# Cashing the Credit: Anomalous Billing and Firm Dynamics in Italy's Superbonus 110%

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**Abstract**

This paper asks whether Italy's Superbonus 110% was followed by anomalous billing among construction firms. The program subsidized home renovation through tax credits that could be sold to third parties, which weakened private incentives to scrutinize invoices: a homeowner who paid little out of pocket, and a buyer of the credit who was repaid by the state, each gained little from questioning the invoiced amount. We use accounts for [RERUN] 66,920 incorporated construction firms over 2015–2024 and the 17 February 2023 closure of credit transfers to new projects. [RERUN: numbers] The share of firm-years with both unusually high revenue per employee and an unusually high profit margin rises from 1.7% before the policy to 3.6% during it and 4.0% in 2023–24. In 2023–24 it exceeds a benchmark that holds the pre-policy dependence between the two signals fixed by 2.0–2.2 percentage points for firms incorporated before the cutoff, against 0.24 points (95% CI [0.03, 0.46]) for firms incorporated after it; comparing firms at the same age narrows this gap to about twofold. Provincial firm formation rose most where predetermined bank-branch density was higher[RERUN], with a coefficient about four times that on an organized-crime index, while provincial income shows no association. Accounting data cannot separate inflated billing from legitimate price increases under subsidized, inelastic demand, and the cohort comparison also bundles access to grandfathered projects with a lower subsidy rate for new

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ones; we therefore interpret the evidence as an accounting anomaly, not an estimate of fraud. Other transferable-credit programs, increasingly used in energy-transition and industrial policy, may face the same trade-off between liquidity and verification.

**Keywords:** tax-credit fraud, anomalous billing, revenue-per-employee indicator, joint-tail detection, bank-exposure design. **JEL:** H26, D22, K42, R31.

# 1 Introduction

Governments increasingly deliver subsidies as *transferable* tax credits: claims on the treasury that the recipient can sell to a third party. Transferability solves a real liquidity problem, since a household or small firm that cannot use a credit immediately can convert it to cash. It also changes who bears the risk of an inflated claim. When a credit can be sold and the state effectively guarantees its payment, the buyer has little reason to verify the underlying work, and the firm that issued the invoice bears little collection risk on an inflated amount. Whether this design leads to measurable overbilling is an empirical question.

We study it in Italy's Superbonus 110%. Introduced in May 2020 by Decree-Law 34/2020, the program granted a tax deduction of 110% of qualifying renovation costs and let beneficiaries take it as an invoice discount (*sconto in fattura*) or sell the resulting credit (*cessione del credito*). Transferable credits already existed for some earlier building bonuses, but the Superbonus combined transferability with a 110% rate and an open market that included banks. A construction firm could grant the discount, receive the tax credit, and sell it, often before the work was complete. The program became one of the largest fiscal interventions in recent Italian history and drew early warnings about firms set up to generate transferable credits [CITE: UIF communication of 11 February 2021]. On 17 February 2023, Decree-Law 11/2023 closed credit transfer and the invoice discount for new projects while grandfathering projects already filed. Firms incorporated after that date could not hold grandfathered projects of their own. We use this asymmetry to compare firms with different opportunities to work under the transfer regime.

We use AIDA accounts for all incorporated construction firms (NACE 41–43) that file statutory accounts, [RERUN] 66,920 firms over 2015–2024. Overbilling is not observed directly, so we look for accounting patterns that are uncommon under the pre-policy relationship between revenue, employment, and margins. We combine two detection designs from the literature. Following Fang and Gong (2017), the revenue-per-employee indicator flags revenue that is high relative to a firm's workforce. Following the joint-tail logic of Jacob and Levitt (2003), we ask whether that indicator coincides with an unusually high profit margin. Revenue billed without matching costs raises both, whereas additional legitimate revenue

with proportional costs leaves the margin unchanged. Both thresholds are set on the pre-policy (2018–19) distribution. We compare how often the two indicators occur together with how often they would if their pre-policy dependence were unchanged, and call the difference the joint-tail excess.

[RERUN: numbers and cohort labels change] Our main evidence comes from comparing incorporation cohorts. The joint-tail rate rises from 1.66% before the policy to 3.57% during it and 3.98% in 2023–24. Within 2023–24, the joint-tail excess is 2.23 and 2.03 percentage points for the two cohorts incorporated before the cutoff, against 0.24 points for firms incorporated after it. Comparing the Superbonus and post-cutoff cohorts at the same firm age narrows the gap from about nine-fold to about twofold (1.59 versus 0.72 points), with the ordering preserved. A second design relates provincial firm formation to bank-branch density in 2015, five years before the policy. [RERUN] In 2023, density has a coefficient about four times that on the Calderoni (2011) Mafia index, while provincial income has none. The outcome counts all incorporations and we impose no exclusion restriction, so we read this gradient as describing where firm formation concentrated, not as isolating the credit-transfer channel.

Four caveats qualify these results. First, accounting data cannot separate inflated billing from legitimate price increases. By making eligible work nearly free to households, the Superbonus made demand inelastic, and firms doing genuine work could raise prices and trip both indicators; our tests identify abnormal revenue–margin behavior, not fraud. Second, the cohort comparison bundles more than transfer access: firms incorporated before the cutoff could also hold grandfathered projects on the original 110% terms, whereas new projects after the cutoff faced a lower rate (90% for condominiums in 2023 and 70% in 2024). Third, cohort, age, and calendar year are linked. Within 2023–24 the cohorts differ in age, and at the same age they are observed in different years, so neither comparison alone isolates the mechanism. The excess for post-cutoff firms is itself positive [RERUN: check sign], so that cohort has low rather than zero access, for instance through subcontracting on grandfathered projects. Fourth, layered subcontracting, the sector's normal organizational form, raises revenue per employee; it would explain the cohort gap only if subcontracting differed by cohort at the same age. The exit patterns we report are descriptive and depend on the choice of time scale.

The paper adds to a growing evaluation of the Superbonus, set within a wider literature on pandemic-era fiscal policy. Italy's real GDP fell by about 9 percent in 2020, and, like most advanced economies, it responded with large fiscal support (Makin and Layton, 2021). Studies of that response stress its value as insurance against sectoral shutdowns (Guerrieri et al., 2022), its role in limiting output losses (Chudik et al., 2021), and its contribution to in-

flation where supply adjusted slowly (de Soyres et al., 2022). For the Superbonus, Accetturo et al. (2024) estimate that about one-quarter of subsidized spending would have occurred without the incentive and that the fiscal multiplier was slightly below one, and Corsello and Ercolani (2024) attribute about half of the rise in Italian residential construction costs between September 2021 and December 2023 to the program. Take-up far exceeded the initial cost estimates [CITE: EUR 220 billion by Q1 2024 against EUR 33 billion expected]. Other studies associate the incentives with energy-efficiency gains, with uptake favoring wealthier households (Carfora et al., 2025); find that most provincial capitals needed less than a 110% subsidy to make retrofits viable (Tajani et al., 2022); and report that the Superbonus was less regressive than earlier renovation deductions [CITE: Ufficio Parlamentare di Bilancio 2023]. These studies evaluate the program's aggregate cost, additionality, and distribution. We study the supply side: how construction firms' reported accounts changed under the credit-transfer regime. The cost evidence of Corsello and Ercolani (2024) bears directly on our main alternative explanation, since legitimate price increases produce the same accounting pattern as inflated billing.

We also contribute to three further literatures. First, we extend the fraud-detection tradition, which infers misconduct from statistical anomalies (Jacob and Levitt, 2003; Fang and Gong, 2017; Fisman and Wei, 2004), to firm accounts under a subsidy. Unlike Fang and Gong (2017), whose bound is absolute (implausible weekly hours), our threshold is relative and flags 10% of the pre-policy distribution by construction, and unlike Jacob and Levitt (2003) we cannot validate flagged cases against a retest or audit. Second, we contribute to work on tax evasion and enforcement, where third-party reporting and paper trails are the central enforcement technology (Kleven et al., 2011; Pomeranz, 2015; Carrillo et al., 2017; Slemrod, 2019) and fake invoicing is a recurring channel of fraud (Carrillo et al., 2023): transferability weakens the credit buyer's incentive to verify. Third, we relate to work on firm responses to fiscal incentives (Zwick and Mahon, 2017; Best et al., 2015), here on the extensive margin of firm entry, using predetermined banking geography as a continuous exposure (Callaway et al., 2024).

The remainder of the paper proceeds as follows. Section 2 describes the program and Section 3 the data; Section 4 lays out the empirical methods, Section 5 presents the results, Section 6 the robustness checks, Section 7 the limitations, and Section 8 concludes.

## 2 Institutional background

The Superbonus was introduced by Decree-Law 34/2020 as an enhanced tax deduction for qualifying investments in the Italian residential building stock. At its inception, eligible

expenditures incurred from 1 July 2020 could benefit from a deduction equal to 110 percent of qualifying costs. The scheme primarily targeted energy-efficiency and seismic-risk improvements, while also covering complementary interventions such as photovoltaic systems, energy storage, and electric-vehicle charging infrastructure when undertaken in conjunction with qualifying works. For energy-efficiency interventions, eligibility generally required an improvement of at least two energy-performance classes, or achievement of the highest attainable class. For this paper, the key feature was not the size of the deduction but the ability to monetize it. Under Article 121 of Decree-Law 34/2020, beneficiaries could opt for an invoice discount (*sconto in fattura*) or transfer the corresponding tax credit (*cessione del credito*) rather than wait to offset the deduction against future tax liabilities. With the invoice discount, the contractor granted an upfront discount and received a corresponding tax credit; with credit transfer, the beneficiary could transfer the credit to another party, including financial intermediaries. These arrangements substantially reduced the liquidity constraint associated with renovation expenditure and created a market in which subsidized construction activity could be converted into transferable fiscal claims. Importantly, the regime was not verification-free. Superbonus claims were subject to technical certifications, expenditure-congruity requirements, and, for the relevant options, a compliance endorsement (*visto di conformità*); controls were subsequently strengthened as concerns over fraudulent credits increased. The economic mechanism studied here is therefore more specific than an absence of formal oversight. Transferability and the invoice discount changed who had a reason to check the invoice: a homeowner who paid little out of pocket, and a buyer of the credit who was repaid by the state, each gained little from questioning the invoiced amount. Formal certification and private incentives to challenge an invoice are distinct margins of verification, and we study the second margin indirectly; we do not observe monitoring. The institutional break central to our empirical design occurred with Decree-Law 11/2023, effective on 17 February 2023, which sharply restricted access to *sconto in fattura* and *cessione del credito* for new interventions, while projects already filed (CILA, assembly resolution or permit application) kept access. The underlying tax deduction was not simultaneously abolished, although its rate fell (to 90% for condominiums in 2023 and 70% in 2024). Grandfathering operated at the project rather than the firm level, so incorporation cohort does not measure realized use of transferable credits. Instead, it proxies for the opportunity to work under the transfer regime: firms incorporated after the cutoff could not hold grandfathered projects of their own, whereas firms already operating before the cutoff could retain access through qualifying projects. This asymmetry motivates the cohort comparison developed below.

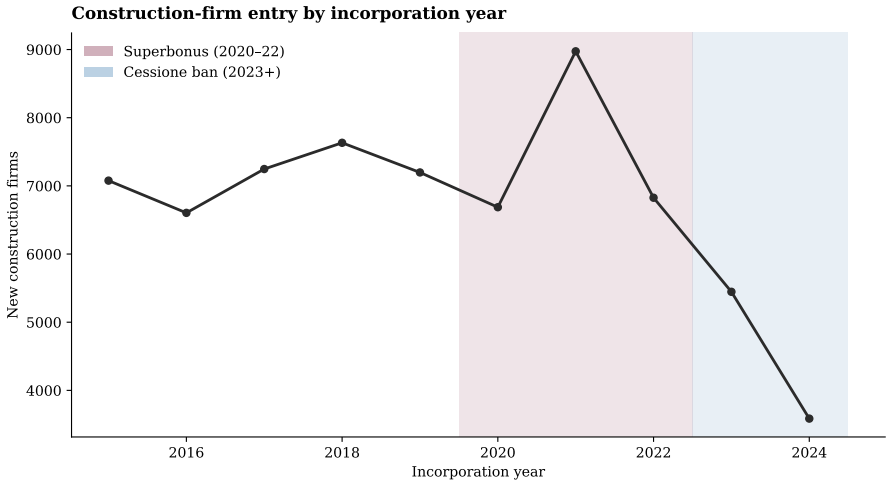



Figure 1: New construction firms (NACE 41/42/43) by year of incorporation, with the Superbonus and credit-transfer restriction marked. Firm formation peaks in 2021 and declines from 2022, falling further after the February 2023 restriction on credit transferability. Source: AIDA (Section 3).

# 3 Data

**Sample.** We obtain firm-level data from AIDA (Analisi Informatizzata delle Aziende Italiane, Bureau van Dijk), which provides registry and balance-sheet information for Italian companies that file statutory accounts. AIDA covers primarily incorporated companies. Sole proprietorships (*ditte individuali*) and most partnerships (*società di persone*) do not file statutory accounts and are therefore absent. Our results apply to the incorporated construction sector. We restrict the sample to construction firms whose primary NACE Rev. 2 code falls within division 41 (construction of buildings), 42 (civil engineering), or 43 (specialized construction), yielding a raw panel of 68,400 firms over 2015–2025.[1] [RERUN: sample counts change] We exclude the incomplete 2025 reporting year and firm-years with nonpositive or missing revenue, missing employee information, or missing profit. The resulting analysis sample contains 66,920 firms and 286,971 firm-years over 2015–2024.[2]

**Treatment definition.** [RERUN: switch to policy-aligned cohorts and update counts] Our treatment measure captures firms' potential exposure to the credit-transfer mechanism (*cessione del credito*) available under the Superbonus. We construct this measure using

[1]The AIDA extract contains 68,419 firms. We exclude 19 firms whose financial statements predate their recorded incorporation dates, leaving 68,400 firms in the panel for 2015–2025. Incorporation dates determine cohort membership, so we exclude these firms rather than assign them to potentially incorrect cohorts.

[2]Of the 39,475 firm-years excluded, 14,241 are from 2025 and 25,234 fall within 2015–2024. Within-window exclusion rates are similar across cohorts: 8.0% for incumbents, 8.2% for the Superbonus cohort, and 8.1% for the post-cutoff cohort.

incorporation dates from AIDA and the 17 February 2023 cutoff introduced by Decree-Law 11/2023, which closed access to credit transfer for new interventions while preserving it for qualifying projects already filed. We distinguish three incorporation cohorts: incumbents incorporated in 2015–2017, the Superbonus cohort incorporated in 2020–2022, and the post-cutoff cohort incorporated from 17 February 2023 through December 2024. The first two cohorts could operate while credit transfer remained available for new projects; the third entered after the cutoff and could not hold grandfathered projects of its own. All three cohorts could do Superbonus work; they differ in whether that work could carry transferable credits on pre-cutoff terms.

[RERUN: exclusions change under policy-aligned cohorts] Firms incorporated in 2018–2019 or between 1 January and 16 February 2023 remain in the full sample but are excluded from the cohort comparisons. The former fall outside the designated incumbent and Superbonus cohort windows. The latter entered shortly before the cutoff and had only a brief period in which to operate while credit transfer remained available for new projects.

Credit-transfer eligibility attached to projects rather than firms, whereas AIDA reports firm-level accounts. A pre-cutoff firm could therefore undertake both grandfathered projects that retained access to credit transfer and new projects that did not. We cannot observe this project mix or allocate revenue between transferable and non-transferable work. We interpret the estimates as differences across cohorts with different opportunities for exposure, rather than as the effect of credit transfer on an individual project.

**Outcomes.** Our accounting variables are operating revenue, net profit, and employee headcount. We express monetary values in constant 2024 euros using the ISTAT construction producer-price index. From these variables, we construct two detection measures: revenue per employee and the profit margin. We calculate revenue per employee as revenue divided by $\max(\text{employees}, 1)$, retaining all firm-years, including those with zero reported headcount.[3] The profit margin is net profit divided by revenue. Employee headcount also enters directly as an outcome and underlies the low-headcount indicator: revenue above the pre-policy revenue P90 with at most one employee. We measure firm entry as the number of newly incorporated firms in each province and year. We date exits from the procedure recorded in AIDA's registry field (e.g., *liquidazione volontaria*, *fallimento*) and classify them as voluntary, judicial, or administrative.

[3] 22,803 firm-years (7.9%) report zero headcount, across 13,590 firms; only 317 report zero in every observed year. Section 6 reports a robustness check that excludes zero-headcount firm-years and yields similar results.

**Banking exposure and provincial controls.** We measure banking exposure as bank branches (*sportelli*) per 100,000 inhabitants at the provincial (NUTS3) level. We use branch counts as of 31 December 2015 from Banca d'Italia and resident population as of 1 January 2016 from Eurostat (Figure 2, Panel A).[4] Measuring banking exposure five years before the Superbonus makes it predetermined relative to the policy. We compare it with three other provincial characteristics: provincial GDP per capita (Eurostat, NUTS3, 2019), the Mafia Presence Index developed by Calderoni (2011), and the share of residential buildings with five or more dwellings (ISTAT 2011 Census). The last variable proxies for condominium stock and potential exposure to the Superbonus condominium track. Bank branch density is our primary provincial exposure; the remaining variables enter as alternative exposures and, in a joint specification, as controls.

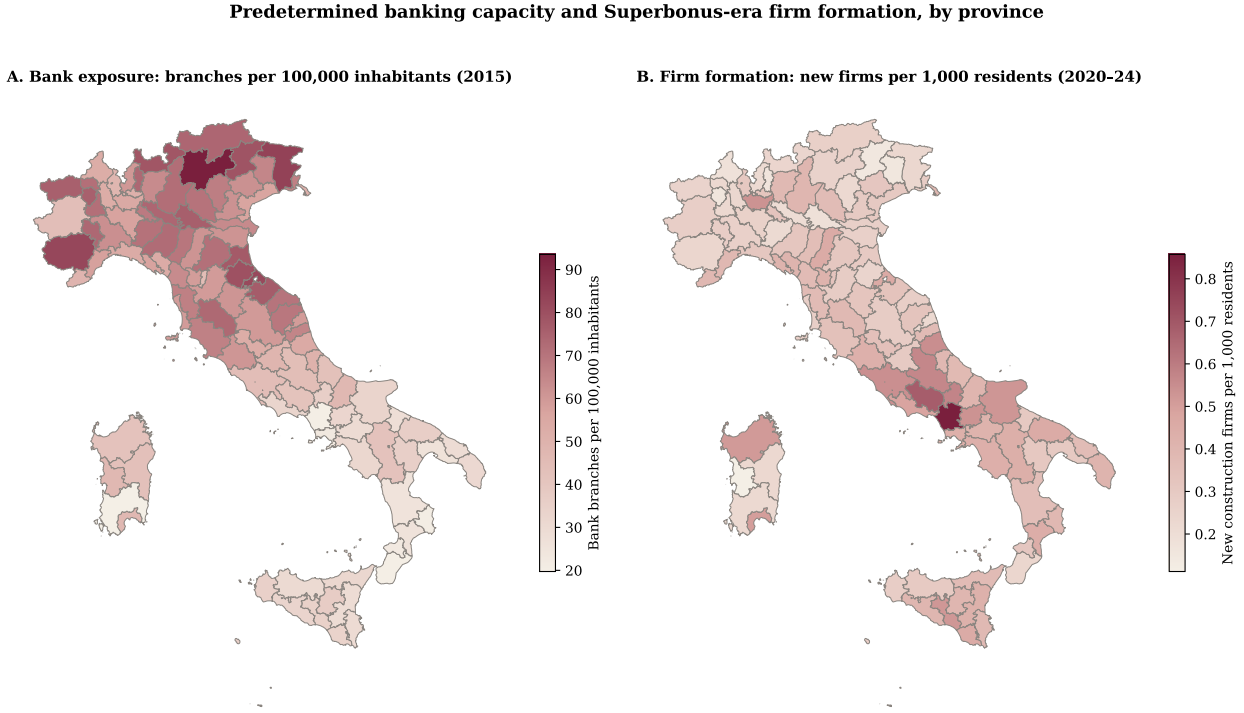



Figure 2: Predetermined banking capacity and Superbonus-era firm formation, by province. Panel A: bank branches (*sportelli*) as of 31 December 2015 per 100,000 inhabitants, using branch counts from Banca d'Italia and population as of 1 January 2016 from Eurostat. Panel B: new construction firms (NACE 41/42/43) incorporated in 2020–2022 (the Superbonus cohort), per 1,000 residents, using ISTAT resident population for 2023. Provincial boundaries are from ISTAT. Bank branch density is highest in the North, whereas Superbonus-era firm formation is highest in Campania and parts of the Centre–South.

[4]We assign 2015 branches in the four Sardinian provinces affected by the 2016 reorganization to their successor provinces so that branch counts and population are measured on consistent boundaries.

Table 1: Summary statistics

*Panel A. Summary sample, 2015–2024*

| | N (firm-years) | Mean | SD | Min | P10 | Median | P90 | Max |
|---|---|---|---|---|---|---|---|---|
| Operating revenue (EUR m) | 286,870 | 0.96 | 8.43 | 0.00 | 0.06 | 0.34 | 1.69 | 1,866.87 |
| Net profit (EUR m) | 286,870 | 0.05 | 0.92 | -83.35 | -0.00 | 0.01 | 0.11 | 293.07 |
| Employees | 286,870 | 5.36 | 10.62 | 0 | 1 | 3 | 11 | 1,069 |
| Revenue per employee (EUR m) | 264,067 | 0.20 | 0.99 | 0.00 | 0.03 | 0.10 | 0.33 | 272.77 |
| Profit margin | 286,870 | 0.059 | 0.199 | -1.000 | -0.022 | 0.044 | 0.225 | 1.000 |

*Panel B. Means by incorporation cohort*

| | Incumbent 2015–2017 | Superbonus Cohort 2020–2022 | Post-cutoff 17 Feb 2023–Dec 2024 |
|---|---|---|---|
| Operating revenue (EUR m) | 1.01 | 0.97 | 0.48 |
| Net profit (EUR m) | 0.04 | 0.07 | 0.03 |
| Employees | 5.46 | 5.18 | 5.01 |
| Revenue per employee (EUR m) | 0.20 | 0.21 | 0.11 |
| Profit margin | 0.045 | 0.077 | 0.085 |
| $N$ (firm-years) | 131,698 | 70,155 | 10,208 |

*Notes:* Panel A reports summary statistics for 286,870 firm-years covering 66,906 construction firms (NACE 41–43) over 2015–2024, with positive revenue and non-missing employee headcount and net profit. This table excludes 101 firm-years with implausible headcount identified as data-entry errors (Table 8); the estimation sample retains these observations and contains 286,971 firm-years covering 66,920 firms. Monetary values are expressed in millions of constant 2024 euros using the ISTAT construction producer-price index. Revenue per employee is summarized over positive-headcount firm-years only; the detection measure instead divides revenue by max(employees, 1) over all firm-years (Section 4). The profit margin is net profit divided by revenue, capped at $[-1, 1]$. Panel B reports means by incorporation cohort: incumbents (2015–2017), Superbonus (2020–2022), and post-cutoff (17 February 2023–December 2024). Firms incorporated in 2018–2019 or 1 January–16 February 2023 are included in Panel A but excluded from Panel B.

# 4 Empirical methods

Overbilling is not directly observed in firm accounts. Our empirical strategy proceeds in two steps. First, we construct two measures of anomalous accounts: a revenue-per-employee indicator and a joint-tail measure combining unusually high revenue per employee with high profit margins. Second, we examine how these anomalies relate to the policy by comparing incorporation cohorts with different opportunities for exposure to credit transfer and testing whether the patterns vary with predetermined provincial banking capacity. We also examine whether firms entering during the Superbonus remained active or subsequently exited, distinguishing voluntary closure, insolvency, and administrative exits.

## 4.1 Accounting anomaly measures

**Revenue-per-employee indicator.** Following the logic of Fang and Gong (2017), the first measure compares reported revenue with an observed input. Labor is the only input our AIDA extract reports every year. We define

$$S_{1,it} = \mathbf{1}\left\{ \frac{\text{revenue}_{it}}{\max(\text{employees}_{it}, 1)} > q^{\text{pre}}_{0.90} \right\},$$

where revenue is in 2024 euros and $q^{\text{pre}}_{0.90}$ is the ninetieth percentile of this ratio in the 2018–2019 pre-policy distribution. The lower bound of one keeps zero-headcount firm-years in the sample; Section 6.2 reports results that drop them. Since the threshold is set on pre-policy data, the indicator rate equals 10% in 2018–2019 by construction, and later increases measure movement of mass above a fixed pre-policy threshold; Table 3, Panel B shows whether that movement is concentrated in the tail.

High revenue per employee does not by itself establish overbilling: it can also reflect productivity, capital intensity, subcontracting, or errors in reported employment. Subcontracting is particularly relevant because a general contractor may book the full contract revenue while employing only a small in-house workforce. Our AIDA extract does not report payments to subcontractors, so we cannot adjust revenue for these costs. We therefore combine $S_1$ with a margin indicator to examine whether high revenue per employee coincides with unusually high profitability. Legitimate changes in operations can also produce this pattern.

**Joint-tail excess.** Drawing on the logic of Jacob and Levitt (2003), we combine two accounting indicators and examine whether they occur together more often than their pre-policy relationship would predict. The second indicator, the margin indicator, identifies unusually high profit margins:

$$S_{2,it} = \mathbf{1}\{\text{margin}_{it} > q^{\text{pre}}_{0.75}\}, \qquad \text{margin}_{it} = \frac{\text{profit}_{it}}{\text{revenue}_{it}} = 1 - \frac{\text{costs}_{it}}{\text{revenue}_{it}},$$

where profit is the year's net result (*utile/perdita di esercizio*) and $q^{\text{pre}}_{0.75}$ is the seventy-fifth percentile of the 2018–2019 profit-margin distribution. The joint-tail indicator, $JT_{it} = S_{1,it}S_{2,it}$, equals one for firm-years with both unusually high revenue per employee and an unusually high profit margin.

The margin identity shows when the two indicators move together. Additional revenue that carries proportional costs raises revenue per employee and leaves the margin unchanged. With fixed costs, higher revenue raises both. Revenue billed without corresponding recorded

costs also raises both (Table 2). Joint occurrence is therefore more informative about billing unsupported by costs than either ratio alone, but higher prices on legitimate work produce the same pattern. By reducing the household's marginal cost of eligible work to near zero, the Superbonus made demand for that work highly inelastic, so firms doing legitimate work could raise prices and trip both indicators. The accounting data cannot separate such price increases from inflated billing; doing so would require enforcement records, such as seized-credit cases, or transaction-level electronic invoices, which we do not observe.

Table 2: Illustration: how real growth and fabricated revenue move the two signals

| Scenario | Revenue | Costs | Profit | Rev/emp | Margin |
|---|---|---|---|---|---|
| Baseline | 100 | 90 | 10 | 50 | 10% |
| 50 new sales (cost) | 150 | 135 | 15 | 75 | 10% |
| 50 new sales (part fixed cost) | 150 | 120 | 30 | 75 | 20% |
| 50 new sales (no cost) | 150 | 90 | 60 | 75 | 40% |

*Notes.* Illustration: a stylized two-employee firm. Real growth whose costs rise in proportion to revenue lifts revenue per employee but leaves the margin flat (with fixed costs it raises both: the third row assumes 30 of the baseline costs are fixed and the rest move with revenue); fabricated billing lifts both, because fabricated revenue carries no matching cost and flows straight to profit. Subcontracting—the leading benign explanation for high revenue per employee—behaves like real growth: a general contractor books full revenue against a matching subcontract cost, so revenue per employee rises but the margin does not, tripping $S_1$ alone and not the joint tail. Requiring both signals also suppresses false positives among small firms, where revenue per employee (size-dependent) is easily tripped by a single large invoice while the margin (a ratio) is not.

The two indicators are related in the pre-policy data, so we do not benchmark their joint probability against the product of the marginals. We instead hold fixed the pre-policy odds ratio

$$\theta^{\text{pre}} = \frac{p_{11}^{\text{pre}}\, p_{00}^{\text{pre}}}{p_{10}^{\text{pre}}\, p_{01}^{\text{pre}}},$$

where $p_{jk}$ is the share of firm-years with $S_1 = j$ and $S_2 = k$. For each group $g$ of firm-years (a fiscal period, or an incorporation cohort within a period), we compute the joint probability $p_{11,g}^{0}$ implied by $\theta^{\text{pre}}$ at the group's observed rates of $S_1$ and $S_2$ (a Plackett copula; Appendix A.5 gives the solution), and define the joint-tail excess

$$\text{Excess}_g = p_{11,g}^{\text{obs}} - p_{11,g}^{0}.$$

The excess is zero in the pre-policy period by construction and compares the data with a benchmark of stable dependence: a positive value means the two indicators occur together more often than their pre-policy dependence implies at the group's observed rates. It reflects

overbilling only if, absent the policy, the dependence between the indicators would have stayed at its pre-policy level. Confidence intervals come from a firm-cluster bootstrap with 2,000 draws that re-estimates the thresholds $q^{\text{pre}}$ and $\theta^{\text{pre}}$ in each draw.

## 4.2 Empirical comparisons

**Cohort comparisons.** We compare both measures across three fiscal periods, pre-policy (2018–2019), policy (2020–2022), and 2023–2024, and, within 2023–2024, across the incorporation cohorts defined in Section 3. Before the 17 February 2023 cutoff, firms could take on projects financed through credit transfer; after it, they could continue those projects. Firms incorporated after the cutoff could not hold such projects of their own. Decree-Law 11/2023 grandfathered projects already under way, so the measures need not fall sharply at the cutoff. Cohort membership measures potential exposure to the mechanism, not its use (Section 3). Comparing cohorts within the same fiscal years holds common calendar conditions fixed, but the cohorts differ in age, and post-cutoff firms often report a partial first fiscal year. We therefore also compare cohorts at the same firm age. Table 3 Panel C and Figure 5 include age zero, which for most firms is a partial first fiscal year; the full-fiscal-year comparison restricts to firm age one and above (Section 6.3). Same-age comparisons place cohorts in different calendar years, so they remove age differences while reintroducing differences in calendar conditions, and neither comparison isolates the credit mechanism on its own. Cohorts may also differ in composition because firms chose when to incorporate. Table 1, Panel B reports observable differences across cohorts; selection on unobservables remains a limitation.

**Provincial banking exposure.** The market for transferred credits relied heavily on banks and other intermediaries, especially after early 2022, so we measure exposure to the mechanism by predetermined provincial banking capacity: bank branches per 100,000 inhabitants in 2015. Let $z_p$ denote this density in province $p$, standardized across provinces. We estimate

$$\ln n_{pt} = \alpha_p + \delta_t + \sum_{y \neq 2019} \beta_y \, z_p \, \mathbf{1}\{t = y\} + \varepsilon_{pt},$$

where $n_{pt}$ is the number of construction firms incorporated in province $p$ in year $t$, $\alpha_p$ and $\delta_t$ are province and year fixed effects, 2019 is the reference year, and standard errors are clustered by province. Every one of the 1,070 province-years has at least one new incorporation, so the logarithm is defined throughout. Each $\beta_y$ measures the change in the cross-province gradient between banking capacity and firm formation relative to 2019. Reading $\beta_{2023}$ as an

effect of the policy requires that, absent the Superbonus, formation in high- and low-density provinces would have followed parallel paths. Flat pre-period coefficients are consistent with this assumption but do not establish it, because a confounder that switches on with the policy, such as renovation demand correlated with wealth, urbanization, or the condominium stock, would leave no pre-period trace. We therefore also estimate a joint specification that adds 2019 provincial GDP per capita and the Calderoni (2011) Mafia Presence Index, each interacted with year. Banking capacity can affect formation through ordinary lending as well as through credit transfer, and we impose no exclusion restriction, so $\beta_y$ does not isolate the credit-transfer channel. Every province has some banks, so the estimates compare more- with less-exposed provinces and cannot be read against a no-exposure counterfactual.

In 2023–24, the only years in which both pre-cutoff and post-cutoff cohorts are observed, we also estimate the firm-level regression

$$\text{flag}_{ipt} = \gamma_{pt} + \lambda\,\text{elig}_i + \kappa\,(z_p \times \text{elig}_i) + u_{ipt},$$

where $\text{flag}_{ipt}$ is $S_{1,it}$, $\gamma_{pt}$ is a province-by-year fixed effect, $\text{elig}_i$ indicates incorporation before the cutoff, and standard errors are clustered by province. The coefficient $\kappa$ measures how much more steeply the indicator rises with banking capacity among pre-cutoff firms than among post-cutoff firms. Province-by-year fixed effects absorb influences common to all firms in a province and year but not influences that differ across incorporation cohorts within it, and pre-cutoff and post-cutoff firms also differ in age and in their base indicator rates. The regression therefore tests whether the banking gradient differs by potential exposure; it does not separate credit transfer from ordinary credit access.

**Firm survival and exit.** We describe exit patterns by incorporation cohort. Using AIDA's registry procedure field, we classify exits as voluntary (solvent wind-down), judicial (insolvency), or administrative; mergers, splits, and transfers end a firm's spell without counting as exits. Duration is measured on the firm-age time scale, and firms without a recorded exit are right-censored at their last filed accounts. The sample contains only firms that filed at least one set of accounts, so firms that exited before their first filing are absent, and not every firm is first observed at age zero. We estimate cause-specific Cox proportional-hazard models for voluntary and judicial exit, stratified by firm size at first observation, with standard errors clustered by province and other exit types treated as censoring, and report Aalen–Johansen cumulative-incidence functions by cause, which treat the other exit types as competing events. Cohorts are compared only at ages observed for every cohort in the comparison. Since cohorts reach a given age in different calendar years and may differ in

composition, these estimates describe what happened to each cohort and do not identify an effect of the mechanism.

# 5 Results

**Revenue per employee.** Under the policy the distribution shifts right, most in the upper tail. From pre-policy to the policy period real revenue per employee rises about 11% at the median, 18% at the ninetieth percentile, 23% at the ninety-fifth, and 38% at the ninety-ninth (Table 3, Panel B): the shift at the ninety-fifth percentile is about twice the median shift, and at the ninety-ninth about three and a half times. By 2023–24 the median catches up, rising about 15%, close to the 15% at the ninetieth percentile, while the far tail stays elevated (30% at the ninety-ninth).[5] A proportional expansion would move all quantiles equally, so the displacement is inconsistent with uniform expansion. It remains consistent with two readings: anomalous billing concentrated in a minority of firms, or a compositional shift toward large-contract work, since the Superbonus favored condominium *cappotto termico* jobs that are large relative to ordinary renovations and could fill the top decile with genuinely bigger projects. The cohort evidence below helps distinguish them: a compositional account would need firms incorporated during the policy to take larger contracts at the same firm age. One caveat: we deflate with a single national construction producer-price index, so if prices rose faster for the work concentrated in the tail, real tail growth is somewhat overstated.

[RERUN: numbers change; 1.45 vs 1.46 resolved: 1.45 is 2023–24, 1.46 came from a 2023–25 window] The share of firm-years flagged by $S_1$ rises from 10.0% pre-policy to 12.68% under the policy and stays at 12.28% in 2023–24 (Table 3). The joint-tail rate rises more sharply, from 1.66% to 3.98%, a joint-tail excess of 1.85 percentage points (Table 5, Panel A). The odds ratio between the two indicators is below one before and during the policy (0.57, 0.94) and above one in 2023–24 (1.45; Figure 6); this change in association does not by itself identify overbilling. Within 2023–24 the pattern changes: the joint-tail rate is 4.68% in 2023, when projects approved before the cutoff could still transfer credits, and falls to 3.27% in 2024, when grandfathered projects were running off and the rate had fallen to 70%, with the revenue-per-employee indicator returning toward its 10% pre-policy baseline (by fiscal year, 14.30% in 2023 and 10.22% in 2024).

[5] A two-sample Kolmogorov–Smirnov test rejects equality of the period distributions at every pairwise comparison ($D = 0.05$ pre vs. policy, 0.07 pre vs. 2023–24, 0.03 policy vs. 2023–24); with tens of thousands of firm-years per period, the $D$ statistics and quantile shifts are the informative quantities, not the (uninformatively small) $p$-values.

Table 3: The revenue-per-employee indicator

*Panel A. Flag rate by fiscal (observation) year (%)*

| | Pre-policy 2018–19 | Policy 2020–22 | 2023–24 2023–24 |
|---|---|---|---|
| >P90 | 10.00 | 12.68 | 12.28 |
| | (0.16) | (0.13) | (0.13) |
| >P95 | 5.00 | 6.75 | 6.30 |
| | (0.12) | (0.10) | (0.09) |
| >P99 | 1.00 | 1.55 | 1.43 |
| | (0.05) | (0.05) | (0.05) |
| $N$ (firm-years) | 49,164 | 115,299 | 92,923 |

*Panel B. Distribution shift, by fiscal year: % change in real revenue/employee vs. pre-policy*

| | Pre → policy | Pre → 2023–24 |
|---|---|---|
| P50 (median) | 10.6 | 14.7 |
| P90 | 18.3 | 14.9 |
| P95 | 23.2 | 16.7 |
| P99 | 37.8 | 29.5 |

*Panel C. Age-adjusted P90 flag rate by incorporation cohort (%)*

| | Incumbent | Superbonus | Post-cutoff |
|---|---|---|---|
| Age 0 | 3.93 | 6.27 | 3.27 |
| | (0.17) | (0.20) | (0.22) |
| Age 1 | 10.25 | 13.25 | 6.76 |
| | (0.23) | (0.24) | (0.40) |
| Age 2 | 11.63 | 13.47 | – |
| | (0.25) | (0.25) | – |

*Notes:* A firm-year is flagged when real revenue per employee (2024 euros via ISTAT construction PPI) exceeds the pre-policy (2018–19) percentile of that ratio (P90 = EUR 308,960, P95 = EUR 485,948); pre-policy rates are 10/5/1% by construction. Panels A and B are organized by *fiscal (observation) year*—a firm-year sits in the period of its accounts, whatever its incorporation year—whereas Panel C is organized by *incorporation cohort*; Panel C's "Post-cutoff" column is the cohort incorporated after 17 February 2023. **Panel A** pools firms across years, so standard errors (parentheses) are firm-clustered; 2023–24 is pooled (the within-period rise-then-fall, 14.3% in 2023 to 10.2% in 2024, is discussed in the text). **Panel B** reports the percentage change in each real revenue-per-employee quantile relative to pre-policy; in the policy period the tail (P90–P99) moves three to four times as much as the median (P50), inconsistent with a proportional expansion, while by 2023–24 the median catches up. **Panel C** compares cohorts at the same firm age (age = year − incorporation year); each firm appears once per (age, cohort) cell, so binomial standard errors are exact. Cohorts by incorporation date (Incumbent 2015–2017, Superbonus 2020–22, Post-cutoff ≥17 Feb 2023). FY2025 excluded. Sample: AIDA construction firms (NACE 41/42/43).

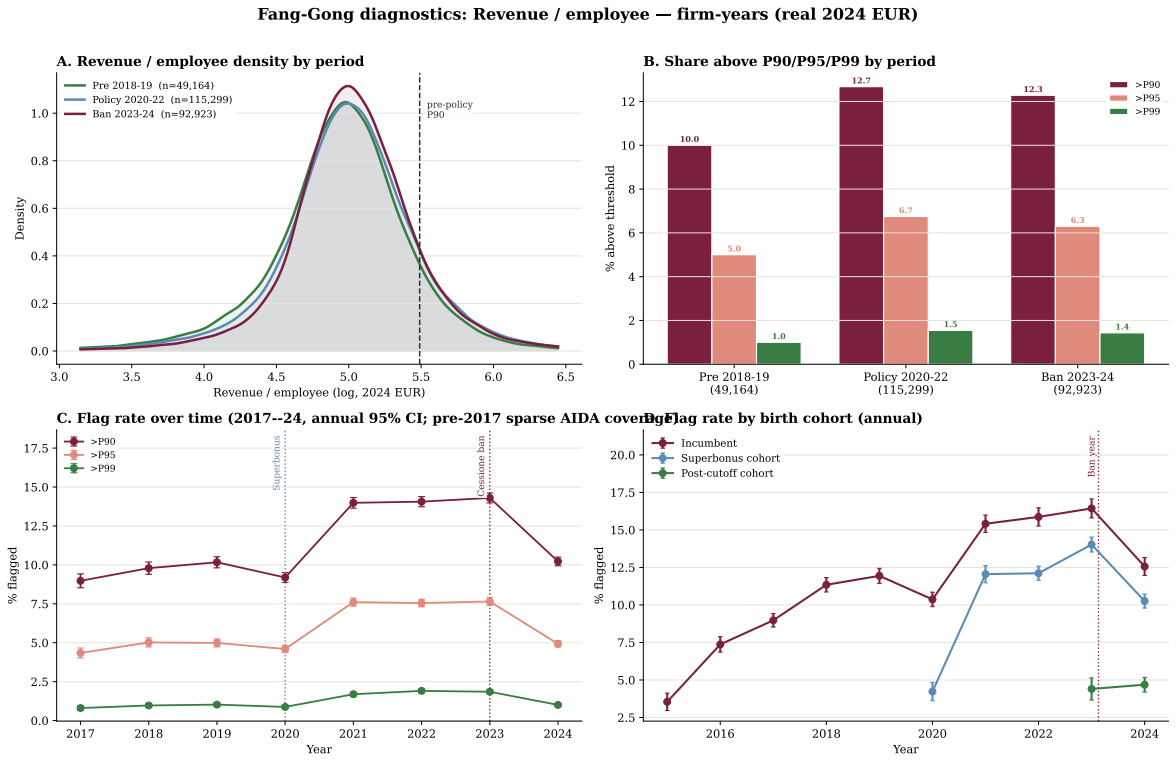



Figure 3: Revenue-per-employee indicator (all firms, $\max(\text{emp}, 1)$). Revenue per employee is formed with $\max(\text{employees}, 1)$, so zero-headcount firm-years are retained rather than dropped. The real revenue-per-employee distribution shifts right, and the share above the pre-policy P90 rises under the policy and in 2023–24. Panels A–C are by fiscal year; Panel D by incorporation cohort and calendar year.

Table 4: The Detection Battery: Full Sample versus Positive-Employee Subsample

| | Main: max(emp, 1) (all firms) | Positive-employee (drops zero-headcount) |
|---|---|---|
| *Panel A. Revenue-per-employee indicator rate (%)* | | |
| >P90, policy | 12.68 | 13.16 |
| >P90, 2023–24 | 12.28 | 13.20 |
| >P95, policy | 6.75 | 7.08 |
| >P95, 2023–24 | 6.30 | 6.99 |
| >P99, policy | 1.55 | 1.63 |
| >P99, 2023–24 | 1.43 | 1.60 |
| *Panel B. Jacob–Levitt joint tail* | | |
| Odds ratio, pre-policy | 0.57 | 0.63 |
| Odds ratio, 2023–24 | 1.45 | 1.52 |
| 2023–24 excess, incumbents (pp) | 2.23 | 2.43 |
| 2023–24 excess, Superbonus (pp) | 2.03 | 2.24 |
| 2023–24 excess, post-cutoff (pp) | 0.24 | 0.17 |
| *Panel C. Bank-exposure × eligibility interaction (pp)* | | |
| Year FE | 2.82 | 2.97 |
| Province + year FE | 2.88 | 3.03 |
| Macro-region × year FE | 2.77 | 2.89 |
| Province × year FE | 2.80 | 2.91 |

*Notes:* The main specification forms revenue per employee with max(employees, 1), retaining zero-headcount firm-years; the positive-employee column instead drops them. A zero-headcount firm is thereby handed a revenue per employee equal to its revenue, which is why the two columns are reported side by side. Pre-policy flag rates are 10/5/1% by construction; the pre-policy P90 anchor is EUR 308,960 in the full sample and EUR 291,532 in the positive-employee subsample. Panel C reports the bank-exposure × eligibility interaction (percentage points), estimated on 2023–24 firm-years under year, province+year, macro-region×year, and province×year fixed effects; this interaction is proportional to the cohorts' base flag rates and is null on the log-odds scale (Table 6), so it is reported as a proportional response, not a transfer-specific channel. In both samples the tail thickens under the policy, the odds ratio rises above one, and the post-cutoff cohort stays lowest—so the detection results do not hinge on the treatment of zero-employee firms.

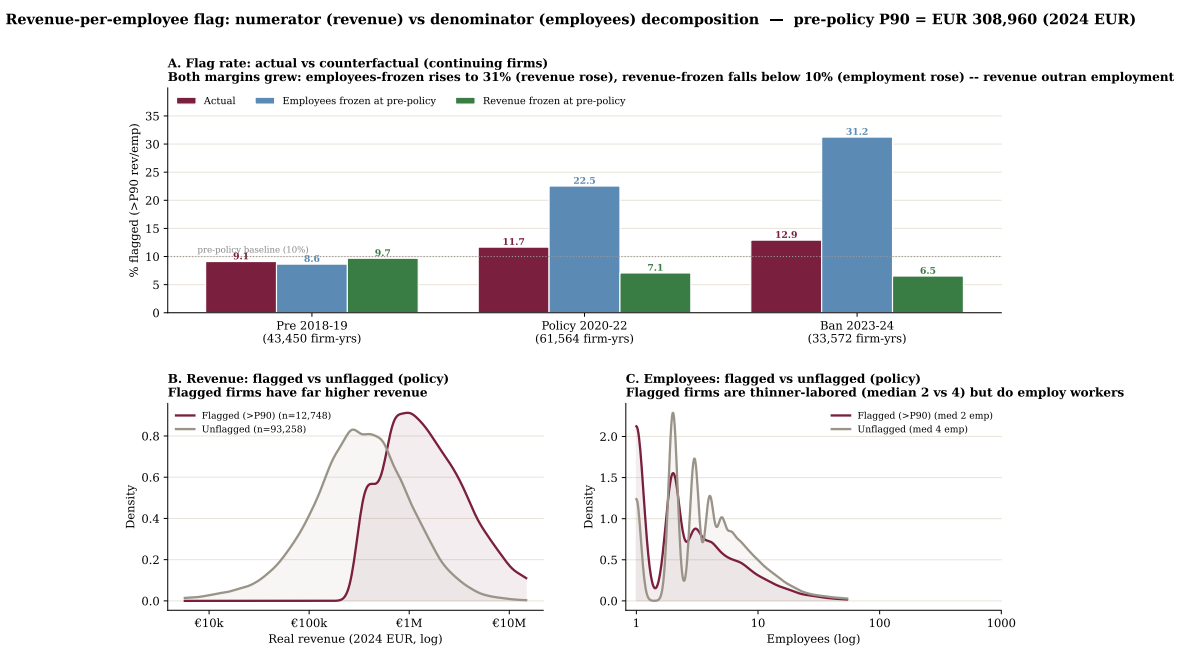



Figure 4: Decomposition of the rise in high revenue-per-employee firms: revenue growth versus a shrinking workforce. Panel A recomputes the indicator holding each firm's employment or revenue at pre-policy levels. Holding employment fixed, the indicator still rises; holding revenue fixed, it falls below the pre-policy baseline. Revenue grew faster than employment. The 10% reference line is the full-sample pre-policy flag rate (by construction); the continuing-firm subsample plotted here starts marginally below it, at 9.1% pre-policy. Panels B–C show that high revenue-per-employee firms have far higher revenue and, though somewhat smaller (median 2 vs. 4 employees), still employ workers.

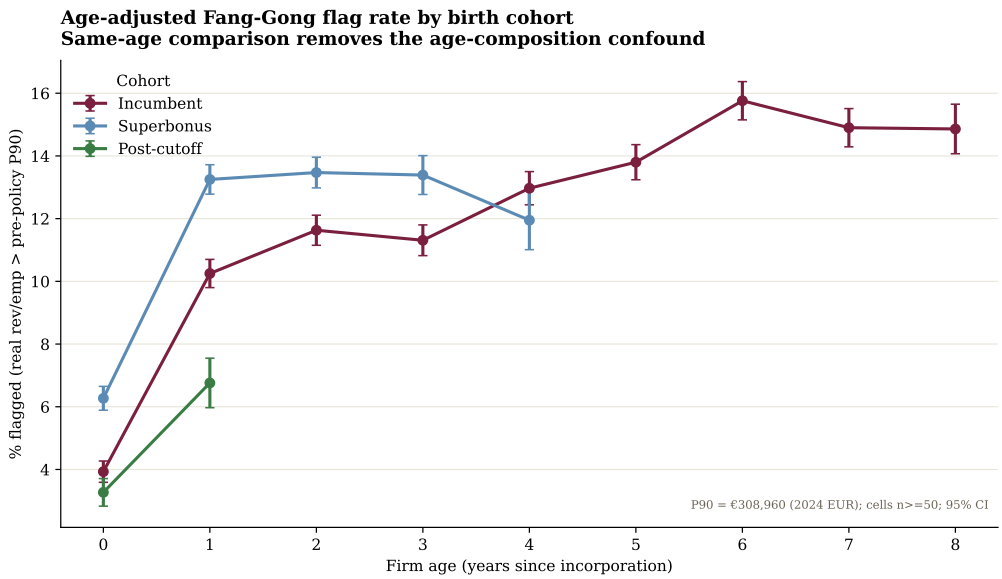



Figure 5: Age-adjusted indicator rate by incorporation cohort. At each firm age, the pre-cutoff cohorts have higher indicator rates than the post-cutoff cohort. At age zero, a partial first fiscal year for most firms, the post-cutoff cohort's rate (3.27%) is below that of incumbents (3.93%), although its age-zero firm-years fall in 2023–24.

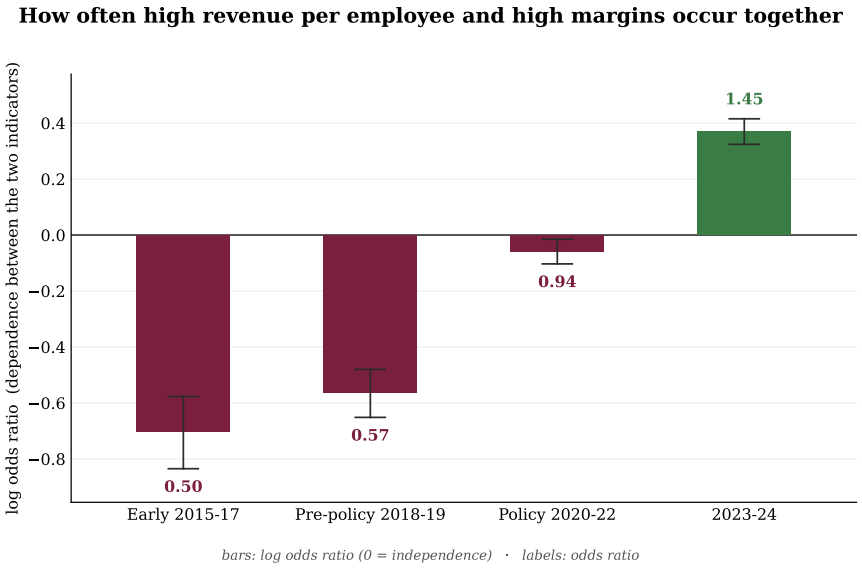



Figure 6: Association between the two indicators (high revenue per employee and high margin) by fiscal period. Bars show the log odds ratio with 95% firm-cluster bootstrap intervals; the number above or below each bar is the odds ratio. The odds ratio is below one before and during the policy period and above one in 2023–24.

If firms booking revenue with no workers were common, the share of firm-years with at

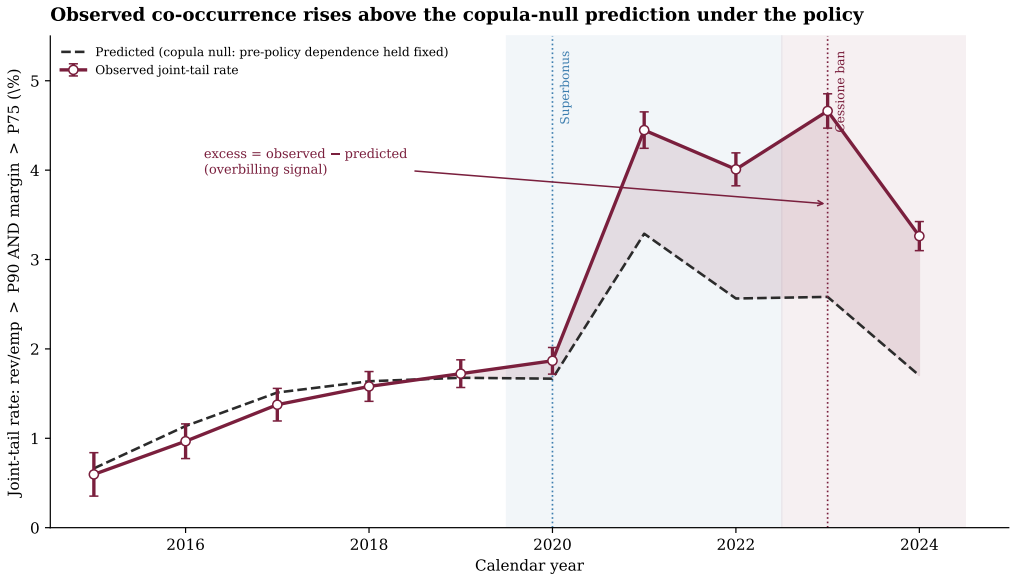



Figure 7: Observed joint-tail rate versus the copula-null prediction (pre-policy dependence held fixed), by year, with 95% firm-cluster bootstrap intervals. The observed rate tracks the benchmark through 2019 and exceeds it from 2021; the shaded gap is the excess.

most one employee would rise under the policy. It falls, from 28.2% in 2018–19 to 13.8% in 2023–24, but the decline began before the policy (30.5% in 2017), and within the Superbonus cohort from 34.2% at age zero to 12.2% at age four (Appendix Figure 17 shows the series by calendar year). The test is weak, since such firms would be a small share of all firm-years; Section 6.2 examines high-revenue, low-headcount firms directly. The zero-employee firm-years that the positive-employee robustness drops are likewise overwhelmingly dormant: only 2.9% clear the pre-policy revenue P90 (Appendix Figure 14), so they are low-revenue firms, not hidden high-revenue overbillers. The revenue-per-employee anomaly is therefore not labor disappearing but revenue outrunning a workforce that itself grew.

The excess differs sharply by incorporation cohort (Table 5, Panel B). Within 2023–24 the joint-tail excess is 2.23 percentage points for incumbents and 2.03 pp for the Superbonus cohort, against 0.24 pp for the post-cutoff cohort (incorporated after 17 February 2023), which could not hold grandfathered projects. These cohorts are observed in the same years but differ in age and size (Table 1, Panel B). The raw gap partly reflects the post-cutoff cohort's young age and partial first fiscal year. At firm age one, the gap narrows to 1.59 pp for the Superbonus cohort versus 0.72 pp for the post-cutoff cohort, with the ordering preserved (Section 6.3).

**Provincial banking exposure.** Banking capacity could raise both firm formation and billing through ordinary lending, so we ask whether the revenue-per-employee indicator rises

Table 5: Jacob–Levitt Joint-Tail Excess vs. the Copula Null

| Group | Firm-yrs | Firms | Joint (%) | Excess (pp) |
|---|---|---|---|---|
| *Panel A: by fiscal period* | | | | |
| Pre-policy 2018–19 | 49,164 | 29,409 | 1.66 | 0.00 |
| Policy 2020–22 | 115,299 | 49,936 | 3.57 | 1.03 |
| | | | | (0.09) |
| 2023–24 | 92,923 | 52,762 | 3.98 | 1.85 |
| | | | | (0.09) |
| *Panel B: 2023–24, by incorporation cohort* | | | | |
| Incumbent (pre-cutoff) | 25,020 | 13,507 | 4.46 | 2.23 |
| | | | | (0.13) |
| Superbonus (pre-cutoff) | 35,263 | 19,455 | 4.23 | 2.03 |
| | | | | (0.12) |
| Post-cutoff cohort | 10,169 | 7,544 | 1.32 | 0.24 |
| | | | | (0.11) |

*Notes.* Joint tail = $\mathbf{1}\{S_1{=}1\} \cdot \mathbf{1}\{S_2{=}1\}$, with $S_1$ real revenue per employee above the pre-policy P90 (the revenue-per-employee indicator) and $S_2$ profit margin above the pre-policy P75. Excess is the observed joint rate minus a copula-preserving null that fixes the pre-policy odds ratio (0.57) at each group's marginals. Sample: all firm-years, margin uncensored. Firm-cluster bootstrap standard errors in parentheses (2,000 draws), with the thresholds and the pre-policy odds ratio re-estimated in each draw. Panels A/B are by fiscal (observation) year and by incorporation cohort within 2023–24, respectively.

with bank exposure *more* steeply among pre-cutoff firms than among post-cutoff firms. Over 2023–24, the only years in which pre-cutoff and post-cutoff firms coexist, we regress the firm-level indicator on bank exposure interacted with pre-cutoff incorporation (the specification is in Section 4). The interaction is about 2.8 percentage points per standard deviation in every specification (2.80, s.e. 0.47, with province-by-year fixed effects; Table 6), but it is proportional to the cohorts' base rates: a one-standard-deviation rise in bank density raises each cohort's indicator rate by about 32% of its base rate (31.8% post-cutoff, 31.7% pre-cutoff), and on the log-odds scale the interaction is a precise zero (0.03, s.e. 0.07). The percentage-point gap reflects the pre-cutoff cohorts' roughly threefold higher base rate (13.5% versus 4.6%), not a transfer-specific channel. We therefore read the banking evidence as describing where firm formation concentrated, not as a test of the mechanism.

Table 6: Bank Exposure × Eligibility Interaction

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Bank exposure × pre-cutoff | 2.82*** | 2.88*** | 2.77*** | 2.80*** |
| | (0.48) | (0.49) | (0.46) | (0.47) |
| Year fixed effects | Yes | Yes | — | — |
| Province fixed effects | No | Yes | No | — |
| Macro-region × year | No | No | Yes | No |
| Province × year | No | No | No | Yes |
| Firm-years | 66,015 | 66,015 | 65,757 | 66,015 |
| Provinces | 106 | 106 | 105 | 106 |

*Notes:* The dependent variable is the P90 revenue-per-employee indicator—real revenue per employee above the pre-policy (2018–19) ninetieth percentile—in percentage points, estimated by linear probability model on 2023–24 firm-years. *Pre-cutoff* firms (the incumbent and Superbonus cohorts) were incorporated before 17 February 2023; their projects were grandfathered, so they retained *cessione del credito* access and could still sell the credit. *Post-cutoff* firms are those incorporated afterward, with no fresh access. Bank exposure is standardized 2015 branch density per 100,000 residents; *macro-region* denotes the five ISTAT NUTS-1 groupings (Nord-ovest, Nord-est, Centro, Sud, Isole), distinct from the twenty regions. Each column reports the interaction coefficient $\kappa$, the extra percentage-point effect of bank density on the flag for pre-cutoff over post-cutoff firms, identified because eligibility varies within province and year. Column 1 includes year fixed effects only; columns 2–4 add progressively finer fixed effects, and $\kappa$ is flat at about 2.8. This percentage-point interaction is proportional to the cohorts' base flag rates (13.5% pre-cutoff versus 4.6% post-cutoff): bank exposure raises the flag by about 32% of each cohort's base rate per standard deviation, and the interaction is a precise zero on the log-odds scale (0.03, s.e. 0.07). It therefore does not identify a transfer-specific channel net of ordinary credit access; identification rests on the cohort design (Table 5). The three cohorts contribute 70,452 firm-years in 2023–24; the 66,015 with both a defined P90 flag and matched 2015 provincial branch data enter the estimation, and column 3 drops one macro-region-by-year singleton. [RERUN: the 4,437 excluded firm-years are not missing flags: 4,103 are in Naples, whose province code "NA" the September build read as missing, and 334 had no province code in the flag file. The current build includes Naples; this table and Table 4 Panel C change at the re-run, and this sentence is then rewritten.] Standard errors clustered by province in parentheses. $^{***}p < 0.01$.

Figure 8 plots $\beta_y$ for each outcome. For new incorporations, pre-period coefficients are

flat and individually indistinguishable from zero ($-0.05$ to $-0.02$ across 2015–2018), with no directional trend toward the 2019 reference. The association is unchanged through 2020 and 2021, then rises sharply: 0.07 in 2022, 0.16 in 2023, 0.11 in 2024. One reading of the delay is the grandfathering rule: Decree-Law 11/2023 let projects filed before the cutoff keep transferring credits, so the gradient could remain high in 2022–23 even though the mechanism was closed to new work from February 2023. We do not observe project filings and cannot test this reading. Aggregate entry itself fell by roughly 31% after the cutoff, so the 2023–24 gradient describes formation contracting less where banking was deeper.

Table 7 compares banking with two alternatives. At 2023, bank exposure enters at 0.205 (s.e. 0.044) for new firms, four times the Mafia-index coefficient (0.053, significant only at the ten-percent level), with GDP per capita ($-0.014$) indistinguishable from zero. The pattern is therefore not "richer provinces did better" or "better-governed provinces did better".

Mean revenue per employee shows no bank gradient in any year (2023: $-0.036$, s.e. 0.044), and neither GDP per capita nor the Mafia index predicts it; mean employment per firm falls with bank exposure (2023: $-0.076$, s.e. 0.027), which the entry of small new firms would produce on its own. The province design therefore describes the entry response and leaves any per-firm banking effect confounded with composition. Figure 8 shows why: the province *mean* of revenue per employee is flat, yet the flag *rate*, a count of the upper tail of the same variable, tracks the policy (Figure 8, bottom row), because a province average washes out a response concentrated in the tail. We examine that tail response with the cohort design (Figure 5), not the provincial gradient.

Table 7: Provincial Banking Exposure: Multivariate Comparison ($\beta_{2023}$)

| Outcome | Bank | GDP/cap | Mafia |
|---|---|---|---|
| ln(new firms) | 0.205*** | -0.014 | 0.053* |
| | (0.044) | (0.035) | (0.032) |
| ln(active) | 0.067*** | -0.005 | 0.018* |
| | (0.015) | (0.014) | (0.010) |
| ln(mean rev/emp) | -0.036 | -0.044 | -0.044 |
| | (0.044) | (0.035) | (0.035) |
| ln(mean emp) | -0.076*** | -0.028 | -0.003 |
| | (0.027) | (0.029) | (0.018) |

*Notes:* Province event study $Y_{pt} = \alpha_p + \delta_t + \sum_y \beta_y z_p \mathbf{1}\{t = y\} + \varepsilon_{pt}$, reference year 2019; $\beta_{2023}$ shown. Exposures $z$ are standardised across provinces: bank = 2015 branches per 100k (predetermined), GDP/cap = 2019, Mafia = Calderoni (2011) index. Province and year fixed effects; standard errors clustered by province in parentheses. Bank exposure predicts *formation* (new and active firms) and lower mean employment per firm; GDP per capita and the Mafia index are individually insignificant or marginal and do not predict the rev/emp composition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

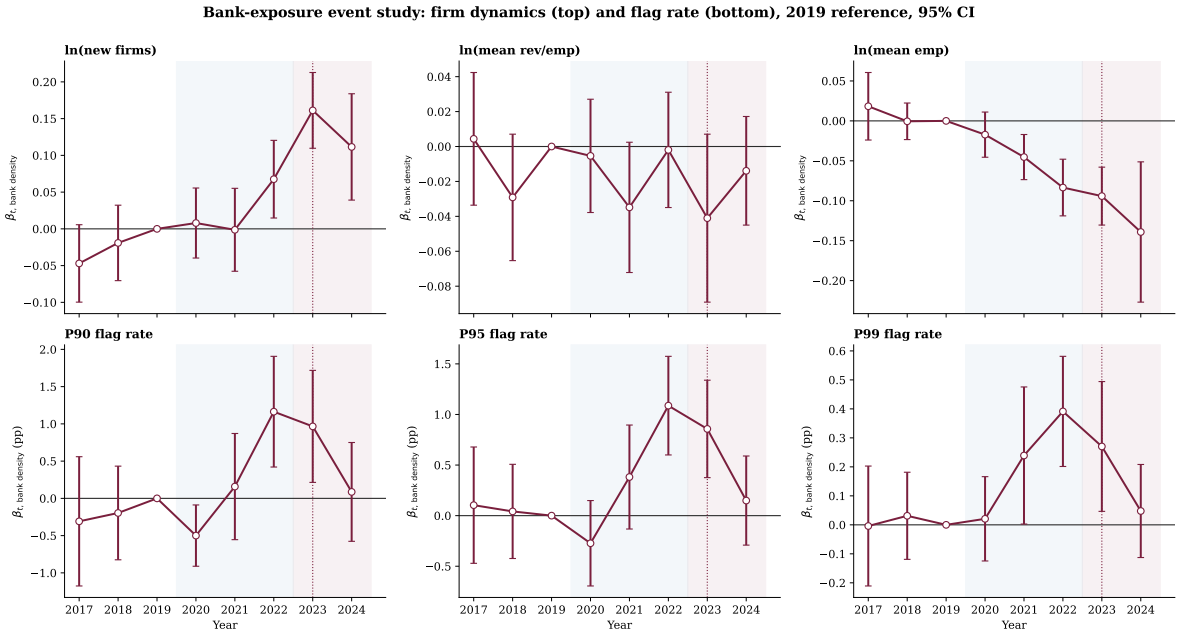



Figure 8: Bank-exposure event study across provincial outcomes (2019 reference, 95% CI; 2017 onward). Top row: new-firm formation rises under the policy, mean employment per firm falls, and the provincial *mean* of revenue per employee is a null throughout. Bottom row: the *rate* of the revenue-per-employee indicator at the P90, P95, and P99 thresholds—a count of the upper tail of that same revenue-per-employee variable—is flat before the policy, peaks in 2022, and recedes in 2024. The provincial mean washes out a tail-concentrated movement that the flag rate recovers; the flag response is examined with the cohort design (Figure 5), not this provincial gradient.

**Firm survival and exit.** Figure 9 reports exit hazards by firm age, cohort, and cause, while Appendix Figure 24 reports the corresponding cumulative incidence. By firm age 2, the oldest age common to all three cohorts, the cumulative incidence of voluntary (solvent) wind-down is 1.3% for incumbents, 2.4% for the Superbonus cohort, and 2.1% for the post-cutoff cohort; judicial insolvency is 1.5%, 2.0%, and 0.5%, respectively. Compared with incumbents, the Superbonus cohort exits more by both routes; the post-cutoff cohort exits more voluntarily and less through insolvency.

We interpret these patterns descriptively rather than as cohort effects. Firm age, incorporation cohort, and calendar period are mechanically linked, so the cohort ordering cannot be cleanly separated from period effects; consistent with this concern, on calendar time, both voluntary-exit hazard ratios fall from above two to below one, while the judicial ratios do not (Appendix Figure 25). Exit causes in AIDA are administrative classifications, which changed over time and complicate comparisons across cohorts. The greater role of solvent wind-down relative to insolvency is therefore descriptive evidence on the fate of the entry boom, not independent identification of the *cessione* mechanism.

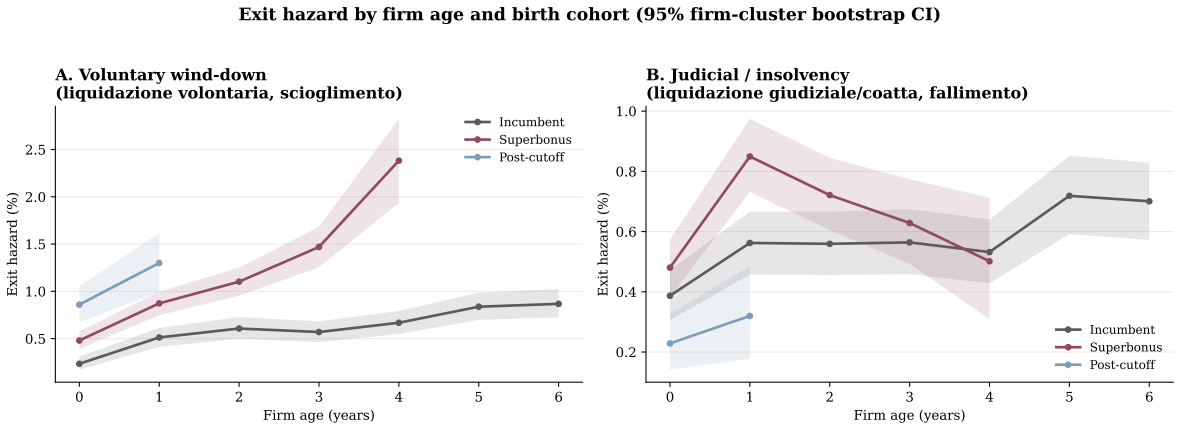



Figure 9: Cause-specific exit hazards by firm age and incorporation cohort, with 95% pointwise firm-cluster bootstrap confidence intervals. Age bins with inadequate risk sets are omitted. Incumbent = 2015–2017 births; Superbonus = 2020–2022 births; Post-cutoff = births on or after 17 February 2023.

# 6 Robustness

## 6.1 Sample

The detection results are stable to restricting attention to firms recorded as active (*Attiva*), which drops about one-eighth of firm-years (firms in liquidation, cancelled or failed): the

headline flag and joint-tail rates move by less than 0.05 percentage points, so the evidence is not driven by the tail of exiting firms.

## 6.2 The employee denominator

Revenue per employee is computed over all firm-years, imputing max(employees, 1) for zero-headcount firms. Dropping those firms and keeping only positive-employee firm-years leaves every result intact: the cohort excess, the P90–P99 indicator rates, the odds-ratio reversal and the bank-exposure × eligibility interaction (Table 4 and Appendix Figure 27). Keeping zero-employee firm-years lowers the P90 indicator rate (12.3% versus 13.2%): they are mostly low-revenue, and their share falls over the window (11.6% pre-policy to 4.5% in 2023–24), so they do not create the rise.

The threshold is fixed, but the population to which it applies is not: the share of firm-years reporting zero headcount falls from 13.0% in 2015 to 3.9% in 2024, largely because revenue growth moves firms across the thresholds that determine the statutory filing format and hence headcount disclosure.

The same low-headcount firms can be examined directly rather than dropped. The low-headcount indicator flags a firm-year with real revenue above the pre-policy (2018–19) revenue ninetieth percentile (about EUR 1.27 million) and at most one employee. The share rises from 0.55% in 2017 to 0.72% in 2019, peaks at 1.11% in 2021, and falls after the cutoff to 0.40% in 2024, below its 2017–19 level (Figure 10). Within 2023–24, the incumbent and Superbonus cohorts sit at 0.58–0.67% against 0.17% for the post-cutoff cohort (Appendix Figure 26). High revenue and low headcount are each legal and common for small firms, and restricting to firms recorded as active moves the shares by under 0.05 percentage points, so the low-headcount indicator is descriptive evidence consistent with billing through firms with almost no staff, not proof.

At the other extreme, 101 firm-years report more than 50 employees with revenue per employee below EUR 10,000, which we treat as data-entry errors. We exclude these firm-years from the descriptive statistics (Table 1) but retain them in estimation, where an inflated headcount places a firm in the *low* tail of revenue per employee and so cannot inflate the flag. At the firm's normal headcount, nine of the ten firm-years in Table 8 (eight firms) would clear the indicator threshold, and seven of those eight firms report revenue above EUR 1 million, so the spike *masks* a high revenue per employee; correcting the headcount would only raise the measured flag rate. We cannot tell from the accounts whether these are errors or deliberate misreporting; either way, they lower our estimates.

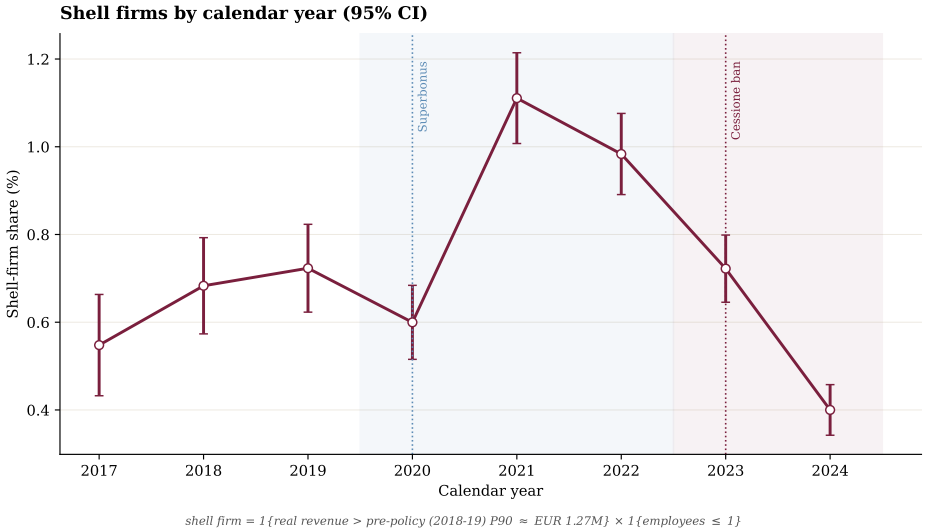



Figure 10: Low-headcount indicator by calendar year. The indicator flags a firm-year with real revenue above the pre-policy (2018–19) P90 (≈EUR 1.27 million) and at most one employee. The share rises through 2017–19, peaks in 2021, and falls after the cutoff to below its 2017–19 level. Wilson 95% intervals.

Table 8: Firms with a one-year headcount spike

| Firm | Cohort | Year | Headcount (reported) | Revenue (EUR m) | Normal HC | Rev./emp. at normal HC (EUR) |
|---|---|---|---|---|---|---|
| 1 | Superbonus | 2023 | 3,310 | 3.14 | 1 | 3,136,093 |
| 2 | Incumbent | 2023 | 417 | 2.70 | 3 | 900,013 |
| 3 | Incumbent | 2019 | 250 | 1.60 | 2 | 801,484 |
| 4 | Superbonus | 2024 | 2,161 | 2.05 | 4 | 511,988 |
| 5 | Incumbent | 2024 | 2,688 | 3.66 | 8 | 457,173 |
| 6 | Incumbent | 2024 | 188 | 1.43 | 3.5 | 409,314 |
| 7 | Superbonus | 2023 | 486 | 1.50 | 4 | 375,705 |
| 4 | Superbonus | 2023 | 694 | 1.46 | 4 | 364,692 |
| 8 | Incumbent | 2021 | 95 | 0.36 | 1 | 356,188 |
| 9 | Incumbent | 2020 | 423 | 1.77 | 6 | 294,600 |

*Notes:* Analysis-sample firm-years (2015–2024) with an implausible headcount spike (reported employees above 50 paired with revenue per employee below EUR 10,000, a data-entry error). "Normal HC" is the firm's median headcount in its non-spike years; "Rev./emp. at normal HC" applies the spike-year revenue, in nominal euros, to that headcount. Nine of the ten rows exceed the pre-policy P90 flag threshold (EUR 308,960), so the reported headcount mechanically suppresses revenue per employee below the flag. This is *suggestive, not dispositive*: from accounts we cannot separate a coincidental error in a high-revenue firm from deliberate misreporting. [RERUN: the generator now converts revenue to 2024 euros and computes the threshold from the main sample.] The direction is conservative—correcting the headcount makes these firms flag, raising the measured overbilling rate. Firm identifiers are anonymized; a label repeated across two rows denotes the same firm with a headcount spike in two separate years.

## 6.3 Age and fiscal-period adjustment

The post-cutoff cohort is young and files a partial first fiscal year, which lowers its revenue per employee and so its indicator rate. Restricting to same-age, full-fiscal-year observations (firm age $\geq 1$; for firms with calendar fiscal years, 99.8% of the sample, age one is the first full fiscal year) raises the post-cutoff cohort's joint-tail excess and narrows the gap: at firm age one the Superbonus-cohort excess is 1.59 percentage points against 0.72 for the post-cutoff cohort, roughly twofold against the raw nine-fold, with the ordering preserved. Part of the raw magnitude is thus a reporting-period artifact, and the ordering survives.

## 6.4 Firm-level bank exposure and the intensive margin

Re-estimating the bank-exposure design at the firm-year level with firm fixed effects absorbs the time-invariant provincial exposure, so only changes within firms over time identify the coefficient, and the bank effect on the province-average per-firm outcomes essentially disappears. The gradient appears in entry, the number of firms that form, and, less precisely, in the *tail* of the revenue-per-employee distribution, where the provincial flag rate carries a bank gradient at the P90 and P95 thresholds in 2022–23 (Section 5); it does not appear in the provincial *mean*, which averages that tail away.

## 6.5 Renovation demand

A natural alternative is that high-bank provinces simply had more building stock to renovate. When we add the predetermined provincial condominium share (ISTAT 2011 census) to the formation comparison, it enters at 0.010 (s.e. 0.034) in 2023, indistinguishable from zero, while bank exposure remains 0.209 (0.048). A dedicated placebo using the vacant- and poor-condition building stock would sharpen the point further; because it requires building-condition data at the province level that is not yet assembled, we leave it to future work; the cohort comparison, in which renovation demand is common to all cohorts but credit-transfer access is not, addresses part of this concern.

## 6.6 Macro-region and time shocks

Banking capacity and the North–South divide are correlated, so the provincial flag-rate gradient could in part reflect geography rather than credit intermediation. We re-estimate the flag-rate event study absorbing macro-region$\times$year fixed effects, where macro-region denotes the five ISTAT NUTS-1 groupings (Nord-ovest, Nord-est, Centro, Sud, Isole), distinct from the twenty regions, so only within-macro-region variation in branch density identifies the

bank gradient; 55% of the cross-province spread in exposure survives this absorption, so the absorbed specification retains meaningful variation. The 2022–23 point estimates fall and lose precision: the 2022 gradient at the P90 threshold moves from 1.20 percentage points (s.e. 0.37) to 0.88 (0.77), and at the P99 threshold from 0.38 (0.10) to 0.20 (0.16). The point estimates shrink by a quarter to a half while the standard errors roughly double, so the tail response is not cleanly separable from a regional gradient at conventional levels; it is suggestive, not sharply identified once geography is absorbed. The same geography threatens the cohort comparison: the northern share of new entrants rises from 34% to 44% across cohorts, and because northern provinces are branch-rich, this compositional shift could inflate the cohort gap, but the flag ordering holds *within* both the North and the South separately, so it is not an artifact of the shift.

# 7 Limitations

Several limitations remain. Cohort assignment proxies for *cessione* eligibility, which was set at the project level, and the February 2023 closure coincided with a reduction in the subsidy rate; cohorts may also differ because firms selected into entry at different points in the policy cycle. The banking design uses predetermined continuous exposure, not administratively assigned access, and the survival evidence is descriptive.

Censoring at the last filed accounts treats firms that stop filing without a recorded exit as survivors to that date; if non-filing often precedes exit, exit is understated, more so for recent cohorts whose 2024 accounts may not yet be filed, which also shortens the post-cutoff cohort's follow-up. The indicator rate and the joint-tail excess are built from the same accounts, so they are one piece of evidence, not two. The banking design describes where formation concentrated; its firm-level interaction is proportional to the cohorts' base rates and is not an independent test of the transfer channel. Predetermined branch density proxies local credit-absorption capacity, and at the twenty-region ENEA level its correlation with realized Superbonus intensity is positive but imprecise ($\rho = 0.19$).

The most consequential check we cannot yet run is enforcement data, seized-credit records that would overturn the reading if they showed no association with the flagged firms; a placebo sector with inelastic subsidized demand but no transferable credit is the natural next step.

# 8 Conclusion

This paper examines whether the transferability of a highly generous tax credit was followed by changes in firms' reported accounts in the Italian construction sector. Using 66,920 incorporated construction firms observed over 2015–2024, we document a marked increase in the joint occurrence of unusually high revenue per employee and elevated profit margins. The joint-tail rate rises from 1.66 percent in the pre-policy period to 3.98 percent in 2023–2024. Within 2023–24, the excess over the dependence-preserving benchmark is 2.23 percentage points for incumbent firms and 2.03 percentage points for firms incorporated during the Superbonus period, compared with only 0.24 percentage points among firms incorporated after the February 2023 cutoff. At firm age one, the gap between the Superbonus and post-cutoff cohorts narrows to 1.59 versus 0.72 percentage points, with the ordering preserved, and the ordering also holds when zero-employee firm-years are dropped.

These patterns provide reduced-form evidence that abnormal revenue–margin behavior was more pronounced among cohorts with an opportunity to access transferable credits. The provincial evidence is consistent with the same institutional environment: predetermined banking capacity strongly predicts where the post-policy firm-formation response was concentrated. At the same time, the bank-exposure interaction in firm-level anomaly rates is proportional to cohort base rates rather than transfer-specific on the log-odds scale, and should therefore be interpreted as corroborating the geography of the response rather than as an independent identification strategy. The survival evidence is similarly descriptive.

The results should not be interpreted as direct estimates of fraud. Accounting data cannot distinguish inflated billing from legitimate increases in prices or markups generated by highly subsidized and relatively inelastic demand; moreover, transfer eligibility is observed only indirectly through firm cohorts, while the 2023 cutoff coincided with other changes in the generosity of the program. The evidence therefore supports a narrower conclusion: transferable credits were associated with a change in the joint distribution of revenues and margins that is difficult to reconcile with proportional expansion alone. Linking these accounting signatures to enforcement records or transaction-level electronic invoices would provide the natural next test.

More broadly, the Italian experience highlights a design trade-off in transferable tax credits. Transferability can relax liquidity constraints and broaden access to fiscal incentives, but it can also separate the party financing the claim from the parties with the strongest incentives to scrutinize the underlying transaction. Effective program design therefore requires verification, liability, and monitoring incentives to remain aligned with the fiscal risk ultimately borne by the state.

## Data and code availability

All analysis is reproducible from a single script (`run_all.sh`) that regenerates every table and figure from the underlying panel; the replication package includes the code and the public province-level inputs (ISTAT, Banca d'Italia, Eurostat, ENEA). The firm-level AIDA data are proprietary (Bureau van Dijk) and cannot be redistributed; a manifest in the replication package lists the required AIDA fields so that licensed users can reproduce the panel. All figures are data visualizations generated by the committed analysis scripts from the underlying data; AI-assisted tools were used to help write and refine that figure-generation code, with all output verified by the authors.

## Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the authors used Anthropic's Claude (via Claude Code) in order to clarify the language throughout the paper, verify the consistency of the mathematical formulation, and help write the figure-generation code. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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# A Supplementary figures

This appendix collects the diagnostic figures underlying each analysis.

## A.1 Descriptive exhibits

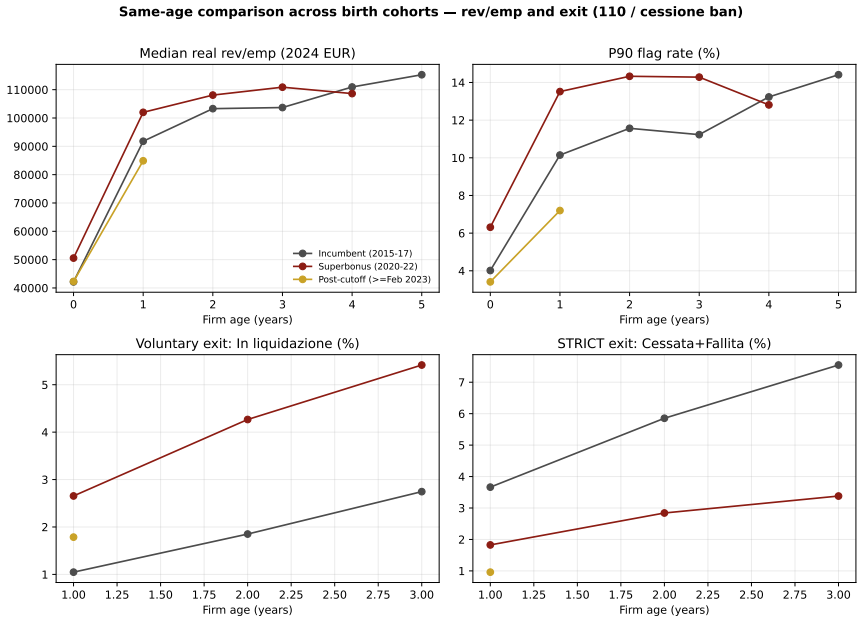



Figure 11: Same-age revenue per employee and exit, by cohort. The exit panels use AIDA's legal-status field, not the procedure field used in the main text: voluntary = in liquidation; strict = ceased or bankrupt; any = any status other than active.

## A.2 Synthetic control (descriptive; placebo does not reject)

The outcome is construction-sector gross value added in chain-linked volumes (reference year 2015; NACE F) from Eurostat national accounts, for Italy against a donor pool of European countries. Italy diverges from its synthetic counterpart after the Superbonus, but a permutation test does not reject the null ($p = 0.56$), so we do not rely on this comparison; the firm-level analysis does not depend on it.

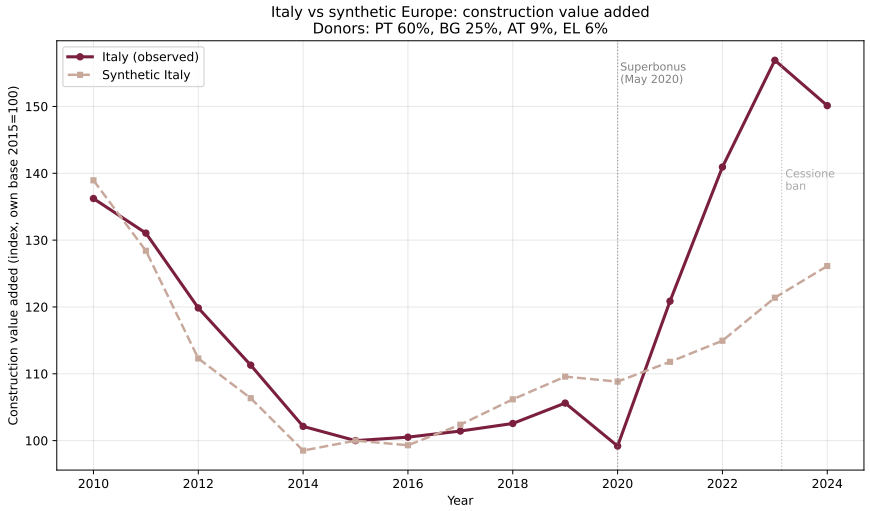



Figure 12: Italy versus synthetic Italy, construction activity (descriptive).

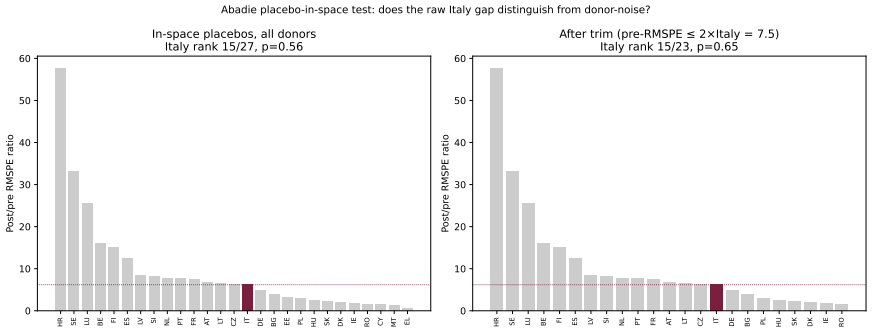



Figure 13: Permutation (placebo) test: Italy's gap is not extreme relative to donor placebos ($p = 0.56$).

## A.3 Revenue-per-employee indicator diagnostics

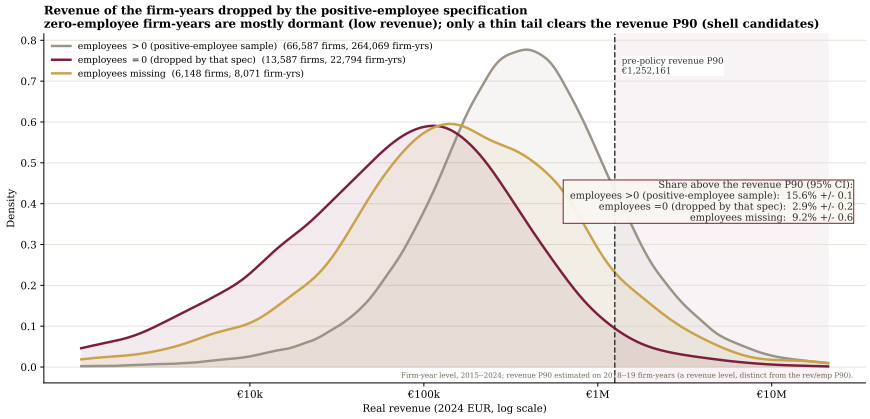



Figure 14: Revenue of the firm-years dropped by the positive-employee specification. The frame is all firm-years with positive revenue, 2015–2024, and so includes the firm-years with missing headcount that the analysis sample (Section 3) excludes. Zero-employee firm-years are mostly dormant (low revenue); only 2.9% clear the pre-policy revenue P90, so the positive-employee robustness sets aside low-revenue firms rather than high-revenue firms. At the main threshold, where a zero-employee firm-year's revenue per employee equals its revenue, 17.3% of zero-employee firm-years in the analysis sample are flagged; Section 6.2 reports results that drop them.

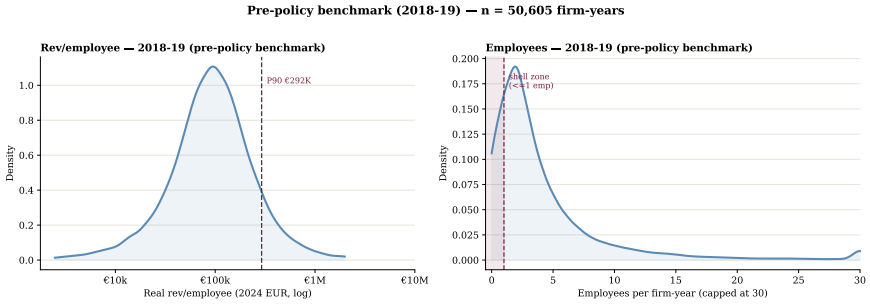



Figure 15: Pre-policy (2018–19) benchmark distributions of revenue per employee and of employment, with the P90 threshold and the ≤1-employee zone marked.

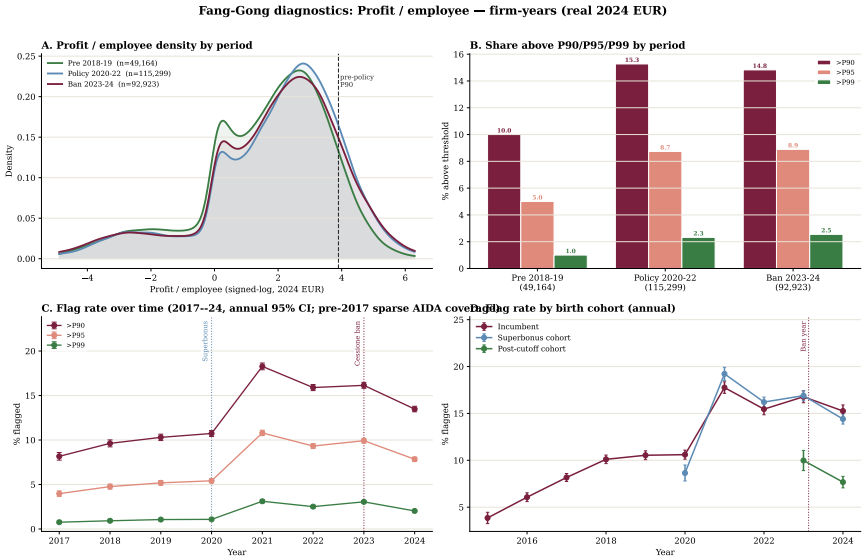



Figure 16: Profit per employee: density by period, share above the pre-policy P90/P95/P99, over time, and by incorporation cohort.

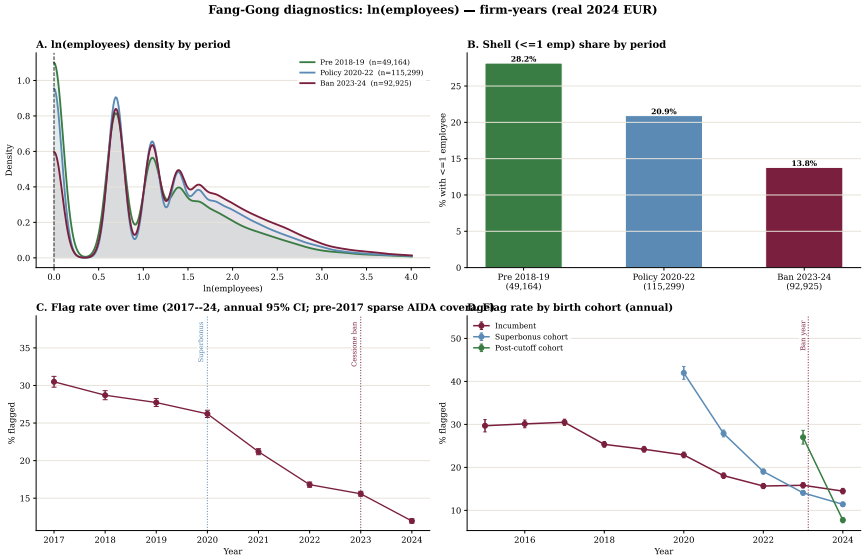



Figure 17: The $\leq$1-employee share falls over time. The share of firm-years with at most one employee *falls* over the window, and within the Superbonus cohort it falls over calendar time.

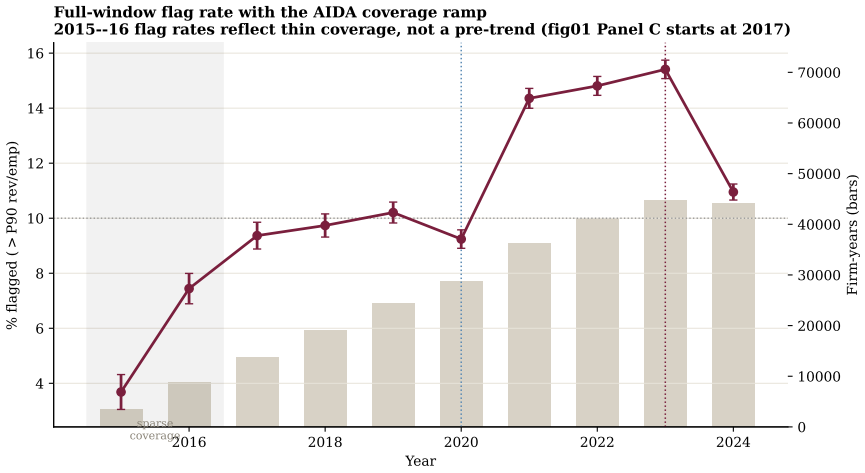



Figure 18: Full-window flag rate with the AIDA coverage ramp. The low 2015–16 flag rates reflect thin early coverage (3,393 firm-years in 2015 vs. 44,167 in 2024), not a pre-trend; the main-text Figure 3 Panel C therefore begins in 2017.

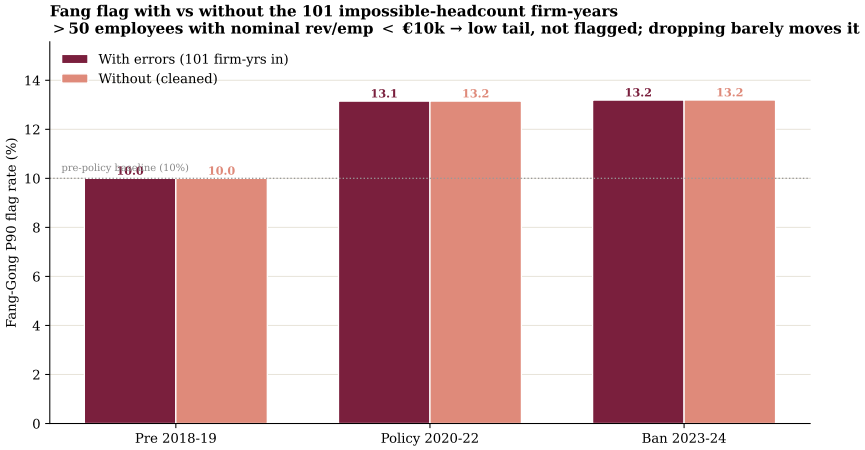



Figure 19: Revenue-per-employee indicator with and without the 101 impossible-headcount firm-years (employees >50 paired with nominal revenue per employee < EUR 10,000, which we treat as data-entry errors). Dropping them moves the P90 flag by at most 0.01 percentage points, so detection is not an artifact of the errors.

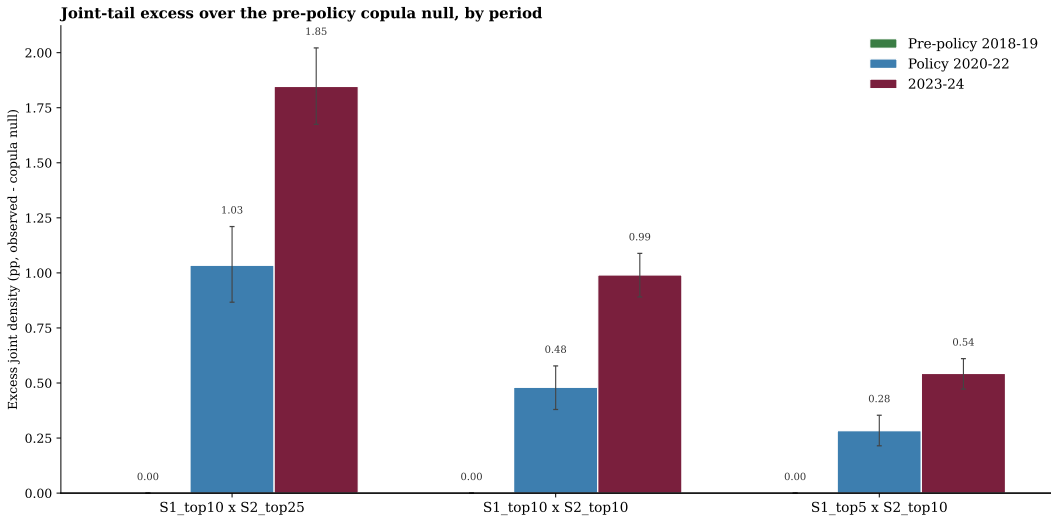



Figure 20: Joint-tail excess over the pre-policy copula null across threshold choices, by period, with 95% firm-cluster bootstrap intervals. The excess, in percentage points, declines as the thresholds tighten, as the base rates of both indicators fall; the figure does not show whether the excess relative to the benchmark rate is larger in the extreme tail.

## A.4 Jacob–Levitt joint-tail diagnostics

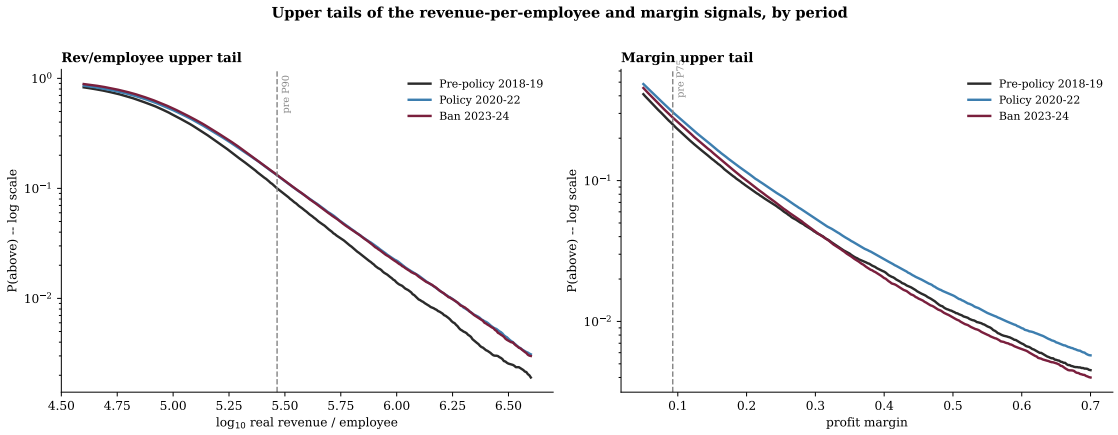



Figure 21: Upper tails of revenue per employee and the margin by period: revenue per employee moves substantially, the margin far less.

## A.5 The Plackett copula-null benchmark

The joint-tail excess compares the observed co-occurrence rate of the two signals against a benchmark that holds their pre-policy *dependence* fixed while letting each signal's marginal frequency move with the period. Let $p_1$ and $p_2$ be the period marginals of $S_1$ (revenue-per-employee indicator) and $S_2$ (margin indicator), and let $\theta$ be the Plackett odds ratio,

estimated once on the pre-policy (2018–19) cross-tabulation as $\hat{\theta} = ad/bc$ over the cells both / $S_1$-only / $S_2$-only / neither ($\hat{\theta} = 0.57$). The predicted joint rate $C = \Pr(S_1 \cap S_2)$ solves the Plackett relation

$$\theta = \frac{C\,(1 - p_1 - p_2 + C)}{(p_1 - C)(p_2 - C)},$$

a quadratic $(\theta - 1)\,C^2 - \big[1 + (\theta - 1)(p_1 + p_2)\big]C + \theta\,p_1 p_2 = 0$; we take the root inside the Fréchet bounds $\max(0,\, p_1 + p_2 - 1) \le C \le \min(p_1, p_2)$. At $\theta = 1$ the quadratic degenerates and $C = p_1 p_2$ (independence). The reported excess is the observed joint rate minus this $C$, evaluated for each period and, in Table 5, Panel B, each cohort, with $\theta$ held at its pre-policy value, so it isolates movement beyond the historical dependence rather than the dependence itself. Table 9 makes the object concrete: it lays out the $2 \times 2$ contingency of the two signals pre-policy and in 2023–24, showing the both-high cell grow and the odds ratio rise above one.

Table 9: Co-occurrence of the two detection signals: pre-policy versus 2023–24

| | Pre-policy (2018–19) | | 2023–24 | |
|---|---|---|---|---|
| % of firm-years | $S_2$ high | $S_2$ low | $S_2$ high | $S_2$ low |
| $S_1$ high | **1.66** | 8.34 | **3.98** | 8.30 |
| $S_1$ low | 23.34 | 66.66 | 21.82 | 65.90 |
| Odds ratio $\hat{\theta} = ad/bc$ | 0.57 | | 1.45 | |

*Notes:* Each panel is the $2 \times 2$ table of the two detection signals as a share of all firm-years. $S_1$ flags revenue per employee above the pre-policy P90; $S_2$ flags the profit margin above the pre-policy P75. Label the cells $a$ (both high, top-left), $b$ ($S_1$ only), $c$ ($S_2$ only), $d$ (neither). The joint tail is cell $a$: it more than doubles, from 1.66% pre-policy to 3.98% in 2023–24, while the two one-signal-only cells ($b$, $c$) barely move. The odds ratio $\hat{\theta} = ad/bc$ measures whether the two signals travel *together*: $\hat{\theta} < 1$ means a firm high on one tends *not* to be high on the other, $\hat{\theta} > 1$ means the two coincide more often than chance. It rises from 0.57 pre-policy—the signals mildly avoid each other, as ordinary revenue variation (which raises one signal and lowers the other) implies—to 1.45 in 2023–24, where they now coincide. That reversal, not the level of either signal, is the fingerprint; the copula-null benchmark isolates it period by period by holding the pre-policy $\hat{\theta}$ fixed (Appendix A.5, Figure 7). Pre-policy marginals are 10% and 25% by construction. Source: canonical full-sample Jacob–Levitt build (`jl_joint_tail_fullsample.xlsx`).

## A.6 Bank-exposure design: additional results

The provincial branch network is highly persistent across pre-policy years: bank-branch density in 2015 and in 2019 rank-correlate at $\rho = 0.98$ (Spearman) across provinces, so the measure is insensitive to the choice of pre-policy base year. We use 2015 because it is furthest from any anticipation of the 2020 policy; branch presence nonetheless remains an imperfect proxy for the capacity to absorb transferred credits. At the twenty-region level of the ENEA reports, predetermined banking capacity is weakly and imprecisely associated with realized Superbonus intensity once normalized per building (correlation 0.19).

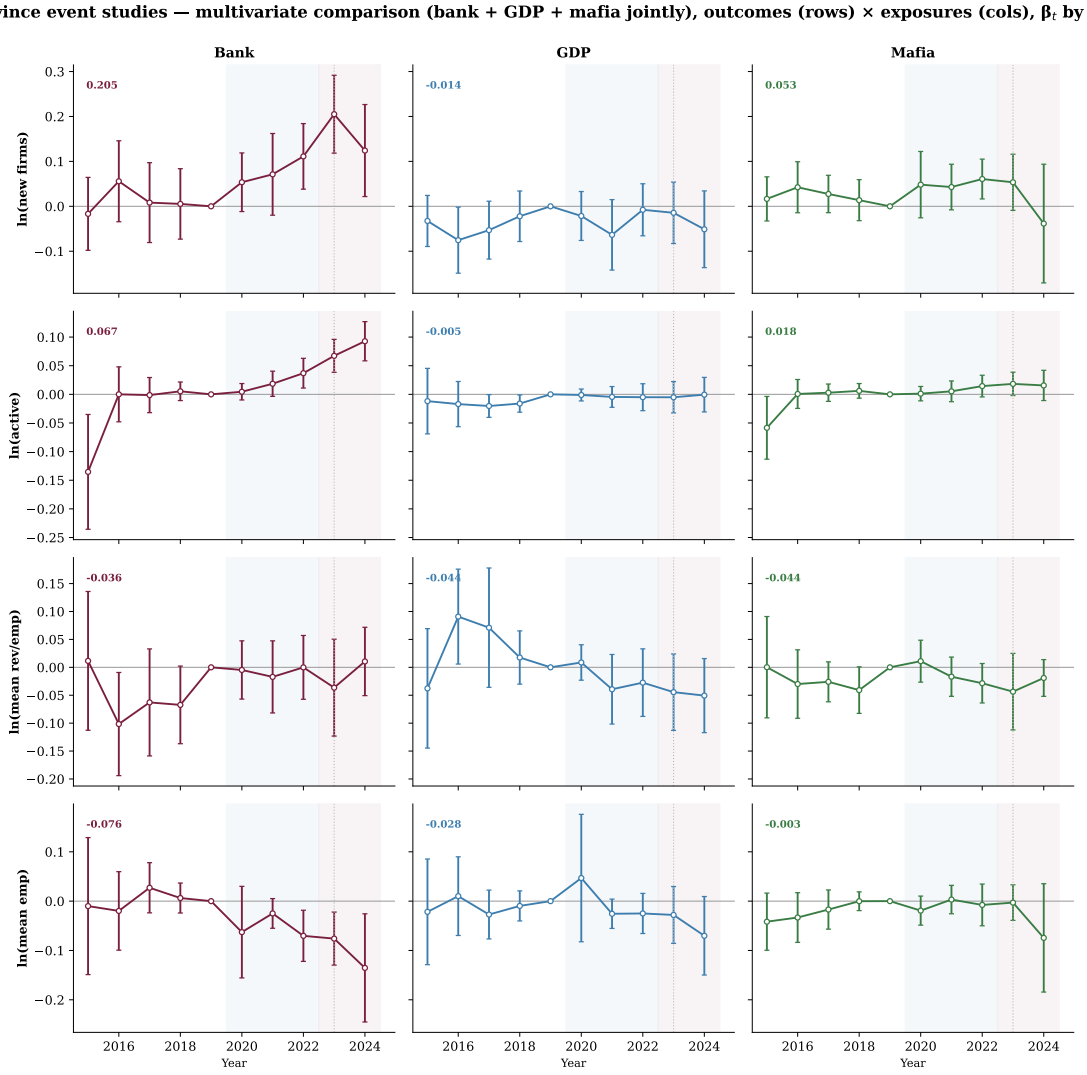



Figure 22: Bank-exposure event studies, joint multivariate comparison: four outcomes (rows) × three exposures (columns), $\beta_t$ by year (2019 reference, 95% CI). Bank exposure is the only column that trends up for new-firm formation ($\beta_{2023} = 0.205$) and active-firm counts; GDP per capita and the Mafia index are flat throughout.

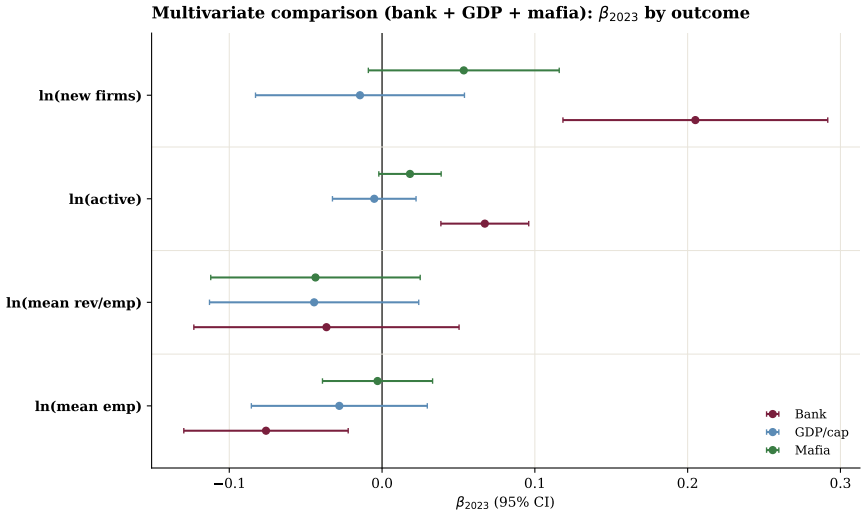



Figure 23: Multivariate provincial-exposure comparison: the 2023 coefficient on each exposure (bank branches, GDP per capita, Mafia index) by outcome, with 95% confidence intervals. Only bank exposure predicts new-firm formation; GDP per capita and the Mafia index are indistinguishable from zero.

## A.7 Firm survival and exit

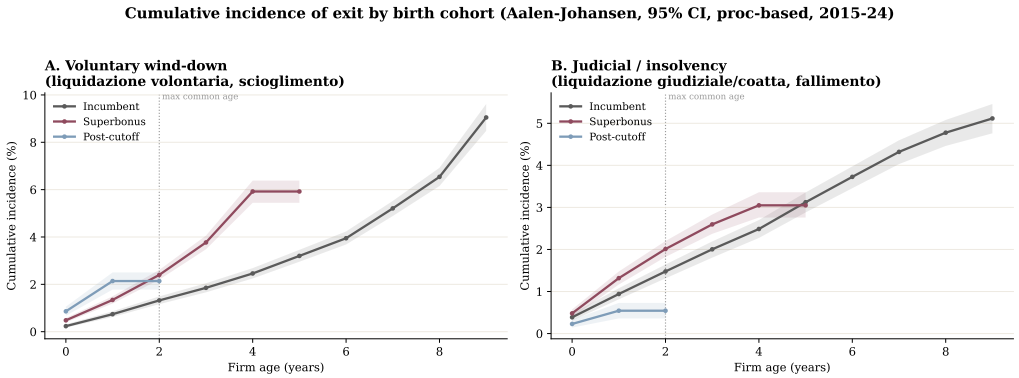



Figure 24: Aalen–Johansen cumulative incidence of exit by incorporation cohort and cause (competing risks; 95% firm-cluster bootstrap intervals). Incumbent = 2015–17 births, Superbonus = 2020–22, Post-cutoff = incorporated on/after 17 February 2023; the dotted line marks firm age 2, the oldest age at which all three cohorts are observed.

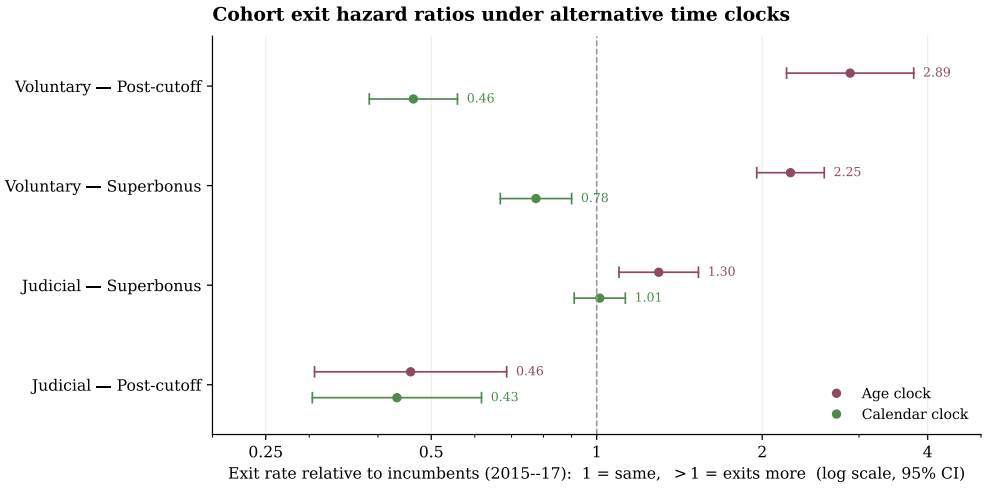



Figure 25: Cohort exit hazard ratios relative to incumbents (2015–17 births) on two time scales: firm age (time since the firm's own incorporation, not since the policy) and calendar time. Each row is one exit route × cohort; points are hazard ratios with 95% confidence intervals (Wald on the log hazard ratio, province-clustered), ordered by the firm-age ratio.

## A.8 Low-headcount indicator

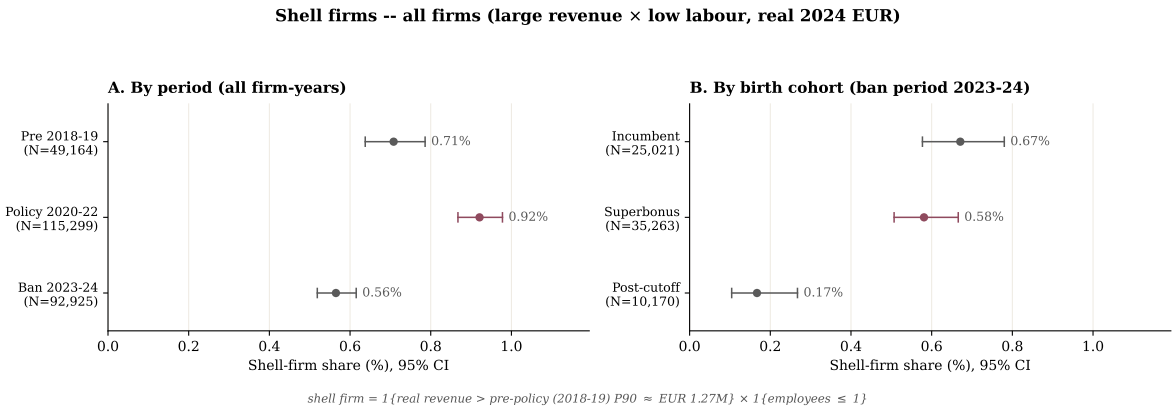



Figure 26: Share of firm-years flagged by the low-headcount indicator, by period (all firm-years) and by incorporation cohort (restricted to 2023–24, so all three cohorts are observed in the same calendar years, though at different ages). The indicator flags a firm-year with real revenue above the pre-policy P90 and at most one employee; Wilson 95% intervals.

## A.9 Robustness: dropping zero-employee firms

The detection and banking results do not hinge on how zero-employee firm-years are treated. The main specification imputes max(employees, 1) and so retains them; the positive-employee robustness instead drops them, cutting the sample from 286,971 to 264,168 firm-years. Table 4 and Figure 27 re-run the full battery both ways: the tail thickens under the policy, the 2023–24 odds ratio exceeds one, the post-cutoff cohort stays lowest, and the bank-exposure interaction is similar, with magnitudes close across the two. The full side-by-side comparison is in Table 4 in the main text; Figure 27 plots the same battery.

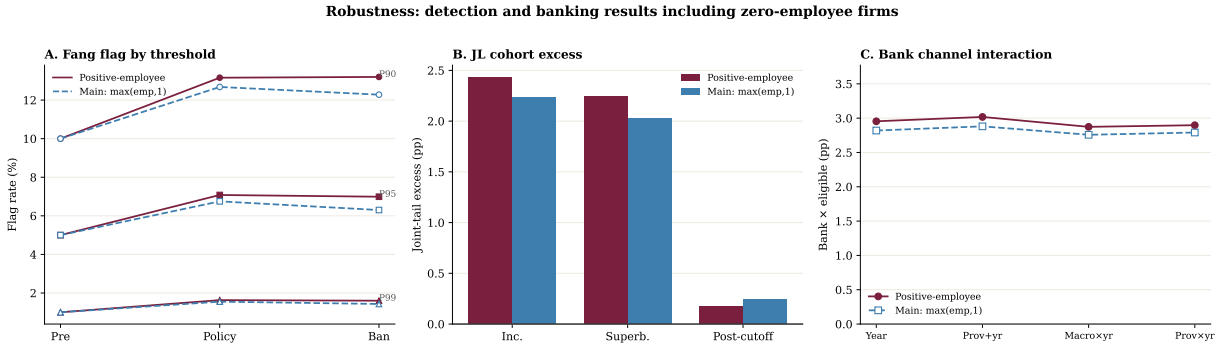



Figure 27: Full sample versus positive-employee subsample. Revenue-per-employee indicator rates by threshold (Panel A), Jacob–Levitt 2023–24 cohort excess (Panel B), and the bank-exposure × eligibility interaction across the fixed-effects ladder (Panel C), each computed on the full sample (all firms) and on the positive-employee subsample that drops zero-headcount firms. The two track each other throughout.