.

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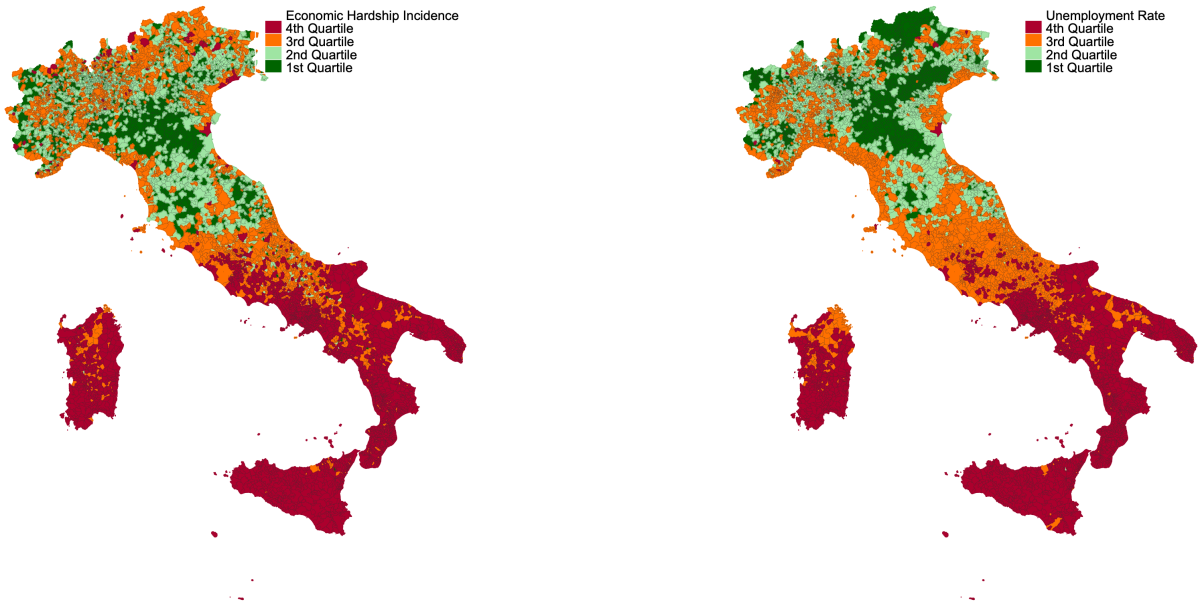
A Context

A.1 Spatial Heterogeneity in Italian Labor Markets

The policy we focus on disbursed subsidies in depressed areas (75% to the South of Italy) between 1996 and 2007. For this reason, in this section, we highlight some key features of Italian labor markets at baseline and characterize how economic deprivation, unemployment, firm size, and labor market power change across regions. We also situate the IDA program within the broader European Cohesion Policy framework and discuss its connections to analogous place-based instruments in the United States.

Deprivation. Figure A.1 highlights these imbalances by showing the spatial distribution of economic hardship (left panel) and unemployment rates (right panel) in 1991. The maps reveal a clear North-South divide: the Northern regions generally experience lower levels of economic hardship and unemployment, as represented by the dominance of green and lighter shades indicating the first and second quartiles, while the Southern regions are significantly more affected by both economic hardship and unemployment, with large areas concentrated in the fourth quartile, shown in red.

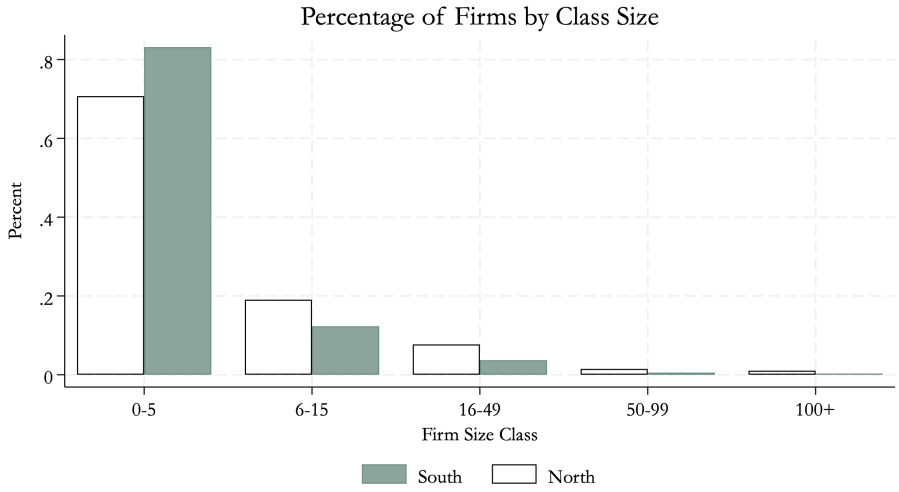
Fig. A.1



(i) **(ii)**

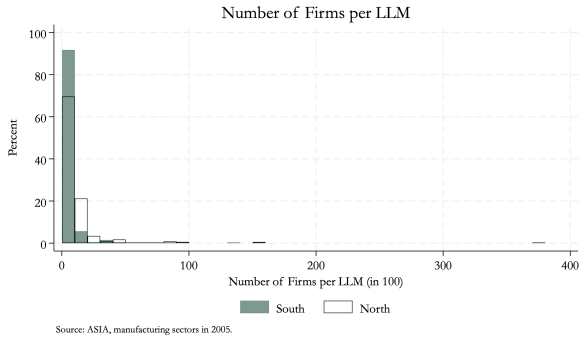
Firm size and number Figure A.2 shows that the South has a higher concentration of very small firms (0-5 employees) relative to the North. This pattern is reinforced in Figure A.3, where the number of firms per Local Labor Market (LLM) is far lower in the South compared to the North, as shown in both the histogram (panel i) and the cumulative distribution function (panel ii). Finally, Figure A.4 shows that the concentration of manufacturing employment in small firms (0-5 employees) is disproportionately higher in the South. The histogram (panel i) indicates that Southern LLMs exhibit a higher share of employment concentrated in small firms, while the empirical cumulative distribution function (panel ii) confirms that the employment share in small firms in the North first-order stochastically dominates that in the South.

Fig. A.2: Heterogeneity in Firm class distribution



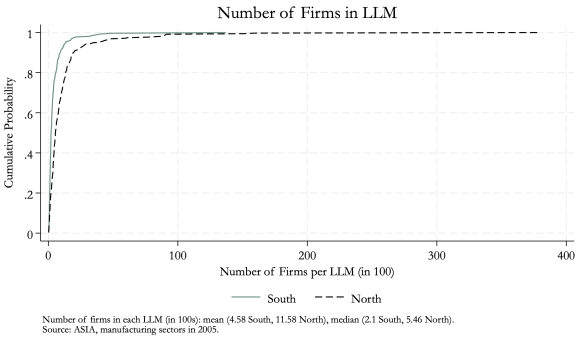
Source: ASIA, manufacturing sectors in 2005.

Fig. A.3: Firm size distribution across space



Source: ASIA, manufacturing sectors in 2005.

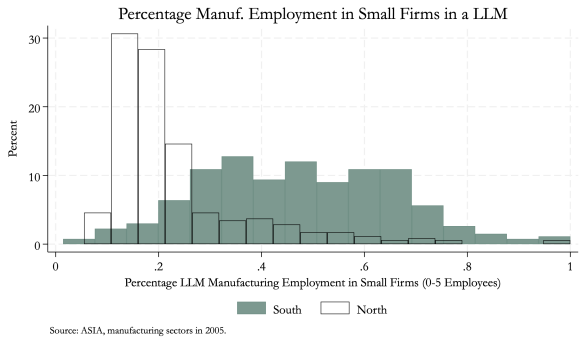
(i)



Number of firms in each LLM (in 100s): mean (4.58 South, 11.58 North), median (2.1 South, 5.46 North).
Source: ASIA, manufacturing sectors in 2005.

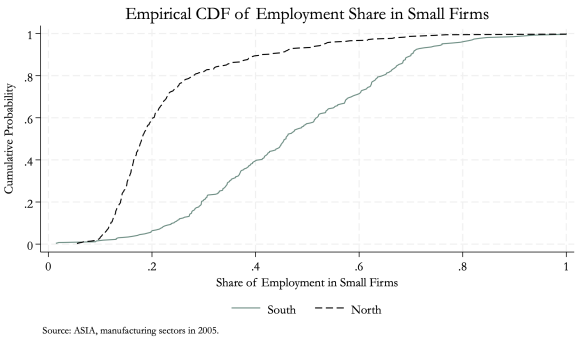
(ii)

Fig. A.4



Source: ASIA, manufacturing sectors in 2005.

(i)



Source: ASIA, manufacturing sectors in 2005.

(ii)

Firm size In 1995, the average firm size in manufacturing was larger in the North, with an average of 17.03 employees per firm compared to 11.38 in the South (median: 5 vs. 3). Beyond having smaller firms, the

South also had fewer firms overall. On average, a local labor market[34] had 34 firms, though there was significant regional heterogeneity. In the South, the average was just 21 firms-half the number found in the North. The median number of firms was also higher in the North compared to the South (8 vs 5).

Decomposition of employment growth (confidential data). In the pre-policy period (1990-1995), manufacturing employment growth at the market level was primarily driven by the extensive margin (firm entry and exit), which was positive, while firm intensive margin was negative (both in North and South).[35] During the same period, the average share of new entrants-measured as a percentage of firms in the manufacturing market at baseline-was higher in the South than in the North, averaging 13% in the South compared to 11.5% nationwide.

Firm entry and number of plants per firm across markets. Defining a market as a combination of NACE2-province (for manufacturing), the average firm operated in 1.01 markets in 1995 (median: 1). Only 0.83% of firms were active in multiple markets. Firm turnover was substantial: in 1995, on average, labor market sees 13% of new firms entering, while 7% exited. Entry rates were higher in the South compared to the North. Between 1990 and 1995, labor markets in the South had an average employment level of 739 (median: 249).

Firm relocation. Evidence on firm location decisions in Italy is scarce.[36] Previous policy research show that urban areas, which naturally attract relocations, tend to yield greater productivity gains, suggesting that firms favor locations with existing agglomeration benefits. However, most relocations are short-range (half involve displacements of 14 km or less), and relocating firms are typically larger, more productive, and younger-traits that may not align with firms likely to enter less desirable markets.

Using admin data, we calculate relocation rates for the period 1990-2004, and we show that this number is particularly low for manufacturing firms (1.7%), while higher in the overall sector (7%) in the period.

A.2 The IDA Program in the European Cohesion Policy Framework

EU Cohesion Policy: origins and scope. Italy's IDA program did not emerge in a vacuum. It was embedded within the European Union's Cohesion Policy-the EU's principal instrument for reducing economic disparities across member states and regions. Established under the Single European Act (1986) and operationalized through the Structural Funds, Cohesion Policy channels resources to "less developed" regions (defined as those with GDP per capita below 75% of the EU average) through three main instruments: the European Regional Development Fund (ERDF), the European Social Fund (ESF), and-for the poorest member states-the Cohesion Fund. Over the 2021-2027 programming period alone, Cohesion Policy committed €392 billion, making it the largest single line item in the EU budget and dwarfing comparable programs elsewhere.

[34] Defined as NACE3 (ATECO3)-province

[35] Based on methodology described in Structural Change and Firm Dynamics in the South of Italy: the intensive margin reflects employment growth among surviving incumbent firms (between 1990 and 1995), while the extensive margin captures the net employment change from firm entry and exit. Each component's contribution is weighted by the initial employment shares of incumbents and exiting firms.

[36] Statistics are retrieved from two Bank of Italy presentations: Lamorgese (2018) link, and Lamorgese and Petrella (2018) link

The central organizing principle of Cohesion Policy is territorial convergence: resources flow from rich to poor regions, explicitly targeting investment in infrastructure, enterprise support, and human capital. Within this framework, each member state designs its own national programs subject to EU co-financing rules and eligibility criteria. Italy was a major beneficiary: its southern regions (*Mezzogiorno*), classified as Objective 1 areas throughout the 1990s and 2000s, received the highest co-financing rates and the largest per-capita allocations in the EU.

IDA as a Cohesion Policy instrument. Law 488/1992 was Italy's primary vehicle for deploying EU structural funds alongside national resources in eligible depressed areas. The program's eligibility map was directly shaped by EU Objective area classifications-Objective 1 (regions with GDP per capita below 75% of EU average, covering most of Southern Italy), Objective 2 (regions in industrial decline), and transitional support areas together covered 48% of the Italian national population. The European Commission set binding caps on maximum subsidy rates that varied by firm size, region, and Objective classification (see Appendix A.4), integrating IDA tightly into the state-aid regulatory framework for Cohesion Policy instruments.

This institutional embedding has two implications for our analysis. First, the program's design reflects EU-level priorities that emphasize both firm creation *and* regional convergence, making it a direct forerunner of today's ERDF-financed entrepreneurship and SME support programs across the EU. Second, the variation in subsidy rates across regions and firm sizes-driven by EC guidelines rather than Italian administrative discretion-provides additional identifying variation that we exploit in our heterogeneity analysis.

Connection to US place-based policy. The IDA program is part of a broader global wave of place-based subsidies whose rationale and design mirror debates playing out in the United States. The *Opportunity Zones* program (Tax Cuts and Jobs Act, 2017) directs capital-gains tax deferrals toward distressed census tracts with a design centered-like IDA-on attracting new investment rather than subsidizing incumbents. The *CHIPS and Science Act* (2022) commits over $52 billion in manufacturing grants explicitly to build new semiconductor plants in targeted locations, mirroring IDA's new-plant focus. The *New Markets Tax Credit* (NMTC, 2000-present) channels over $60 billion into low-income communities through equity investments in businesses. Closer in structure to IDA is the *Small Business Innovation Research* (*SBIR*) program-federal R&D grants to small high-tech firms allocated through ranking competitions, studied with an RDD design by Howell and Brown (2023) and Howell (2017)-whose results we compare directly with ours in the main analysis. Despite their different forms, all these programs share the fundamental premise that market failures in capital allocation across space justify public intervention. Whether such intervention creates "good jobs" for workers-and which workers-is the central empirical question that unites our analysis with recent US-focused work (Slattery and Zidar, 2020; Kline and Moretti, 2014a; Busso et al., 2013).

A.3 Type of eligible projects

Eligible investment projects covered a broad set of firm activities aimed at creating, expanding, or reorganizing production capacity. These included the establishment of new plants; expansions, either through higher capacity for existing or new products (horizontal expansion) or through the addition of upstream or downstream stages of production within the same facility (vertical expansion); and modernization

projects introducing innovations to raise productivity or improve environmental conditions. The program also supported restructuring interventions aimed at rationalizing organization, upgrading technology, or improving production processes, as well as conversion projects in which firms replaced existing products by shifting into different sectors through changes in production cycles. In addition, firms could receive support for reactivation projects that resumed operations at inactive facilities, and for transfers involving the relocation of plants when required by public-administration orders or by urban and environmental restructuring needs.

A.4 Maximum subsidy set by EC

Within the EU regional policy framework, territorial eligibility was organized around distinct categories of economic disadvantage. Objective 1 identified regions affected by broad underdevelopment, defined by GDP per capita below 75% of the EU average; in Italy, this category included seven Southern regions, excluding Abruzzo. Objective 2 captured areas facing industrial restructuring, as indicated by relatively high unemployment, a strong concentration of employment in declining industries, and observable sector-specific job losses. Objective 5b instead covered predominantly rural and peripheral areas, characterized by a high incidence of agricultural employment and low agricultural income. In Italy, the areas classified under Objectives 2 and 5b were all located in the Centre-North, together with Abruzzo, as were the areas approved under Article 92(3)c but excluded from Structural Fund eligibility. These territorial distinctions mattered directly for policy design, since they shaped the geography of aid eligibility and the ceiling on subsidy intensity across areas.

Table A.1: Maximum Subsidy Rates 1995-1999.

Areas	Small-Medium	Large
Province di Benevento, Potenza, Catanzaro, Cosenza, Crotone, Reggio Calabria, Vibo Valentia, Agrigento, Caltanissetta, Enna, Messina, Trapani, Nuoro, Oristano	65%	50%
Province di Avellino, Caserta, Napoli, Salerno, Matera, Bari, Brindisi, Foggia, Lecce, Taranto, Catania, Palermo, Ragusa, Siracusa, Cagliari, Sassari	55%	40%
Abruzzo	30%	25%
Molise (until 30/06/1995)	55%	40%
Molise (01/07/1995-31/12/1996)	45%	35%
Molise (01/01/1997-31/12/1998)	40%	30%
Molise (01/01/1999-)	30%	25%

Table A.2: Maximum Subsidy Rates 1995-1999

Areas	Small Enterprises	Medium-Large Enterprises
Obiettivo 2 and 5b (derogation art. 92.3.c)	20%	15% (medium), 10% (large)
Other Obiettivo 2 and 5b	15%	7.5%
No 1, 2 or 5b, but derogation art. 92.3.c	20%	15% (medium), 10% (large)

Table A.3: Maximum Subsidy Rates 2000-2006

Area	Aree in deroga	Small	Medium	Large
Southern regions	Aree 87.3.a - Calabria	60	60	50
	Aree 87.3.a - other	45	45	35
Molise	Aree 87.3.c	27	27	20
	Aree non in deroga	10	5	-
Abruzzo	Aree 87.3.c	27	27	20
	Aree non in deroga	10	5	-
Centro-nord	Aree 87.3.c)	15	12	8
	Aree non in deroga	10	5	-

B IDA: what and where is getting subsidized?

We use our dataset on subsidy applications to document three key facts: (i) the majority of subsidies are allocated to firms opening new plants, which typically promise more jobs per unit of investment; (ii) new plants are predominantly built by small firms, with notable variation across local labor markets; (iii) firms prioritize specific labor market characteristics when applying for new-plant subsidies, often in ways inversely correlated with what would be socially desirable.

B.1 Which markets are subsidized?

When linking the data to the administrative archives, we retrieve 40,683 firms, of which 17,487 (36%) received financing, covering 19,900 projects. Among these, 8,607 firms were financed specifically to open new plants (8,824 projects); more than half operate in manufacturing (4,818 firms, 4,911 projects). We retrieve 2,610 new plants opened in a new province where the firm was not active before 488 funding-half in manufacturing-appearing on average 2.6 years after the funding date. Defining a market as a province, the average number of financed projects per market is 200 (median 135); restricting to manufacturing, the mean is 129 (median 92). At baseline (1995), manufacturing markets receiving funded projects have 156 firms on average (median 57).

B.2 Which projects are subsidized?

Table B.4 summarizes spending patterns on funded projects. Projects for constructing new plants receive half of the spending even though they are relatively fewer. The mean grant for opening a new plant is approximately €617,000.

We then turn to examine the commitments in terms of employment and capital that firms make in the application process. In Table B.5, we show that for new plant creation, nearly all firms commit to investing their own capital and creating jobs. However, this pattern does not hold for projects classified as other types, where the commitment to employment and capital investment is weaker.

In Figure B.5, panel (i), we show that new plants consistently generate more jobs per unit of investment compared to other projects. However, job creation per unit of capital varies significantly with firm size: larger firms appear less likely to create jobs relative to their investment levels (panel ii).

In Figure B.6, the left panel displays the distribution of promised jobs by investment type: new plants average 20.64 promised jobs versus 12.66 for expansions/other investments. The right panel shows total investment distributions, which are more similar across project types.

Table B.6 shows variation in the type of submitted projects by firm size: small firms submit more new-plant projects, while medium and large firms undertake other project types. The manufacturing sector receives the largest support, and funds are concentrated in the South of Italy, with variation across municipalities.

Table B.4: New-plant projects receive half of total IDA spending despite being fewer in number.

	Total Subsidies (M)	Mean Subsidy	N. Projects	Share Spending (%)
Other	5997.39	530836.6	11298	49
New Plant	6145.25	616807.3	9963	51

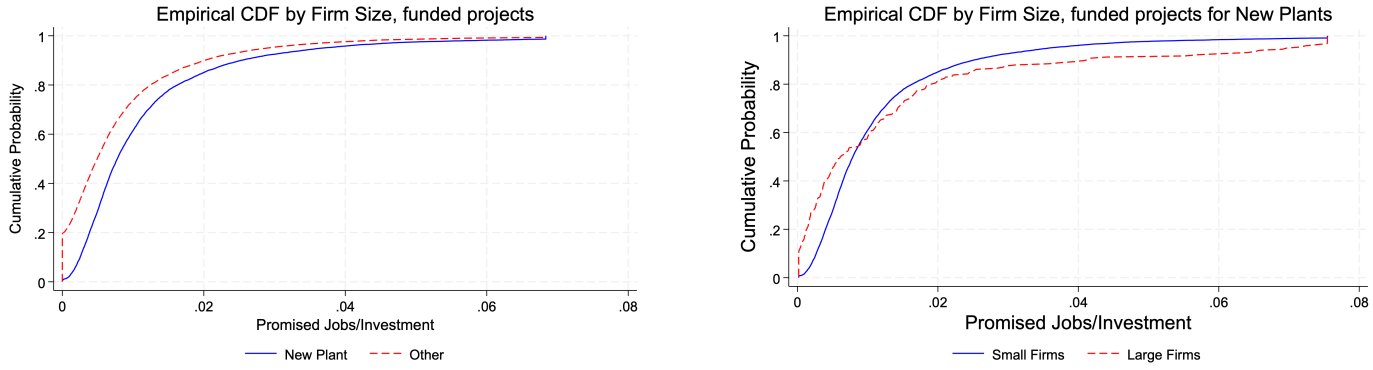
Notes: Summary of approved IDA projects by investment type. New plants (costruzione) account for roughly 50% of total grant spending with a mean award of approximately €617,000, while expansions and other project types account for the remainder. The high per-project grant for new plants reflects the program's deliberate design to subsidize firm creation and greenfield investment.

Table B.5: New-plant applicants almost universally pledge job creation and own capital investment.

	Total Projects	Share L>0	Share K>0	Share L>0,K>0
Other	15684	82.91	96.97	81.21
New Plant	13902	99.34	99.12	98.5

Notes: For winning projects by firm size and type, the table reports the fraction committing to employment creation (L criterion) and own-capital investment (K criterion). New-plant projects show near-universal compliance with both criteria, reflecting the program's requirement to link grants to verifiable investment and employment commitments.

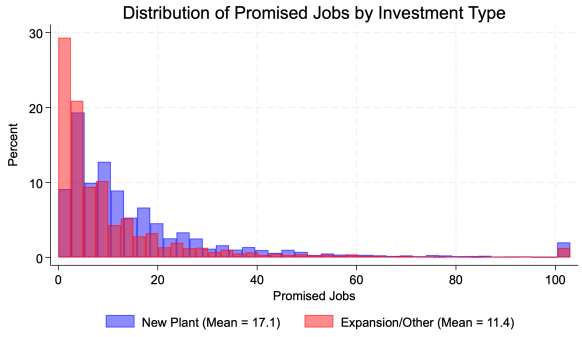
Fig. B.5: New plants promise more jobs per euro invested, especially among small firms.



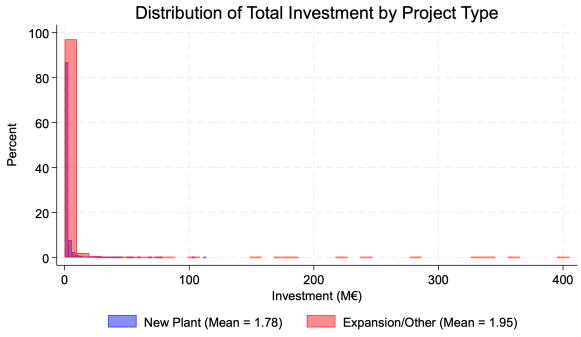
(i) **(ii)**

Notes: Empirical CDFs of promised jobs per unit of investment (left panel: by project type; right panel: by firm size for new plants only). New-plant projects consistently dominate expansions in employment intensity across the distribution. Among new plants, small firms promise more jobs per euro than medium or large firms, consistent with the program's intent to channel employment-intensive investment to lagging regions.

Fig. B.6: New-plant projects target more jobs but similar investment amounts as expansions.



(i)



(ii)

Notes: Distribution of promised job counts (left) and total investment amounts (right) for winning projects, by project type. New plants average 20.6 promised jobs versus 12.7 for expansions-a 63% gap-while the investment distributions are more similar, confirming that the employment intensity advantage of new plants is real and not simply an artifact of project size.

Table B.6: Small firms dominate new-plant applications; larger firms favor expansions.

	Total Projects	Share New Plants (%)	Total Win Projects	Share New Plants Win (%)
Large	4467	19.5	2255	18.4
Medium	7090	21.14	3849	20.34
Small	57820	54.38	23482	54.1

Notes: Number and share of applications and funded projects by firm size and project type. Small firms account for the vast majority of new-plant projects, while medium and large firms are more likely to propose expansions. The manufacturing sector receives the largest share of funds, and the geographic distribution is concentrated in the South.

B.3 Building new plants: who and where is subsidized?

Given the significant share of funding allocated to new plants, we focus on applications approved for new plant construction. 16,621 firms applied for funding to open a new manufacturing plant; half received it. Table B.7 shows that the majority of new-plant projects are by small firms (9,237 projects) versus medium (508) and large (222). Small firms building new plants pledge to generate employment in 99% of projects. Figure B.7 highlights that small firms consistently promise more jobs per unit of investment.

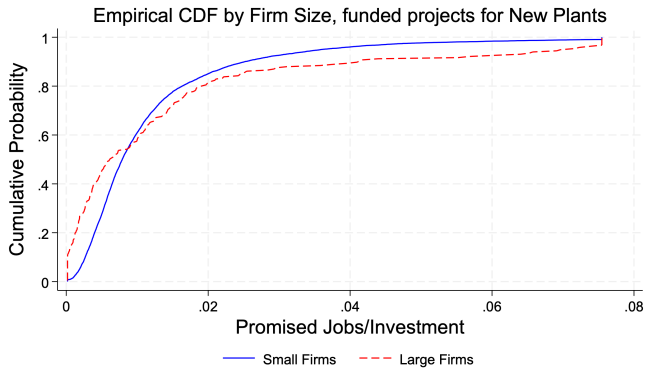
A total of 47,200 firms submitted applications (average 1.33 projects per firm). The vast majority (45,651 firms) applied in only one municipality. Figure B.8 shows that regions with higher pre-policy unemployment received larger per-plant grants and more plants overall. Figure B.9 maps the geographic distribution of funded new manufacturing plants, showing a clear concentration in the South.

Table B.7: Small firms account for nine in ten new-plant projects and pledge jobs in virtually every case.

Firm Size	Total Projects	Share Committing L Creation (%)	Share Manufacturing (%)
Large	222	92	56
Medium	508	98	79
Small	9237	99	77

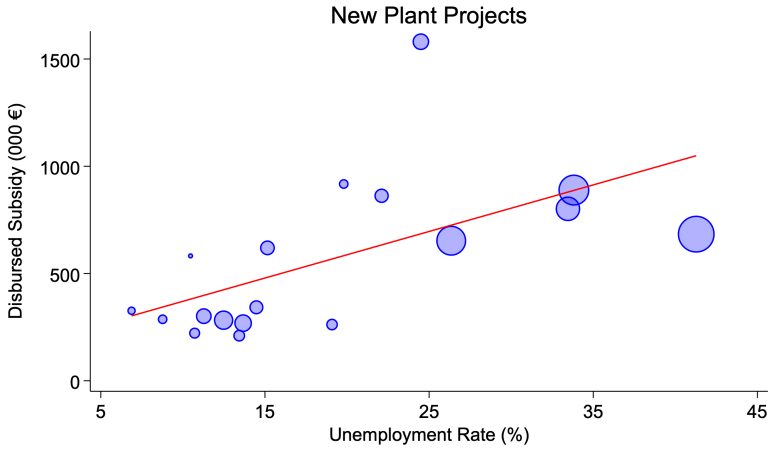
Notes: Breakdown of funded new-plant projects by firm size. Of 9,967 new-plant projects, small firms account for 9,237 (93%); medium and large firms together account for fewer than 800. Small firms commit to creating employment in 99% of their new-plant projects.

Fig. B.7: Small firms building new plants promise more jobs per euro than larger firms.



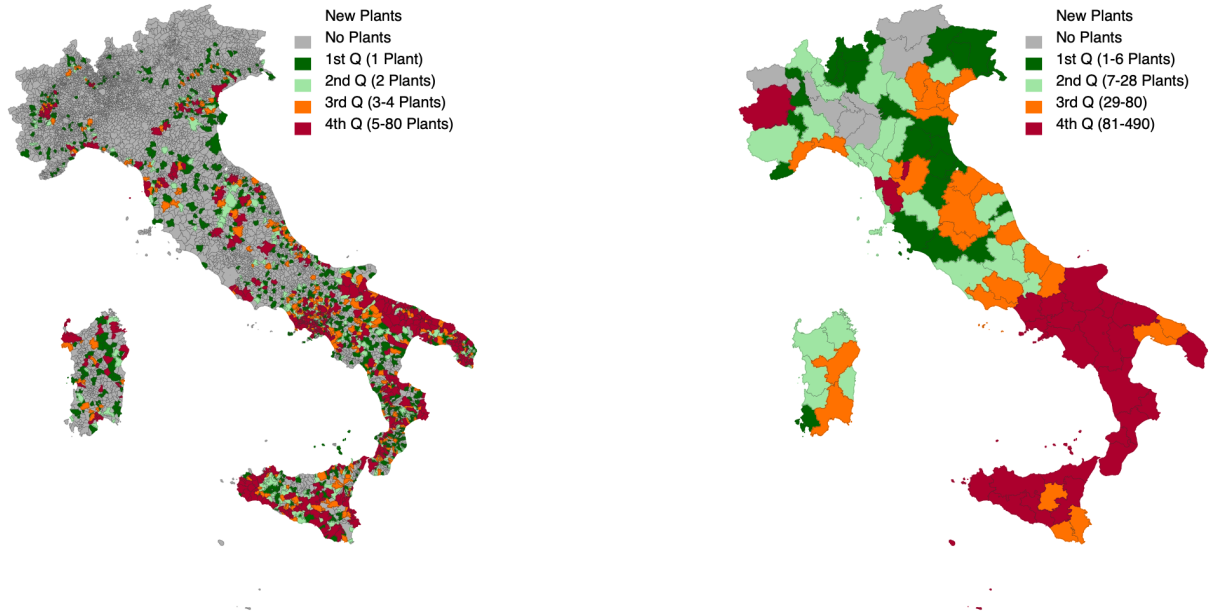
Notes: Empirical CDF of promised jobs per unit of investment for winning new-plant projects, by firm size. The distribution for small firms first-order stochastically dominates those for medium and large firms, confirming that the program's small-firm orientation is associated with greater employment intensity of the subsidized investments.

Fig. B.8: Higher-unemployment regions attract more new-plant projects and receive larger grants.



Notes: Binned scatter plots of mean subsidy amount (left) and number of funded new-plant projects (right) against the regional pre-policy unemployment rate. Both series slope upward, indicating that the program's spatial allocation broadly tracks the geographic distribution of labor market distress-though this is partly mechanical given that eligible areas were defined by EC Objective Area classifications based on economic indicators.

Fig. B.9: IDA-funded new plants are heavily concentrated in Southern Italy.



(i) Municipality level **(ii) Province level**

Notes: Geographic distribution of approved new manufacturing plant projects at the municipality level (left) and province level (right). The Southern regions (Campania, Calabria, Sicily, Sardinia, Puglia, Basilicata) account for the overwhelming majority of funded plants, reflecting both the program's explicit targeting of EC Objective 1 areas and the higher pre-policy unemployment rates in the Mezzogiorno.

B.4 Building new plants: What Attracts Firms?

The following figures investigate the relationship between new plant applications (normalized by population) and local labor market characteristics, controlling for region fixed effects.

Figure B.10 shows that firms are more likely to open new plants in areas with a high share of manufacturing employment and a strong concentration of manufacturing firms, but less inclined to invest in large labor markets. Figure B.11 highlights that more plant applications are submitted in markets with *lower* unemployment rates, greater underutilized housing, and fewer signs of economic hardship-a pattern that runs counter to the program's stated targeting goals.

Fig. B.10: Manufacturing Census variables and new plant applications

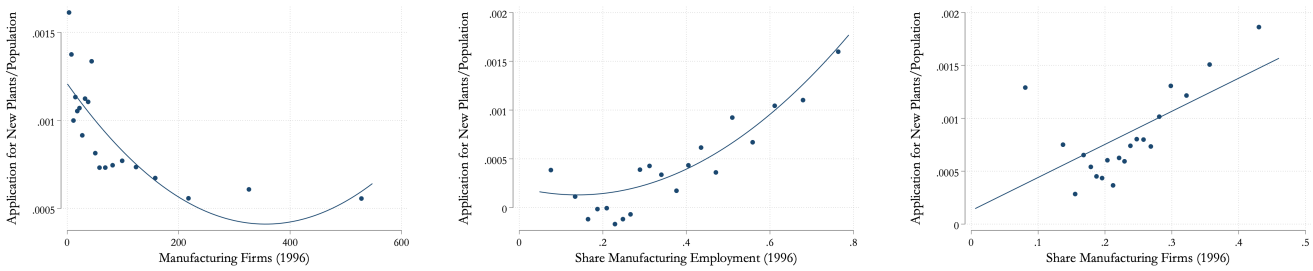


Fig. B.11: Market characteristics and number of applications for new plants

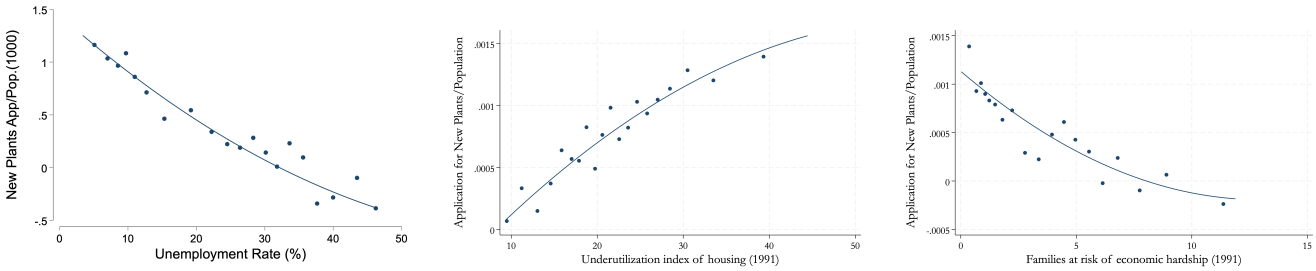
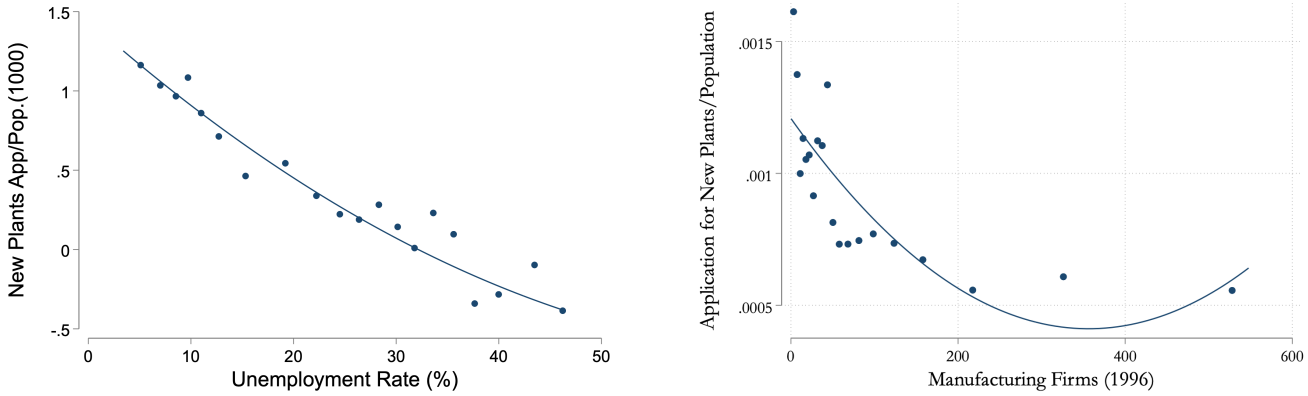


Fig. B.12: New plant applications, local unemployment rate, and local manufacturing firm density (1996)



C Additional data discussion

C.1 Market definition

To assess whether province × 2-digit sector is a meaningful definition of the labor market, we use matched employer-employee records to study voluntary job-to-job transitions. Specifically, we focus on male workers in permanent and temporary contracts whose jobs start between 2005 and 2008, and we construct transition matrices across provinces and 2-digit NACE sectors, optionally also considering 1-digit occupations. The idea is that a plausible market definition should capture the set of firms that workers realistically view as outside options. Reassuringly, we find that 27% of voluntary transitions occur within the same province × 2-digit sector cell, with an even higher share for workers on permanent contracts. This suggests that labor mobility is substantially concentrated within these cells, consistent with the view that province × 2-digit sector captures an economically relevant market boundary. This pattern is also in line with the evidence in Appendix Table A1 of Arnold (2019), which documents substantial within-industry and within-occupation mobility in the CPS. Taken together, these findings support our use of province × 2-digit sector pairs as the baseline market definition.

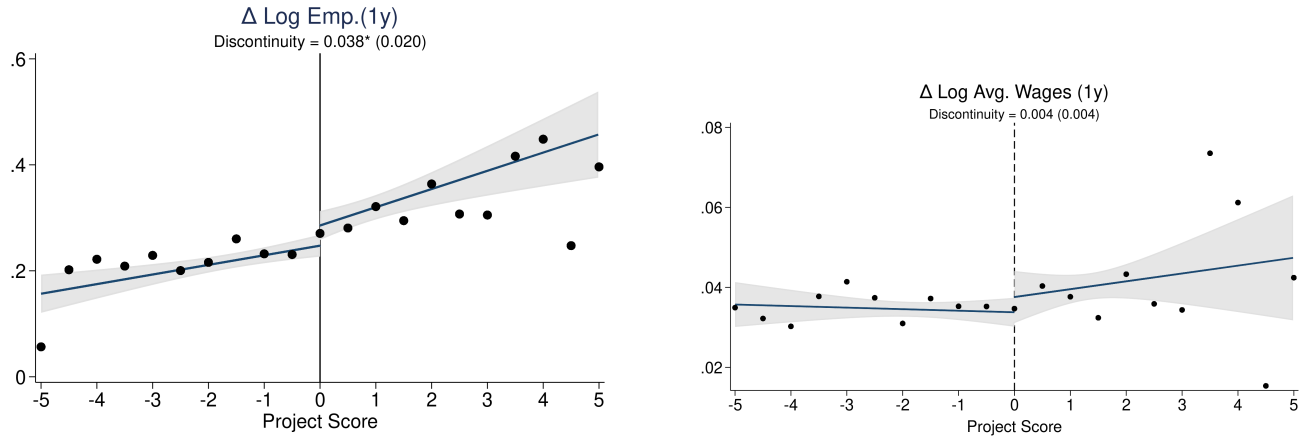
	(1)	(2)	(3)	(4)	(5)
VARIABLES	N	mean	sd	min	max
Same 2 Digit Industry-Province	2.038e+06	0.270	0.444	0	1
Same 2 Digit Industry-Province, Permanent	1.146e+06	0.319	0.466	0	1

D Additional RDD Figures and Tables

D.1 Employment and Wages: All Horizons

The figures below show RD plots at $t+1$ and $t+3$ (the main text reports $t+2$). The employment effect grows from approximately 0.03 at $t+1$ to 0.13-0.14 at $t+3$; the wage response is consistent across horizons.

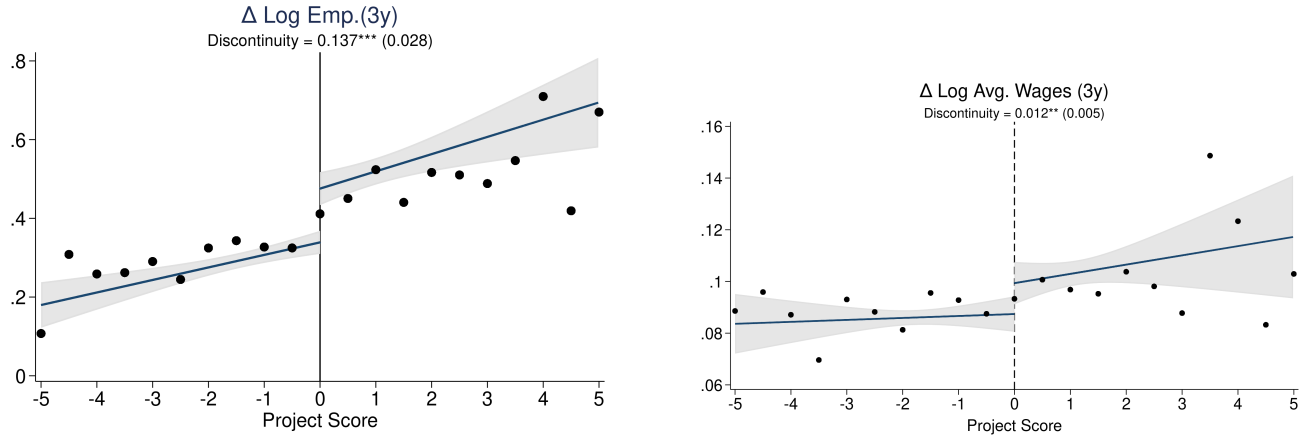
Fig. D.13: Employment and wage effects emerge already at $t+1$.



(i) Employment ($t+1$) **(ii) Wages ($t+1$)**

Notes: RD plots of the change in log employment (left) and log wages (right) one year after the application. Effects are visible but smaller than at $t+2$ (Figure 2 in the main text), consistent with the phased nature of capital investment in new plant construction.

Fig. D.14: Effects continue to grow through $t+3$ as investment matures.



(i) Employment ($t+3$) **(ii) Wages ($t+3$)**

Notes: RD plots of the change in log employment (left) and log wages (right) three years after the application. The employment discontinuity has grown to approximately 0.13-0.14 log points-roughly double the $t+2$ estimate-confirming that the program's effects strengthen over time as capital-intensive projects reach full capacity.

D.2 RDD Coefficient Tables: Employment and Wages

Tables D.8-D.13 report the Win coefficient estimates at horizons $t+1$, $t+2$, and $t+3$, for the pooled sample and separately by skill group.

Table D.8: Employment effects are small at $t+1$ and grow sharply thereafter.

	(1)	(2)	(3)
VARIABLES	Δ Emp. (1)	Δ Emp. (2)	Δ Emp. (3)
Win	0.029	0.067***	0.126***
	(0.020)	(0.024)	(0.030)
Observations	22,473	22,140	21,611

Notes: RDD coefficient estimates for the effect of subsidy receipt on log employment change, pooling all workers. Columns correspond to post-application horizons $t+1$, $t+2$, and $t+3$. The Win coefficient grows from near-zero at $t+1$ to 0.067^{***} at $t+2$ and 0.126^{***} at $t+3$. Standard errors are clustered at the ranking-group level.

Table D.9: Average wages rise significantly from $t+2$ onward.

	(1)	(2)	(3)
VARIABLES	Δ Wages (1)	Δ Wages (2)	Δ Wages (3)
Win	0.006	0.016***	0.015***
	(0.004)	(0.005)	(0.005)
Observations	22,473	22,140	21,611

Notes: RDD coefficient estimates for the effect of subsidy receipt on log average wage change, pooling all workers. The Win coefficient is near zero at $t+1$, turns positive and significant at $t+2$, and remains significant at $t+3$. Standard errors clustered at the ranking-group level.

Table D.10: White-collar employment expands substantially at winning firms.

	(1)	(2)	(3)
VARIABLES	Δ Emp. (1)	Δ Emp. (2)	Δ Emp. (3)
Win	0.044**	0.083***	0.126***
	(0.022)	(0.025)	(0.031)
Observations	17,428	16,963	16,427

Notes: RDD coefficient estimates for log white-collar employment change. The effect is positive and growing across horizons, with the $t+3$ estimate notably large. This is consistent with capital-skill complementarity: new plant investment raises the marginal product of white-collar workers disproportionately.

Table D.11: White-collar wages rise sharply, driving the aggregate wage effect.

	(1)	(2)	(3)
VARIABLES	Δ Wages (1)	Δ Wages (2)	Δ Wages (3)
Win	0.014***	0.019**	0.022**
	(0.005)	(0.007)	(0.009)
Observations	17,428	16,963	16,427

Notes: RDD coefficient estimates for log white-collar wage changes. Wage gains for white-collar workers are larger in magnitude and more precisely estimated than for blue-collar workers (Table D.13), consistent with greater scope for firm-level top-ups above the CBA floor in this occupational group.

Table D.12: Blue-collar employment also expands at subsidized firms.

	(1)	(2)	(3)
VARIABLES	Δ Emp. (1)	Δ Emp. (2)	Δ Emp. (3)
Win	0.026	0.049*	0.103***
	(0.021)	(0.025)	(0.031)
Observations	20,425	19,998	19,465

Notes: RDD coefficient estimates for log blue-collar employment change. Effects are positive and significant at $t+2$ and $t+3$, confirming that the employment expansion extends across the occupational distribution rather than being concentrated among skilled workers.

Table D.13: Blue-collar wage gains are positive but smaller than for white-collar workers.

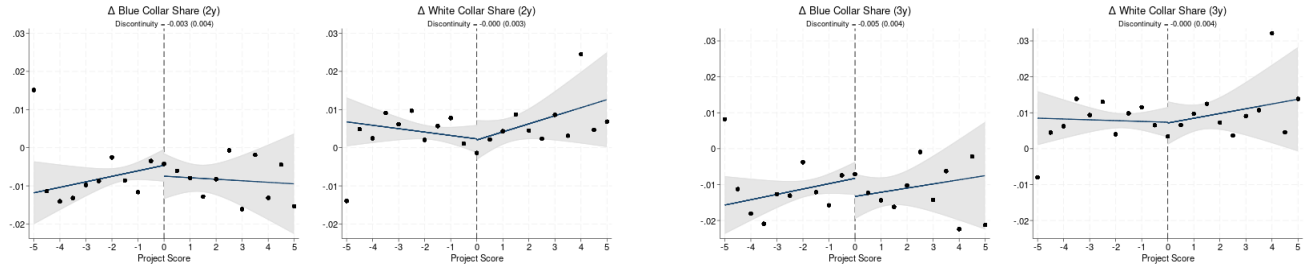
	(1)	(2)	(3)
VARIABLES	Δ Wages (1)	Δ Wages (2)	Δ Wages (3)
Win	0.003	0.006	0.007
	(0.005)	(0.006)	(0.006)
Observations	20,425	19,998	19,465

Notes: RDD coefficient estimates for log blue-collar wage changes. Effects are smaller in magnitude than for white-collar workers (Table D.11), consistent with nationally negotiated CBA floors binding more tightly for production workers and leaving less room for discretionary firm-level supplements.

D.3 Skill Composition

Figure D.15 shows the change in workforce skill composition at $t+1$ and $t+2$, which does not change significantly at treated firms.

Fig. D.15: Subsidized firms do not upgrade their occupational mix.

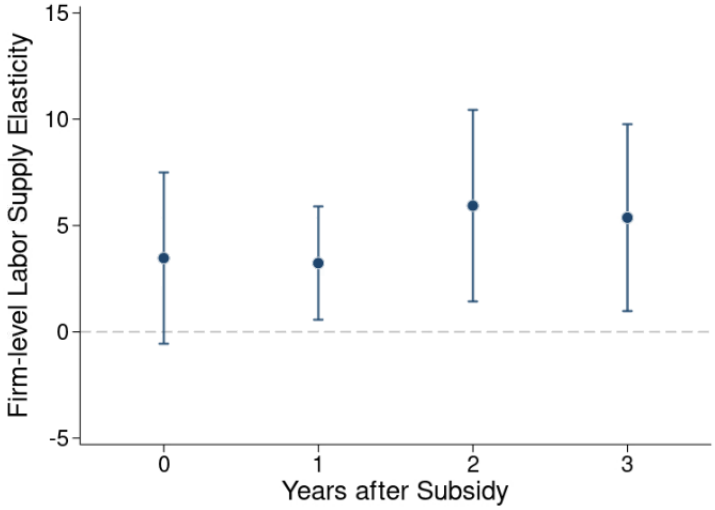


(i) Skill composition $(t+1)$ **(ii) Skill composition** $(t+2)$

Notes: Event-study estimates for the white-collar employment share at $t+1$ (left) and $t+2$ (right) at treated vs. control firms. Neither panel shows a significant post-application shift in skill composition, confirming that the policy expands the workforce at scale rather than selectively recruiting higher-skilled workers. This stability validates that the wage gains for stayers documented in the main text reflect genuine within-worker pay increases, not compositional selection.

D.4 Labor Supply Elasticity

Fig. D.16: The firm-level labor supply elasticity is stable over the post-application horizon.



Notes: Point estimates and 95% confidence intervals for the implied firm-level labor supply elasticity $\hat{\eta}_f = 1/(1/\hat{\eta}_f)$ at each post-application horizon. The elasticity is remarkably stable across the short and long run ($\hat{\eta}_f^{SR} \approx \hat{\eta}_f^{LR} \approx 6$), ruling out interpretations based on temporary demand shocks or short-run adjustment costs. A firm-level labor supply elasticity of 6 implies that workers receive approximately 86% of their marginal revenue product-substantially below the competitive benchmark of 100%.

Tables D.14 and D.15 report the 2SLS inverse labor supply elasticity estimated separately for white-collar and blue-collar workers. Both groups show positive, statistically significant estimates broadly consistent with the pooled result in the main text.

Table D.14: Inverse labor supply elasticity: white-collar workers.

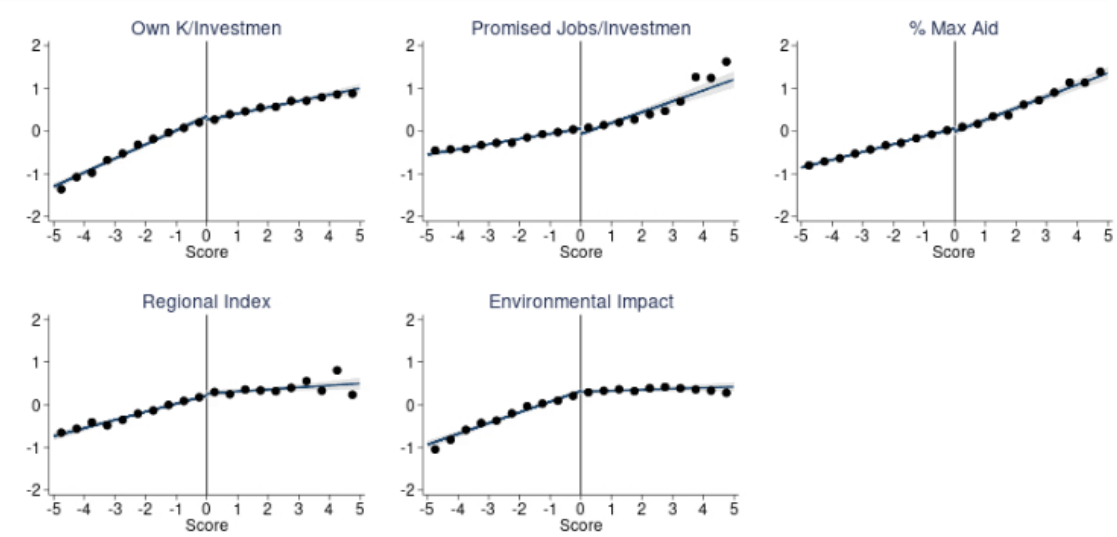
	(1)	(2)	(3)	(4)
VARIABLES	Δ log Wage (t+1)	Δ log Wage (t+2)	Δ log Wage (t+3)	Δ log Wage (t+4)
Δ Log White Collar Emp.(1y)	0.1237**			
	(0.0544)			
Δ Log White Collar Emp.(2y)		0.0986**		
		(0.0489)		
Δ Log White Collar Emp.(3y)			0.1207*	
			(0.0620)	
Δ Log White Collar Emp.(4y)				0.0748*
				(0.0446)
Observations	16,465	16,035	15,526	15,084
First-stage F-stat	9.54	13.44	10.66	15.67
Overid p-val	0.131	0.167	0.420	0.777

Table D.15: Inverse labor supply elasticity: blue-collar workers.

VARIABLES	(1) Δ log Wage (t+1)	(2) Δ log Wage (t+2)	(3) Δ log Wage (t+3)	(4) Δ log Wage (t+4)
Δ Log Blue Collar Emp.(1y)	0.0743			
	(0.0658)			
Δ Log Blue Collar Emp.(2y)		0.0955		
		(0.0732)		
Δ Log Blue Collar Emp.(3y)			0.0902*	
			(0.0526)	
Δ Log Blue Collar Emp.(4y)				0.0671
				(0.0452)
Observations	19,290	18,877	18,391	17,879
First-stage F-stat	4.89	5.07	8.85	14.25
Overid p-val	0.733	0.804	0.293	0.042

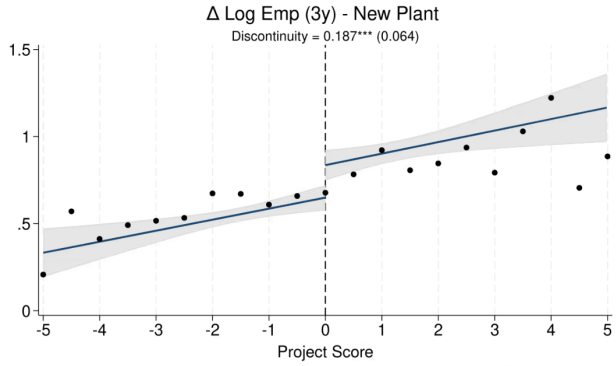
D.5 Balance Checks

Fig. D.17: Balance of individual score components at the funding cutoff.

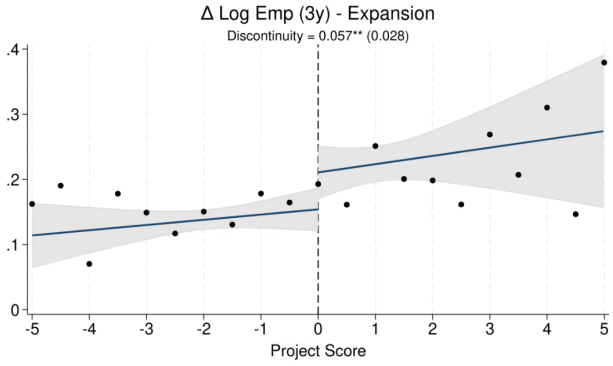


D.6 Heterogeneity: New Plants vs. Expansion Projects

Fig. D.18: Employment effects are driven by new-plant creation.



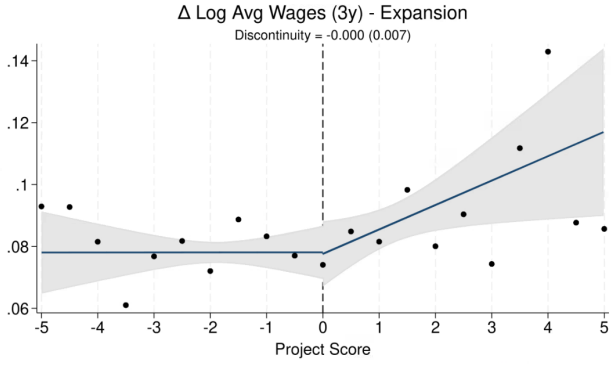
(i) New plant



(ii) Expansion



(iii) New plant



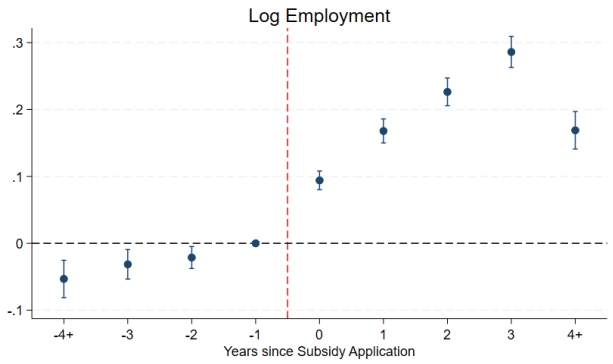
(iv) Expansion

Notes: RD plots of the change in log employment and log wages at $t+2$, estimated separately for new-plant projects (left) and plant-expansion projects (right). The employment and wage response is concentrated entirely among new-plant projects, with no detectable discontinuity for expansions.

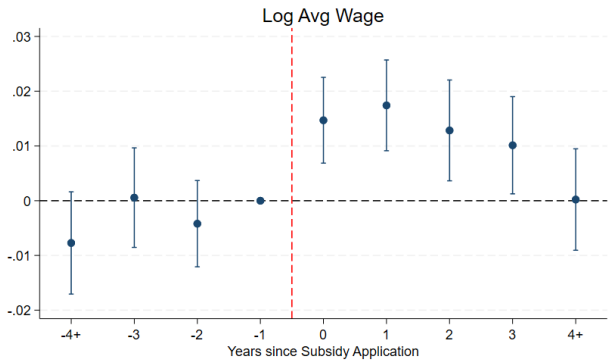
D.7 DiD Validation: Firm-Level Employment and Wages

The difference-in-differences event-study design used in Section 4 compares near-cutoff treated and control firms over event time. As a validation, Figure D.19 plots firm-level DiD estimates of the policy effects on employment and average wages. These estimates are closely aligned with the RDD estimates reported in Section 3, confirming that the DiD design recovers the same causal effects as the regression discontinuity.

Fig. D.19: Firm-level DiD event-study estimates. The DiD design recovers effects closely aligned with the RDD estimates in Section 3, validating the near-cutoff comparison as a credible control group.



(i) Employment (firm-level DiD)



(ii) Average wage (firm-level DiD)

E Additional Worker-Level Tables and Figures

E.1 Incumbent Worker Sample: Descriptive Statistics

Table E.16 reports descriptive statistics for the incumbent worker sample-workers employed at IDA applicant firms at the application year t_f^a-separately for treated (above-cutoff) and control (below-cutoff) firms. The two groups are balanced on key pre-application characteristics: earnings, weekly wages, weeks worked, occupational distribution, age, gender, and tenure are similar across the threshold. This balance is consistent with the near-random assignment of subsidies among near-cutoff applicants exploited in the DiD design.

Table E.16: Treated and control firms are balanced on pre-application worker characteristics.

	(1)	(2)
	Treated	Control
Earnings	21196.467	18602.582
Weekly Wage	451.200	412.957
Log Weekly Wage	6.027	5.937
N. Weeks	44.726	42.186
Log Weekly Wage	3.694	3.598
White Collar	0.243	0.244
Blue Collar	0.699	0.681
Age	36.454	35.904
Female	0.223	0.247
Tenure	3.604	3.778
Observations	153943	162506

Notes: Descriptive statistics for the incumbent worker sample (workers employed at IDA applicant firms in the application year t_f^a), separately for workers at treated (above-cutoff) and control (below-cutoff) firms. The two groups are closely similar in earnings, wages, weeks worked, occupational distribution, age, gender, and tenure at baseline. This balance across the funding threshold supports the validity of the near-cutoff DiD design.

E.2 Cross-section estimates of retention, accounting for workers' characteristics

Worker-firm panel event study (baseline specification):

$$\text{Retain}_{it} = \alpha_{f(i)} + \tau_t + \beta \text{Treated}_{f(i)} + X_{it}'\gamma + \varepsilon_{it}$$

Table E.17

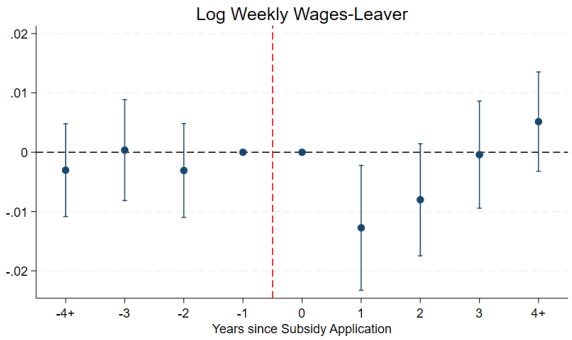
	(1)	(2)	(3)
VARIABLES	Retained 1 y	Retained 2 y	Retained 3 y
Treated	0.026***	0.052***	0.075***
	(0.006)	(0.008)	(0.009)
Observations	269,036	265,321	260,475
R-squared	0.209	0.254	0.285
Mean Dep Var (Control)	0.791	0.670	0.579

Table E.18

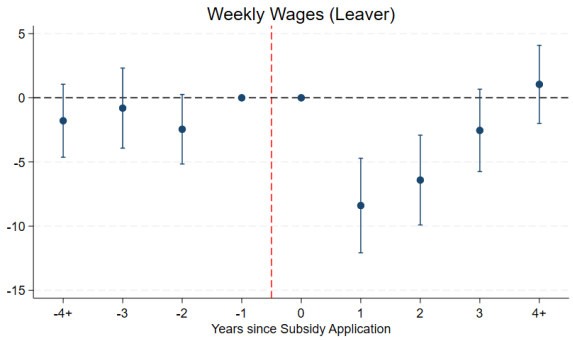
	(1)	(2)	(3)
VARIABLES	Retained 1 y	Retained 2 y	Retained 3 y
Treated	0.026***	0.052***	0.075***
	(0.006)	(0.008)	(0.009)
Observations	269,036	265,321	260,475
R-squared	0.209	0.254	0.285
Mean Dep Var (Control)	0.791	0.670	0.579

E.3 Leavers Wage Effects

Fig. E.20: Workers who leave treated firms receive no wage premium.



(i) Leavers: log wages

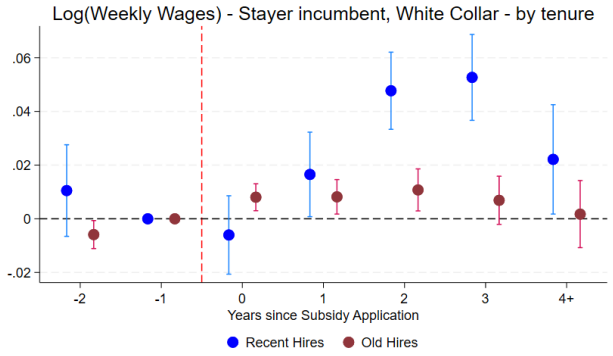


(ii) Leavers: employment days

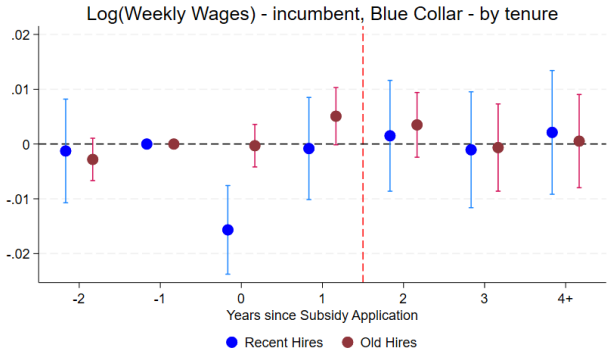
Notes: Worker-level event-study estimates for the leaver subsample-incumbents who depart after the application year.

E.4 Stayer Wage Effects: Tenure Heterogeneity

Fig. E.21: Wage gains for stayers do not vary monotonically with tenure-ruling out owner-pay explanations and equity-financed backloaded compensation.



(i) White-collar stayers: by tenure

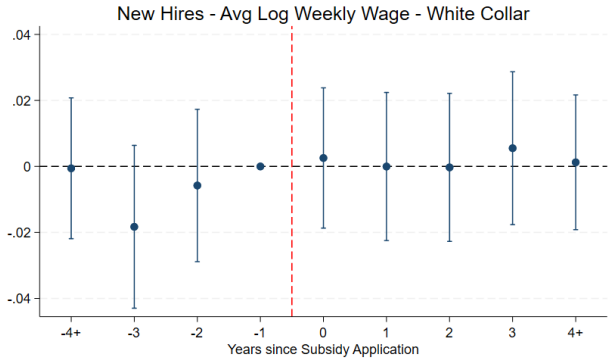


(ii) Blue-collar stayers: by tenure

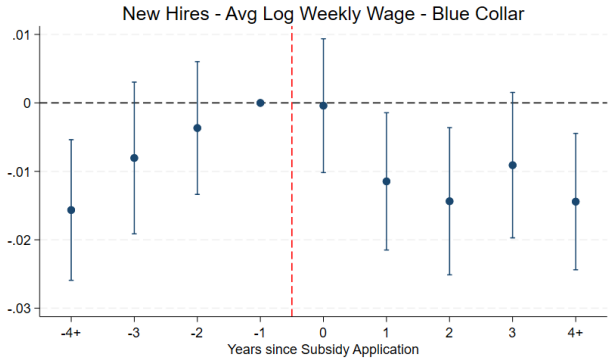
Notes: Worker-level event-study estimates for stayer log wages, stratified by tenure quartile, separately for white-collar (left) and blue-collar (right) workers. The absence of a monotonic tenure gradient is important: if the observed wage gains were driven by equity-based remuneration for firm owners or long-tenured managers, we would expect the effect to be concentrated in the highest-tenure group. The flat pattern across tenure groups is inconsistent with this interpretation.

E.5 New Hire Wages: Unresidualized

Fig. E.22: Unresidualized entry wages confirm no wage premium for new hires.



(i) White-collar new hires

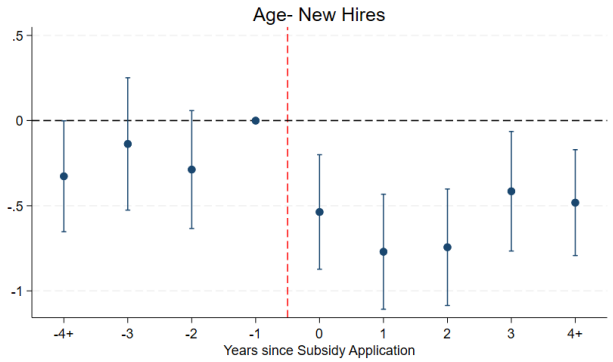


(ii) Blue-collar new hires

Notes: Event-study estimates for raw (non-residualized) average log wages of new hires at treated vs. control firms, separately for white-collar (left) and blue-collar (right) workers. Results mirror the residualized estimates in the main text (Figure 13): entry wages are flat at the CBA floor regardless of occupation, confirming the result is not an artifact of the residualization procedure.

E.6 Composition of New Hires

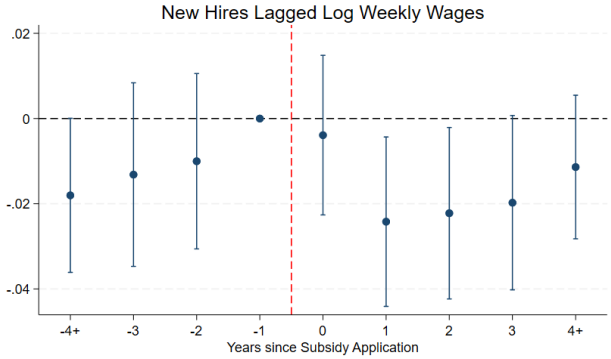
Fig. E.23: The observable characteristics of new hires do not change at subsidized firms.



(i) Age of new hires



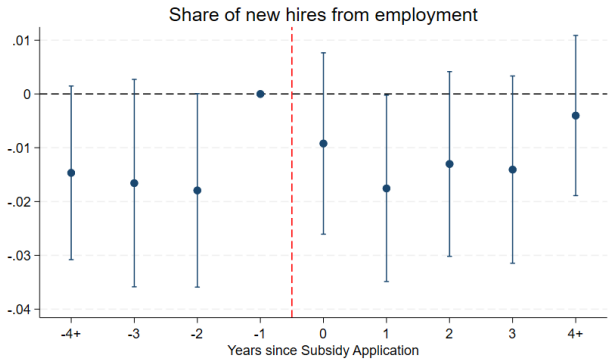
(ii) Female share of new hires



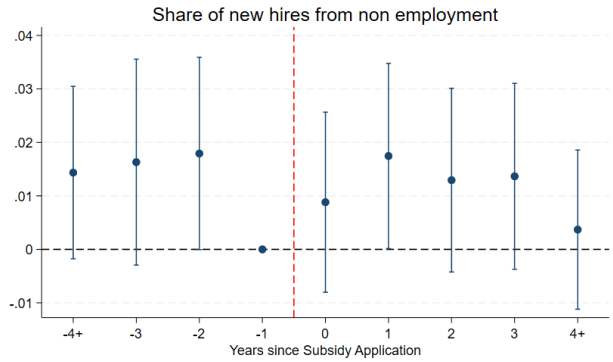
(iii) Previous log wage of new hires

Notes: Event-study estimates for three composition measures among new hires: average age (top left), female share (top right), and average pre-hire log wage (bottom left). None of the three changes significantly at treated relative to control firms, confirming that the flat entry wage result is not driven by firms hiring observably different (lower-quality) workers after the subsidy.

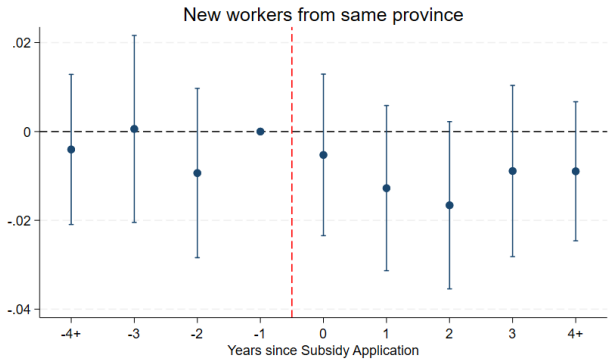
Fig. E.24: Treated firms recruit from the same sources as control firms.



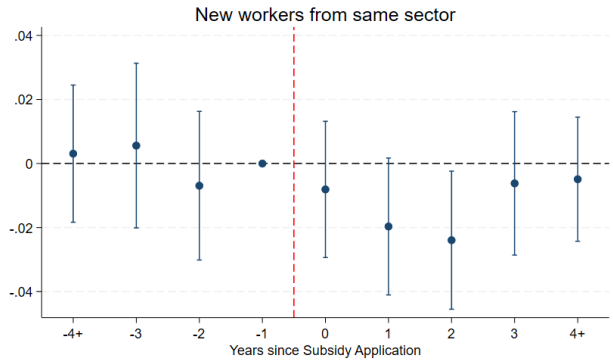
(i) Job-to-job hires rate



(ii) Non-employment to employment rate



(iii) Share from same province



(iv) Share from same industry

Notes: Event-study estimates for four origin measures among new hires: job-to-job hire rate (top left), non-employment entrant rate (top right), share from the same province (bottom left), and share from the same industry (bottom right). None changes significantly, ruling out the possibility that subsidized firms respond to the capital grant by shifting toward cheaper or more geographically mobile workers.

E.7 Role of collective bargaining agreements

Table E.19: Incumbents earn well above the CBA floor-leaving room for discretionary top-ups.

	(1)						
	mean	p25	p50	p75	min	max	count
Incumbent workers- Share of earnings due to CBA	**0.784**	0.605	0.752	0.892	0.401	1.472	196667
Incumbent workers - Share of earnings due to CBA - White Collar	**0.744**	0.532	0.687	0.869	0.331	1.528	51089
Incumbent workers - Share of earnings due to CBA - Blue Collar	**0.797**	0.633	0.767	0.896	0.449	1.439	145578
Incumbent workers - Log(Earnings) - Log(CBA Earnings)	0.281	0.115	0.285	0.503	-7.847	5.866	196667
Incumbent workers - Log(Earnings) - Log(CBA Earnings) - Blue Collar	0.251	0.109	0.266	0.457	-7.847	5.866	145578
Incumbent workers - Log(Earnings) - Log(CBA Earnings) - White Collar	0.367	0.140	0.375	0.631	-7.710	3.684	51089
Incumbent workers- Top-up as a share of CBA earnings	0.411	0.121	0.330	0.654	-0.322	1.493	196730
Incumbent workers - Top-up as a share of CBA earnings - Blue Collar	0.361	0.115	0.304	0.580	-0.307	1.228	145631
Incumbent workers - Top-up as a share of CBA earnings- White Collar	0.560	0.150	0.455	0.879	-0.346	2.021	51099

Notes: For workers employed at IDA applicant firms at the application year, the table reports the distribution of the CBA base-wage share in total earnings. On average, the base wage accounts for 78% of incumbents' earnings (median: 75%), meaning 22-25% is made up of firm-specific discretionary supplements above the nationally negotiated floor. This scope for top-ups is the institutional channel through which capital subsidies translate into wage gains for stayers.

Table E.20: New hires earn at the CBA floor, with no room for rent sharing.

	(1)						
	mean	p25	p50	p75	min	max	count
New Hires - Share of earnings due to CBA	**1.054**	0.713	0.887	1.178	0.338	2.974	612320
New Hires - Share of earnings due to CBA - White Collar	**1.003**	0.630	0.888	1.264	0.241	2.354	165926
New Hires - Share of earnings due to CBA - Blue Collar	**1.094**	0.732	0.886	1.148	0.395	3.531	446394
New Hires - Log(Earnings) - Log(CBA Earnings)	0.070	-0.164	0.120	0.338	-9.873	4.951	612299
New Hires - Log(Earnings) - Log(CBA Earnings) - Blue Collar	0.039	-0.138	0.121	0.312	-9.733	4.222	446376
New Hires - Log(Earnings) - Log(CBA Earnings) - White Collar	0.153	-0.235	0.119	0.462	-9.873	4.951	165923
New Hires - Top-up as a share of CBA earnings	0.218	-0.153	0.127	0.401	-0.674	1.954	613230
New Hires - Top-up as a share of CBA earnings - Blue Collar	0.167	-0.131	0.128	0.366	-0.726	1.525	447091
New Hires - Top-up as a share of CBA earnings- White Collar	0.375	-0.211	0.125	0.586	-0.579	3.146	166139

Notes: For workers hired after the IDA application year, the CBA base wage accounts for 100% of earnings on average (median: 89%), implying that entry wages are effectively pinned at the nationally negotiated minimum. The contrast with Table E.19 is stark: incumbents enjoy substantial firm-level supplements above the floor, while new hires do not-the precise institutional mechanism that drives the distributional asymmetry in rent sharing.

F Market-level analysis

This appendix describes in detail how we construct the counterfactual employment needed to build the recentered market-level instrument $\tilde{Z}_m^{\text{Scale}}$, validates the promised-jobs measure used in the instrument construction, and

F.1 Validation of Promised Jobs Using RDD

We begin by validating the use of firms' promised job creation as an input for the counterfactual employment measures used in the permutation exercises. The key concern is whether the number of jobs firms commit to create at the application stage is informative about their realized post-subsidy employment trajectory once funding is awarded. To assess this, we compare the counterfactual employment level implied by baseline employment plus promised jobs to actual observed employment at winning and non-winning firms. We do so at two post-baseline horizons: three years after baseline, using $\Delta \log(E^{\text{Count}}) - \log(E^{\text{Actual},,t=4})$, and four years after baseline, using $\Delta \log(E^{\text{Count}}) - \log(E^{\text{Actual},,t=5})$. If promised jobs are a meaningful proxy for realized employment expansion, then among subsidized firms the discrepancy between promised and actual employment should be smaller than among firms that applied but did not receive funding.

We test this prediction using the same regression discontinuity design employed in the main analysis. Figure F.26 shows a clear downward discontinuity at the funding cutoff at both horizons. This implies that winning firms exhibit a significantly smaller gap between promised and realized employment than losing firms, exactly as one would expect if promised jobs contain genuine information about the employment response induced by the subsidy. Put differently, the firms that receive the subsidy appear substantially closer to delivering the employment expansion embedded in their application, which supports our use of promised jobs to construct counterfactual post-treatment employment in the simulation exercise.

As an additional check on the validity of the design, Figure F.25 shows that baseline log employment does not display any significant discontinuity at the funding threshold, consistent with smooth sorting around the cutoff. Figure D.17 further plots the individual components of the project score against the standardized score, providing a visual assessment of balance in the underlying determinants of ranking around the threshold.

We complement this validation with two further pieces of evidence. First, Figure F.27 shows that promised jobs themselves vary smoothly at the cutoff, with no economically meaningful discontinuity in the number of jobs firms pledge to create. This is reassuring because it suggests that firms just above and just below the threshold are comparable in their ex ante employment commitments. Second, Figure F.28 shows a discontinuity in the log amount of requested subsidy per promised job, indicating that firms around the cutoff differ in the amount of funding associated with each planned position even when promised job counts themselves remain broadly similar. Taken together, these patterns strengthen the interpretation that the discontinuity in realized employment outcomes is driven by subsidy receipt rather than by a discontinuous change in firms' underlying promises at the threshold.

Overall, these validation exercises provide support for the simulation strategy used in the paper. Promised jobs are not simply cheap talk: conditional on being near the cutoff, they are informative about subsequent employment performance, and they become more tightly aligned with realized outcomes precisely for firms that obtain the subsidy. This makes them a reasonable and empirically grounded ingredient for constructing counterfactual employment paths in the permutation-based analysis.

Fig. F.25: Balance check: baseline log employment at the funding cutoff. No significant discontinuity.

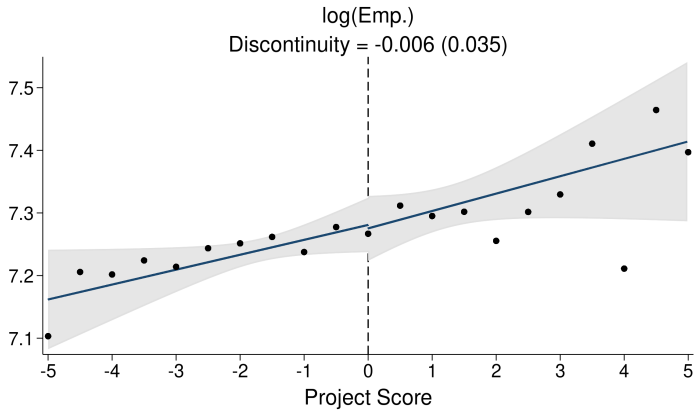


Fig. F.26: RD validation: promised vs. actual employment

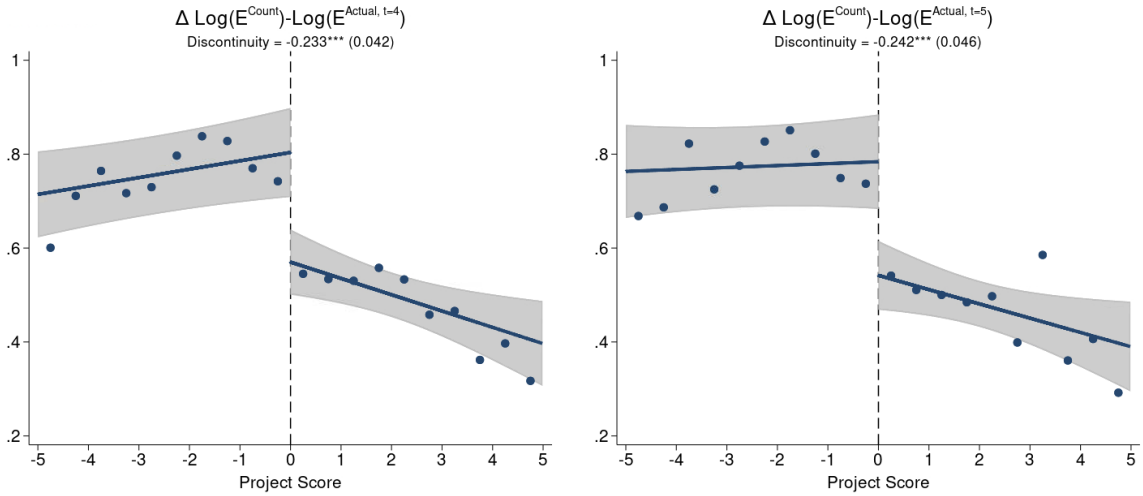


Fig. F.27: Promised jobs: RD discontinuity

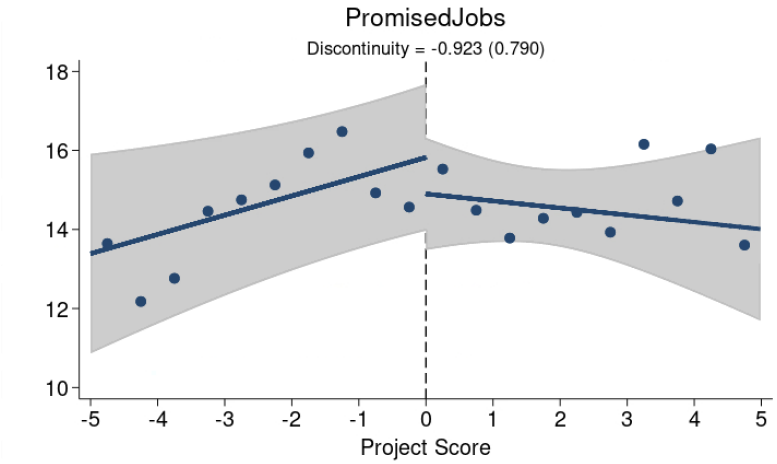


Fig. F.28: Promised jobs and subsidy amount: RD



F.2 Counterfactual Employment for Firms

We predict counterfactual 2001 employment for all firms using a Poisson Pseudo-Maximum Likelihood (PPML) model estimated on pre-policy data (1995), using only predetermined firm-level covariates. For applicant firms, we add simulated promised jobs on top of the PPML prediction:

$$\widetilde{E}^{(s)}_{i,2001} = \begin{cases} \hat{\mu}_i + \mathrm{z}_i^{(s)} \cdot \mathrm{promjobs}_i, & i \in \mathcal{A}, \\ \hat{\mu}_i, & i \notin \mathcal{A}, \end{cases}$$

where $\hat{\mu}_i = \mathbb{E}[Y_i \mid X_{i,1995}, \mu_{m(i)}]$ is the PPML predicted 2001 employment for firm i, with $Y_i = E_{i,2001}$ for survivors and $Y_i = 0$ for exiters. The PPML model includes market fixed effects and a 1995-employment offset:

$$\mathbb{E}[Y_i \mid X_{i,1995},\, \mu_{m(i)}] = \exp\{\alpha + \beta' X_{i,1995} + \mu_{m(i)} + \log E_{i,1995}\}.$$

The covariates $X_{i,1995}$ include firm age, blue-collar share, and an indicator for employing directors. The model achieves AUC ≈ 0.652 for survival prediction, with tight market-level fit (RMSE ≈ 0.20 on log market employment).

Model Estimates

Variable	Coefficient
Age (1995)	-0.006***
Blue-collar share	0.057
Has director	0.032
Markets	7,146
Observations	1.3M

Model Performance

- **Survival prediction:**
  AUC = 0.65 (moderate)

- **Level prediction:**
  Correlation = 0.80 (strong)
- **Market totals:**
  RMSE = 0.20 (excellent)

We aggregate firm-level simulated employment to province-sector markets:

$$E^{(s)}_{m,2001} = \sum_{f \in \mathcal{F}_m} E^{(s)}_{f,2001},$$

compute the expected (mean over simulations) log employment growth μ^{Scale}_m, and form the recentered instrument $\tilde{Z}^{\text{Scale}}_m = Z^{\text{Scale},g^{\text{obs}}}_m - \mu^{\text{Scale}}_m$.

F.3 Validating the IV

Table F.21: Cross-market variation in employment growth over the policy period

	Mean	St. Dev	Median	P25	P75	P90
Scale						
Log Emp. (1995)	5.623	1.753	5.764	4.550	6.831	7.752
Log Emp. (2001)	5.868	1.619	5.953	4.794	7.031	7.941
Δ Scale (2001-1995)	0.270	0.805	0.254	-0.025	0.578	1.035
Instrument (recentered shock)						
Scale IV (2001)	0.158	0.359	0.048	0.000	0.172	0.437

Notes: Summary statistics for province-sector markets in the estimation sample over 1995-2001. The table reports the distribution of market size, measured by log employment, its change over the policy period, and the recentered scale shock used as an instrument in the spillover analysis.

Table F.22

	(1) $\Delta \log E$	(2) Z^{aggl}	(3) $\tilde{Z}^{\text{aggl}}$
$\Delta \log$ Avg Wage	-0.012	0.053	-0.021
	(0.044)	(0.043)	(0.020)
Δ White-collar Share	-0.154	0.077	0.122
	(0.244)	(0.259)	(0.116)
Δ Entry share	0.146	0.085	0.023
	(0.119)	(0.089)	(0.044)
Δ Exit share	0.230	0.070	0.138
	(0.401)	(0.441)	(0.149)
Δ Share firms < 5 Emp	0.261	0.010	0.078
	(0.175)	(0.188)	(0.095)
$\Delta \log$ N firms	-0.211**	-0.024	-0.046
	(0.096)	(0.097)	(0.075)
Observations	1,599	1,594	1,581
FE (Industry & Prov)	Yes	Yes	Yes
R^2	0.463	0.197	0.159
p-value(Other regressor)	0.001	0.232	0.381