Does Internal Finance Matter? Evidence from Severance Payment Reforms and Italian SMEs

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Abstract

This paper identifies the causal effect of internal finance on firm investment and financial structure by exploiting a large and plausibly exogenous cash flow shock to Italian small and medium-sized enterprises (SMEs). The shock originates from pension reforms mandating the outflow of the Trattamento di Fine Rapporto (TFR), a severance payment scheme historically retained on firms' balance sheets and used as a source of internal funding. A size-contingent provision required firms with more than 50 employees—based on predetermined employment—to divert newly accrued TFR flows to external funds, reducing cash flows by about 6.3 percent of the annual wage bill. Using a Difference-in-Regression-Discontinuities design with administrative firm-level data, we find that firms do not substitute lost internal funds with external finance. Instead, compared to firms just below the threshold, treated firms experience a decline of both long-term bank credit and fixed assets, particularly tangible capital. The effects are stronger for financially vulnerable firms, consistent with internal and external finance acting as complements rather than substitutes for credit-constrained SMEs.

JEL: G31, G32, H32, L25

Keywords: internal finance, severance payments, investment, liquidity shocks

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1 Introduction

Severance payments (SPs) are employer-managed funds of deferred compensation that accrue during employment and are paid out upon job separation. Beyond their insurance role for workers, SPs have also played a central—yet largely overlooked—function in corporate finance. In countries such as Italy, Korea, and Austria, SPs have traditionally been retained on firms' balance sheets, effectively operating as a stable, low-cost source of internal finance, especially for SMEs facing high external borrowing costs (Holzmann et al., 2011; Guiso, 2003). Over the last two decades, demographic pressures on public pension systems prompted a wave of reforms that allowed workers to choose whether to redirect severance accruals from firms toward funded supplementary pension schemes.[1] Understanding how firms respond when this internal liquidity channel is curtailed holds substantial policy relevance—particularly in Italy, where severance savings have remained at the center of repeated legislative interventions.[2] Yet, causal evidence on these effects remains absent. This study fills this gap by exploiting quasi-experimental variation generated by a major Italian reform, delivering the first causal estimates of how severance payments' liquidity affects firms' investment and financing decisions.

While prior studies have documented workers' allocation choices (Fornero and Monticone, 2011; Corsini et al., 2012) or simulated firm responses (Bardazzi et al., 2005; Calcagno et al., 2011), estimating the causal effects of SP reallocations has proven empirically challenging. On top of limited micro-level information on severance allocations, observed outflows from firms' balance sheets may suffer from endogeneity concerns: financially constrained firms may discourage workers from joining pension funds by withholding information or exerting pressure (Boeri and Zingales, 2008; Cesaratto, 2011). We overcome these issues by exploiting a sharp discontinuity introduced by Italy's 2007 reform of the *Trattamento di Fine Rapporto* (TFR), the Italian severance payment scheme.[3] The 2005 legislative framework established automatic enrollment of new TFR flows into pension funds, beginning in 2007, unless workers explicitly chose to keep it with the employer.[4] A sub-

[1] Despite incentivized or quasi-automatic enrollment, reforms in Austria (2003), Italy (2007), and Korea (2005) exhibit persistently low rollover into funded plans, suggesting that liquidity at job loss remains highly valued or that frictions—such as limited trust in financial institutions or employer influence over workers' choices—impede the outflow of severance wealth from firms.

[2] The 2026 Budget Law (L. 199/2025) revisits and extends the size-based mandate introduced in 2007 by progressively lowering the employment threshold above which firms are required to divert new severance accruals to external funds.

[3] Severance arrangements vary significantly across jurisdictions. Following the classification in Holzmann et al. (2011) and Gerard and Naritomi (2021), we distinguish between *severance payments* (SP) and *severance saving accounts* (SSA). While SPs are typically lump-sum indemnities contingent on involuntary dismissal, SSAs are individualized accounts funded by periodic wage deferrals and accessible upon any job separation. Structurally, TFR is an SSA, as it accumulates through monthly contributions and is paid regardless of the reason for termination. However, unlike the funded SSA models common in Latin America, the TFR is traditionally managed through *book reserves*, remaining on the employer's balance sheet until disbursement. This unique configuration—an unfunded SSA held as book reserves—combines the firing cost properties of traditional severance payments with a source of internal financing for firms. For consistency with the broader labor literature, we refer to these arrangements as *severance payments* throughout the paper.

[4] Under Law 252/2005, enacted in December 2005, workers had six months from the policy start date—or from the hiring date, for new entrants—to choose whether to keep new TFR flows with the employer.

sequent and unanticipated mandate (Law 296/2006) required all firms with more than 50 employees—measured using average 2006 workforce—to transfer all new TFR accruals that workers chose to leave with the employer to a state-administered fund (*INPS Fondo di Tesoreria*). Announced in late October 2006 and implemented in January 2007, this requirement deprived affected firms of a low-cost source of internal financing, roughly amounting to 6.3 percent[5] of the annual wage bill net of government compensation measures, with limited scope for anticipation or manipulation. By leveraging this unique policy discontinuity, administrative data for the universe of private-sector firms and a Difference-in-Regression-Discontinuities (DiRD) design, this paper provides the first causal estimates on the corporate-finance implications of the reallocation of severance accruals.

To study how Italian SMEs—who rely heavily on internal cash resources for financing (Berger and Udell, 1998; Guiso, 2003)—adjusted to this sharp and unanticipated cash flow shock, we leverage administrative data linking private-sector firms' balance sheets from CERVED, employment records from INPS and firm–bank credit exposures from the Italian Credit Registry. This integrated panel allows us to trace how the loss of a long-standing, low-cost source of internal finance propagates through firms' liquidity management, borrowing behavior and real-side decisions. We estimate a Difference-in-Regression-Discontinuities (DiRD) design (Grembi et al., 2016; Picchetti et al., 2024) around the pre-reform 50-employee threshold. The DiRD specifications compare firms just above and below the cutoff before and after the mandate that diverted new TFR flows away from employers, identifying the local effect of losing access to this cheap and liquid liability (Takahashi, 2024). The running variable is firm employment in 2006, centered at 50. We control for firm fixed effects, region-by-year and sector-by-year fixed effects and allow for flexible, year-specific functions of the running variable. Identification rests on local parallel trends and the non-manipulability of the size cutoff; any pre-existing size-50 regulations are differenced out, provided their effects are time-invariant.

Our results reveal a clear sequence of adjustments consistent with a binding and persistent cash flow shock. We document a large and steadily widening depletion of severance payment accounts for treated firms; by 2010, the cumulative shortfall in TFR stocks reaches approximately €150,000, representing an overall 28.6% decline relative to counterfactual levels. This erosion maps into tighter operating budgets, with cash flows declining by approximately €50,000 per year. Firms are unable to offset this shortfall through external borrowing. In the first year of the reform, firms experience a decline in liquid assets compared to firms just below the threshold, with no increase in short-term bank loans, trade payables, or non-bank financing. From 2008 onward, as buffers are depleted, firms are characterized by a lower level of both fixed assets and long-term bank debt. By 2010, the cumulative impact of the TFR-liquidity drain results in a substantial contraction of the firm's balance sheet: long-term bank debt exhibits a cumulative decline of 12% (€120,000) compared to the control group, while fixed assets fall by 7% (€220,000), a retrenchment almost entirely driven by a 6% (€170,000) reduction in tangible assets.

[5]This number reflects post-reform reductions in social-security contributions and corporate-tax deductions associated with the transfer of TFR accruals to external funds.

One possible interpretation of these patterns is provided by models with limited pledgeability (Holmstrom and Tirole, 1997; Calcagno et al., 2011), in which internal and external finance are complements rather than substitutes. When banks finance only part of investment and firms must cover the remaining share with internal or unsecured funds, diverting TFR flows reduces firms' ability to co-finance investment alongside banks, lowering the feasible scale of investment and, consequently, bank borrowing. Lower investment also slows capital accumulation, reducing pledgeable collateral and further tightening borrowing capacity (Guiso, 2003; Almeida and Campello, 2007).

While our point estimates across subsamples are not always statistically distinguishable, the aggregate pattern provides suggestive evidence that the reform's impact scales systematically with pre-reform firm characteristics. Specifically, the contraction in investment and long-term credit is substantially more pronounced among financially constrained firms, proxied in our baseline specification by low liquidity-to-asset ratios, consistent with severance-based liquidity acting as a critical buffer when external financing is costly. This real response is further amplified by supply-side frictions and production technology. Firms borrowing from interbank-dependent lenders, which were more exposed to the post-2007 wholesale funding freeze (Cingano et al., 2016), exhibit larger retrenchments, suggesting that credit-supply shocks compounded the loss of internal funds. Finally, the effects are most acute among firms with high capital-to-labor ratios, where the cash drain appears to have significantly raised the cost of maintaining a capital-intensive production structure (Adamopoulou et al., 2021).

Our findings have direct implications for the long-standing debate on investment–cash flow sensitivities. Since Fazzari et al. (1987) high investment–cash flow sensitivities have been taken as evidence of financing constraints, yet this interpretation has been repeatedly challenged (Kaplan and Zingales, 1997). A central concern is that cash flow may proxy for firms' investment opportunities or expected profitability, making it difficult to isolate the causal effect of internal finance on investment. By exploiting a reform-induced cash flow shock that is orthogonal to firms' investment opportunities within a DiRD framework, we address these endogeneity and measurement concerns (Erickson and Whited, 2000; Gomes, 2001) and improve on quasi-experimental pension-funding designs, where treated and control firms are often imperfectly comparable (Rauh, 2006; Bakke and Whited, 2012). This study aligns with the financing-constraints view: firms that were more liquidity-constrained ex ante exhibit the strongest investment responses to cash-flow shocks, consistent with Italian SMEs historically characterized by chronic credit frictions and heavy reliance on internal liquidity (Guiso, 2003). Moreover, by treating deferred compensation as an overlooked financing margin, our results complement evidence that shocks to financing sources propagate via firm balance sheets to real outcomes (Chodorow-Reich, 2014; Berton et al., 2018; Adamopoulou et al., 2021). Lastly, the work speaks directly to the design of severance and pension systems in economies considering similar reforms. Redirecting severance flows toward funded pension schemes is intended to strengthen retirement provision, but it simultaneously deprives firms—especially liquidity-constrained SMEs—of a

stable, low-cost source of internal finance. We provide the first causal evidence on these mechanisms.

The remainder of the paper is organized as follows. Section 2 describes the institutional background and the policy change that generated the sharp discontinuity in access to severance-based liquidity. Section 3 presents the data and empirical design, documenting pre-reform comparability between treated and control firms and the absence of manipulation around the size threshold. Section 4 reports the core DiRD estimates and explores heterogeneity to shed light on the underlying mechanisms. Section 5 conducts validity and robustness checks, including placebo tests, bandwidth sensitivity and graphical RDD evidence, confirming that the effects are uniquely driven by the 2007 reform. Finally, Section 6 concludes.

2 Institutional Background

The Italian *Trattamento di Fine Rapporto (TFR)* is a form of mandatory deferred compensation requiring employers to set aside almost 7% of each employee's monthly wage, equivalent to one month of gross salary per year, to be returned upon job separation for *any reason*.[6] Workers may access part of their TFR before separation only under narrow conditions, and subject to stringent quantitative limits.[7]

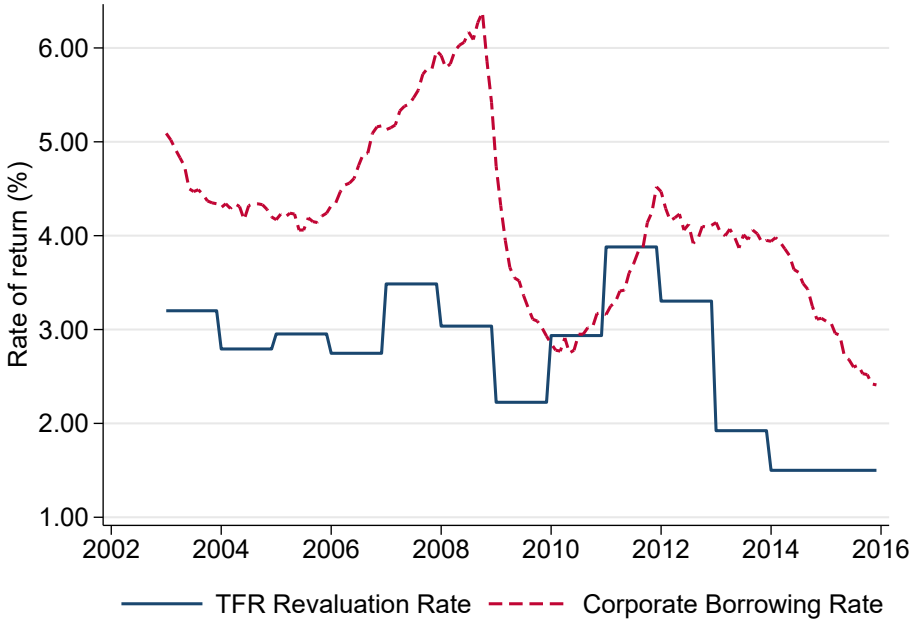
Balances are revalued annually at a statutory rate equal to 1.5 percent plus 75 percent of inflation. Because this rate has historically remained below average bank lending rates for non-financial firms (Figure 1), TFR has long served as a low-cost source of internal finance (Bardazzi et al., 2005; Calcagno et al., 2011; Corsini et al., 2012; Guiso, 2003), a role particularly valuable for SMEs that rely heavily on bank loans. For firms that depend on external financing to fund marginal investment, retaining TFR funds effectively lowers the marginal cost of capital, as the implicit return credited to workers is typically below the cost of market borrowing. For financially unconstrained firms, instead, the value of retaining TFR depends on alternative investment opportunities and the expected duration of this implicit loan from employees. The statutory return has generally fallen between short-term deposit rates and medium- to long-term government bond yields, placing it between highly liquid and longer-horizon safe assets. In addition, the fixed component of the revaluation formula limits exposure to fluctuations in market interest rates, making TFR balances a low-volatility funding source and a stable liquidity buffer against temporary shocks. Moreover, outstanding TFR claims are guaranteed in case of firms' insolvency by a public fund, the *INPS Fondo di Garanzia*.

Concerns over the long-run sustainability of Italy's public pension system prompted a

[6] This unconditional payout feature is uncommon internationally. In most severance systems, benefits are triggered only upon involuntary separation.

[7] Employees with at least eight years of tenure may request an advance (*anticipazione*) of up to 70% of their accrued balance, typically for medical expenses, the purchase or renovation of a primary residence, or—where permitted by collective agreements—parental or training needs. Employers may grant advances to no more than 10% of eligible workers and to at most 4% of their total workforce per year unless sectoral CBAs specify otherwise.

Figure 1: TFR Revaluation Rate vs Corporate Borrowing Rate (2003–2015)
Source: ECB – ISTAT.
The figure plots the statutory TFR revaluation rate, equal to 1.5% plus 75% of annual inflation, against the average cost of bank borrowing for Italian non-financial corporations.



sequence of reforms in the early 2000s encouraging the reallocation of private-sector workers' TFR flows from employers to complementary pension schemes, with the aim of improving retirement adequacy. A key step to promote the development of second-pillar pension schemes[8] was the enactment of Law 252/2005 in December 2005, which introduced an automatic enrollment mechanism: beginning in 2008, new TFR flows would be irrevocably redirected to closed pension funds[9] with a guaranteed investment profile[10] unless workers explicitly opted to keep their TFR within the firm or chose an alternative fund or investment line. The 2007 Budget Law (L. 296/2006) introduced an additional and unexpected provision that created a sharp and plausibly exogenous policy discontinuity: all private-sector firms with more than 50 employees—defined by their average workforce in 2006 for active firms[11]—were required to transfer to a state-administered Treasury Fund (*INPS Fondo di Tesoreria*) all new TFR accruals belonging to workers who had explicitly chosen to keep their severance within the firm. Furthermore, the policy anticipated the implementation of Law 252/2005 by one year, with mandatory transfers for above-threshold firms taking

[8]Despite repeated policy interventions, Italy's second-pillar pension system remains underdeveloped. Assets held by Complementary Social Security (CSS) schemes increased from 3% of GDP in 2006 to 3.5% by 2008 (Corsini et al., 2012), gradually reaching 8.5% by 2023 (COVIP, 2024). Nonetheless, the system continues to lag behind international benchmarks and structural reforms are widely viewed as necessary in light of growing demographic and fiscal pressures.

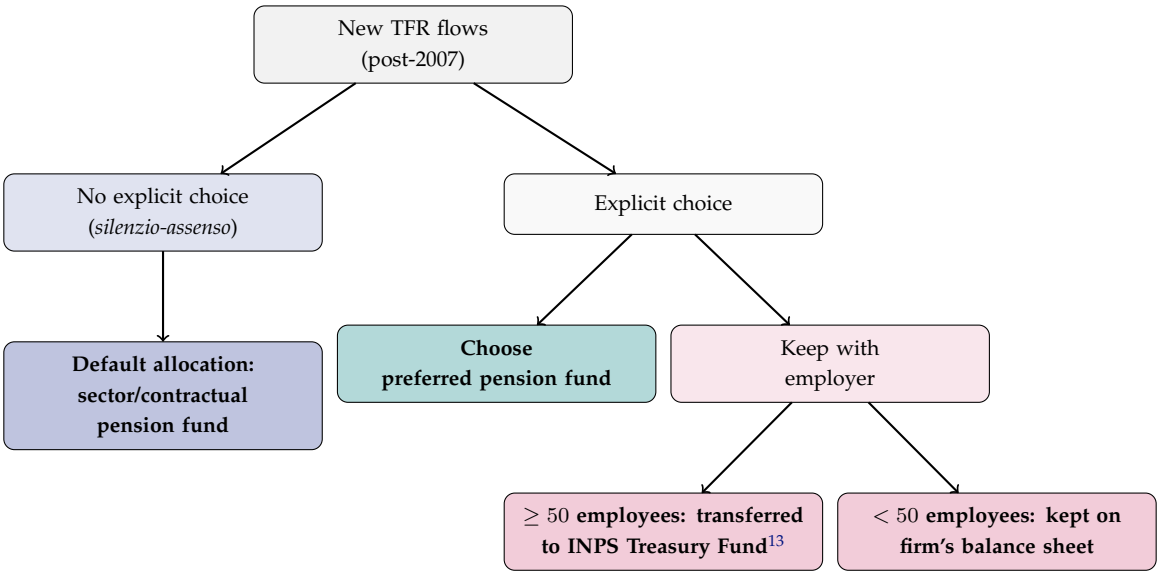
[9]Closed funds are industry-level schemes established under collective bargaining agreements or professional associations; open funds correspond to standard pension funds offered by financial institutions.

[10]These funds guarantee preservation of initial capital and a return close to the statutory revaluation applied to TFR retained within firms.

[11]For newly established firms, the relevant size threshold is measured at the end of the first year of activity.

effect on January 1, 2007. Since the threshold was based on pre-reform 2006 averages and announced with minimal advance notice, active firms had limited scope for manipulation,[12] enhancing the credibility of the quasi-experimental setting. Notably, despite targeted fiscal offsets intended to ease the transition, SMEs strongly opposed the reform (COVIP, 2008), underscoring the importance of TFR as a source of internal finance.

Figure 2: Allocation of New TFR Flows After the 2007 Severance Reform



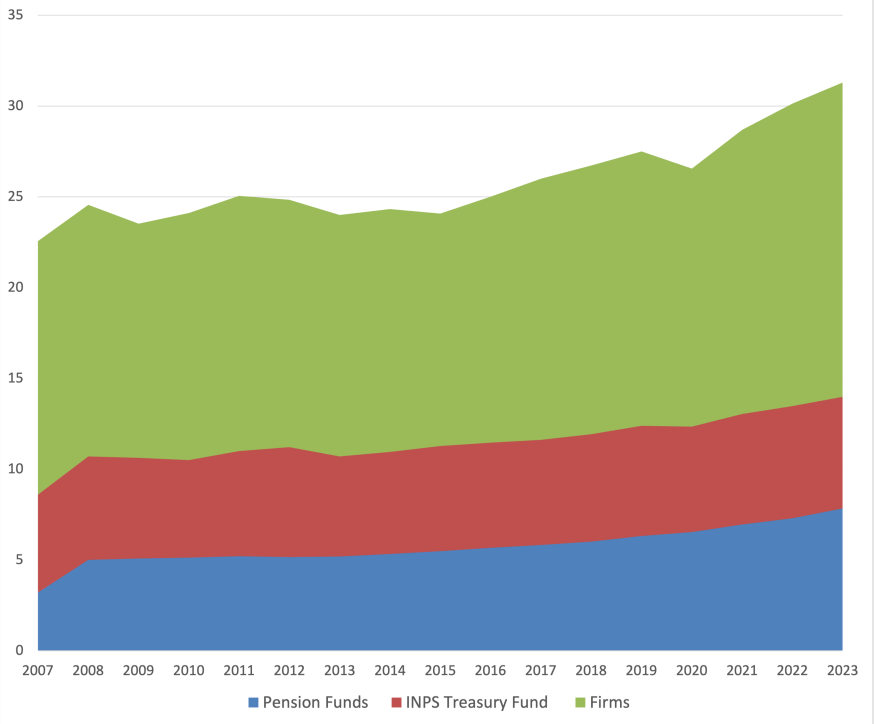
Participation in supplementary pension schemes remained strikingly limited after the reform. Although enrolment in complementary pension funds rose from 16% to 25% between 2006 and 2007, it stalled immediately thereafter and remained highly skewed toward larger firms: only 12% of workers in firms with fewer than 50 employees joined a pension fund, compared with 42% in medium and large firms. Such patterns are mirrored in the aggregate distribution of TFR flows, which has remained remarkably stable: since 2007, roughly 55 percent of new TFR has continued to be retained within firms, with the remainder split between complementary pension funds and the *INPS Fondo di Tesoreria* (COVIP, 2024). Figure 3 illustrates this persistent distribution.[14]

[12] A firm with 52 employees in October would have needed to fall to about 39 workers in both November and December to bring the year-average below 50; even a firm with 51 employees would have needed to drop to around 44. These reductions—13 and 7 dismissals in two months—were infeasible, as more than five dismissals within 120 days trigger the collective dismissal procedure (L. 223/1991), requiring union consultations and multiweek administrative steps.

[13] The INPS Treasury Fund *(Fondo di Tesoreria)* receives TFR flows from firms with at least 50 employees in 2006.

[14] The lower 2007 flow to pension funds reflects the reform's timing: "silent" workers were enrolled only on June 30, so TFR accrued in the first half of the year remained with firms.

Figure 3: Annual TFR Flows by Destination, 2007–2023 (Billions of Euros)
Source: COVIP.
The figure plot the allocation of new flows of TFR in pension funds, the INPS Treasury Fund, and employer balance sheets after the 2007 TFR reform.



The decision to retain TFR within the firm or redirect it to a pension fund reflects not only statutory differences in expected returns, tax treatment, employer matches, management fees, withdrawal rules, and liquidity at separation and retirement, but also deeper behavioral and informational forces—including risk aversion, limited trust in financial institutions, and employer influence over workers' choices. Supplementary pension funds offer substantially more generous fiscal incentives[15] and, in most contractual funds, workers who contribute at least 1–1.2% of gross pay receive an employer match of about 2%—a sizeable transfer absent under the TFR system. Expected returns also favour pension funds, though net gains depend on management fees (Cesari et al., 2008).[16] Advances for medical expenses or housing are available under both systems, although pension funds often allow more flexible withdrawal conditions. Liquidity considerations partly offset the financial advantages of pension funds: while TFR held within the firm is fully liquid at separation, pension funds historically allowed only partial withdrawals conditional on prolonged

[15] Employee contributions are deductible up to €5,164.57 per year; fund returns face a lower tax rate; and retirement withdrawals are taxed at 15%, declining by 0.3 p.p. per year after 15 years to a minimum of 9%—far below the 23–43% IRPEF rates applied to TFR lump-sum payments.

[16] Average annual fees are about 0.6% in closed funds and exceed 1% in many open funds. Cesari et al. (2008) show that, over a 30-year horizon, pension funds generally outperform employer-retained TFR even after accounting for fees and taxes. With a 23% income-tax rate, the net advantage of reallocating TFR to a pension fund ranges from $+1.7\%$ in the most conservative guaranteed portfolio ("Tacit"), to $+28\%$ in a standard occupational fund ("Mini"), and up to $+49.8\%$ when workers contribute enough to obtain the employer match ("Maxi"). Even open funds with higher fees yield $+16.8\%$ relative to TFR. Under a higher income-tax rate (33%), these gains widen markedly—to $+38.5\%$, $+28.6\%$, and $+64.7\%$ for the Mini, Open, and Maxi options, respectively.

unemployment (until reforms in 2016), although full withdrawals were permitted when workers no longer met participation requirements, such as after changing sector.

Despite these financial advantages, take-up remained low. Survey evidence suggests that the strong preference for retaining TFR within the firm may be driven by risk aversion, a preference for lump-sum liquidity, and limited awareness of the fiscal benefits of pension funds (Cozzolino et al., 2006). Additional survey evidence indicates that some workers continued to retain TFR within the firm "to help the employer", particularly in small firms, sometimes responding to perceived or explicit pressure or fearing dismissal if they switched (Boeri and Zingales, 2008). Corsini et al. (2012) also highlight the role of strategic interactions. Because TFR represents a source of low-cost internal finance, workers in small firms may internalize the risk that widespread switching to pension funds would weaken the firm's financial position and increase insolvency risk, thereby reducing participation.

From the firm's perspective, Calcagno et al. (2011) characterize TFR as subsidized, state-insured long-term debt with an implicit interest rate below market benchmarks, and predict that removing this source of financing may reduce borrowing capacity and investment. At the aggregate level, the reform diverted more than €10 billion per year away from firms' balance sheets (Figure 3), with only limited fiscal offsets. Before the 2007 reform, employers retained 7.41% of each worker's gross wage as TFR liabilities; after paying a 0.5% levy (0.2% to the Guarantee Fund and 0.3% in social-security contributions), roughly 6.91% of each wage euro remained within the firm as liquid resources. Under the new rules, firms required to transfer TFR flows outside the balance sheet received rebates totaling 0.48% of gross wages—the elimination of the 0.2% levy and a reduction in social-security contributions of up to 0.28%[17]—plus a corporate-tax deduction equal to 4% of the transferred quota (6% for firms below the 50-employee threshold). Overall, these measures replaced only a small fraction of the lost internal funding, resulting in a net cash-flow loss of about 6.3 cents per wage euro.

3 Empirical Analysis

3.1 Data

The empirical analysis primarily draws on CERVED, a longitudinal registry covering the universe of incorporated firms and reporting detailed end-of-the-year annual balance-sheet information. Our dataset focuses on non-financial corporations and relies on additional administrative sources to obtain a comprehensive panel suitable for analysing firms' real and financial responses to the loss of severance-based internal finance. First, we use firm-level information drawn from the *INPS Universo Imprese* archive, which provides average annual headcounts and total annual gross wages by workers' type, as well as sector and geographic location for all private non-farm firms filing social-security returns. Second, we incorporate detailed firm-level indicators constructed from bank–firm credit exposures in the Bank of Italy's Central Credit Register (CR), which covers all credit relationships above

[17] The reduction starts at 0.19% and rises to 0.28% after 2014.

€75,000 (reduced to €30,000 after 2009) for every bank operating in Italy.[18] The integration of these sources yields a rich panel spanning 2004 to 2010 that jointly captures firms' balance sheets, labor inputs, and access to external finance.

From balance-sheet data (CERVED), we construct standard measures of firm inputs, liquidity and operating performance. Fixed assets are defined as the sum of tangible (property, plant, and equipment) and intangible assets, while liquid assets are measured as the sum of cash and deposits plus short-term financial assets. Beyond studying input reallocation through fixed assets and labor costs, we analyze firms' responses along the working-capital margin by leveraging information on inventory, accounts receivable, and accounts payable. These variables capture short-run adjustments in internal liquidity management and trade-credit policies, which may serve as partial substitutes for external financing following the reform.

On the financing side, we use the Italian Credit Register (Centrale dei Rischi, CR), which provides high-quality granular information on firm–bank relationships. The data distinguish between short-term and long-term exposures,[19] as well as between bank and non-bank lending instruments (including leasing and factoring). This allows us to separately analyze long-term bank debt, short-term bank debt, and non-bank debt. Furthermore, the CR data report both granted and utilized credit. We focus on actual drawdowns when studying whether firms actively substitute away from internal liquidity toward external finance following the TFR-induced cash drain. We also leverage the CR to construct firm-level predetermined exposure to the post-2007 tightening in interbank funding induced by the global financial crisis. Following Adamopoulou et al. (2021), we first compute, for each bank, the ratio of interbank funding (deposits plus repurchase agreements) to total intermediated funds using Supervisory Reports, averaged over 2004–2006. We then measure each firm's ex ante exposure to funding shocks by calculating the 2006 share of granted credit received from each lending bank. A firm's vulnerability to a contraction in bank credit supply is defined as the credit-weighted average of the interbank funding ratio across its 2006 lenders.

A central requirement for identification is an accurate measure of firm size, as eligibility for the 2007 TFR reform depends on whether a firm exceeded 50 full-time-equivalent (FTE) employees in 2006. Legally, INPS Circular 70/2007 requires that all subordinate workers be counted toward the threshold, with part-time and intermittent contracts weighted pro rata based on hours worked and unpaid workers included only if formally replaced. The relevant eligibility criterion is therefore based on FTE employment. Our main firm-level employment source—the *INPS Universo Imprese* archive accessed at the Bank of Italy—reports annual headcounts rather than FTEs. This may introduce misclassification near the cutoff if contract intensity differs systematically around the threshold.

To assess the magnitude of this issue, we use INPS monthly firm–worker histories and data on actual TFR transfers to the INPS Treasury Fund (*Fondo di Tesoreria*), accessed through

[18] The threshold does not apply to positions in default, for which any amount has to be reported.

[19] Specifically, we classify credit lines, overdrafts, and self-liquidating credits as short-term exposures.

a separate worker-side project.[20] These data allow us to reconstruct the legally relevant FTE measure in 2006 and to observe both the "1R" authorization code, which identifies firms required[21] to transfer post-2007 TFR flows to the INPS Treasury Fund, and actual TFR transfers for newly hired workers over the period 2011–2019.

Evidence from these microdata indicates that headcounts from the Bank of Italy archives provide a reliable proxy for treatment status around the policy threshold. First, within a narrow window around 50 employees, approximately 90 percent of contracts in 2004–2006 are full-time, implying minimal divergence between headcounts and FTE-based employment within the bandwidth used in our baseline specifications. This is consistent with the 94 percent full-time share documented by Fenizia et al. (2024) among small Italian firms. Second, FTE-based size classification strongly predicts effective treatment exposure as measured both by the INPS "1R" code[22] and by observed TFR transfers to the INPS Treasury Fund, with discrepancies concentrated among firms extremely close to the threshold, specifically those within two employees of the 50-worker cutoff.

These findings imply that our headcount-based classification closely approximates FTE-based eligibility and, through it, actual treatment status. To further attenuate any residual misclassification, all baseline specifications exclude firms within two employees around the 50-employee threshold where measurement noise is most likely, implementing a ± 2-worker donut around the cutoff.

3.2 Difference-in-Regression Discontinuities

Estimating the causal effect of diverting TFR flows to external accounts is empirically challenging. In countries where severance payments can be redirected to pension funds—such as Italy, Austria, and Korea—firms lose access to these funds based on workers' allocation decisions. As a result, observed severance outflows may be endogenous: financially constrained firms may discourage pension enrollment in order to retain severance-based liquidity (Boeri and Zingales, 2008; Cesaratto, 2011). In this case, changes in internal liquidity would reflect firm conditions rather than exogenous variation, making causal interpretation difficult. The 2007 Italian TFR reform provides a source of plausibly exogenous variation. The reform mandated that firms with at least 50 employees in 2006 transfer newly accrued TFR flows to external pension funds or to the INPS Treasury Fund, effectively removing access to this source of internal finance independently of contemporaneous worker enrollment decisions. The reform therefore generated a sharp discontinuity in firms' access to severance-based internal liquidity based on firm size in 2006.

Although treatment is triggered by a predetermined size threshold, firm size is not randomly assigned. Larger firms differ systematically from smaller firms, so a simple compar-

[20] The microdata are accessed under the *VisitINPS* protocol and cannot be merged with balance-sheet or credit data. We use them exclusively to reconstruct the FTE-based size measure and to validate treatment classification.

[21] While the code was expected for treated firms in the post-reform period, it became legally mandatory only from 2016 onward, making it informative but not mechanically binding in our sample years.

[22] More than 99 percent of firms below the 50-FTE threshold do not display the code, while roughly 80 percent of firms above the threshold do.

ison between firms above and below 50 employees would confound the reform effect with underlying size-related differences in outcomes. Identification therefore relies on comparing firms close to the threshold. However, a standard Regression Discontinuity Design (RDD) in levels would capture not only the effect of the TFR reform but also discontinuities generated by other regulations that apply at the same 50-employee threshold.[23] To isolate the reform's effect, we implement a Difference-in-Regression-Discontinuities (DiRD) design (Grembi et al., 2016; Picchetti et al., 2024), which differences out all time-invariant discontinuities at the threshold by comparing the jump at the 50-employee cutoff after the 2007 reform with the corresponding jump in the pre-reform period. The specification also allows the relationship between outcomes and firm size to evolve flexibly over time on each side of the threshold, so that identification comes from the change in the discontinuity rather than from smooth changes in the outcome–size relationship over time.

Let $S_{i,2006} = \text{size}_{i,2006} - 50$ denote the running variable and Y_{it} an outcome for firm i in year t, such as fixed assets or bank credit. The DiRD estimand is:

$$\tau = \left[\lim_{\varepsilon \to 0^+} E[Y_{it} \mid S_{i,2006} = 0 + \varepsilon, t \geq 2007] - \lim_{\varepsilon \to 0^-} E[Y_{it} \mid S_{i,2006} = 0 - \varepsilon, t \geq 2007]\right] -$$

$$\left[\lim_{\varepsilon \to 0^+} E[Y_{it} \mid S_{i,2006} = 0 + \varepsilon, t < 2007] - \lim_{\varepsilon \to 0^-} E[Y_{it} \mid S_{i,2006} = 0 - \varepsilon, t < 2007]\right].$$

Under standard RD assumptions of continuity of potential outcomes in the running variable $S_{i,2006}$ at the cutoff in the absence of treatment and no manipulation of the running variable around the threshold,[24] the parameter τ identifies the local average treatment effect (LATE) of the mandated outflow of severance-based internal liquidity for firms near the statutory threshold.

We estimate the DiRD using local–linear regressions within a bandwidth h around the cutoff. Our baseline static specification is:

$$\begin{aligned} Y_{it} = \lambda_i + \lambda_{rt} + \lambda_{st} + \beta \text{Post}_t \cdot A_{i,2006} \\ + \sum_{k \neq 2006} \delta_k S_{i,2006} \cdot \mathbb{1}\{t = k\} \quad + \sum_{k \neq 2006} \xi_k S_{i,2006} \cdot A_{i,2006} \cdot \mathbb{1}\{t = k\} + \epsilon_{it}, \end{aligned} \tag{1}$$

where $A_{i,2006} = \mathbb{1}\{\text{size}_{i2006} \geq 50\}$ is the treatment indicator and Post_t is a binary variable equal to one from 2007 on. Firm fixed effects λ_i absorb time-invariant firm heterogeneity, while year $\times$ region fixed effects λ_{rt} and year $\times$ 2-digit sector fixed effects λ_{st} absorb time-varying shocks at the local and industry levels. The coefficient β captures the reform-

[23] Importantly, our running variable is fixed over time and defined as average employment in 2006, whereas other regulations apply based on firm size in each year. This difference helps separate the effect of the reform from the effect of other size-based regulations, including disability hiring quotas (Law 68/1999) and thresholds for simplified financial reporting (Civil Code art. 2435-bis). Disability quotas require firms with more than 50 employees to hire workers with disabilities equal to 7% of their workforce (vs. two mandatory hires below the threshold), with penalties for noncompliance; simplified accounting is permitted only below size 50 if two of three criteria (employees, sales, assets) are not exceeded. We present supporting evidence from RD specifications in Section 5.2.

[24] Section 3.3 presents McCrary density tests and covariate balance tests around the cutoff.

induced change in the cutoff jump net of baseline policies. The interaction coefficients $\{\delta_k, \xi_k\}$ allow the relationship between outcomes and firm size to evolve flexibly over time on each side of the cutoff. Standard errors are clustered at the firm level to account for serial correlation in outcomes.

To trace the treatment path and formally test for pre-trends, we also estimate dynamic coefficients substituting the term $\beta\,\text{Post}_t \cdot A_{i,2006}$ in the baseline static specification with $\sum_{k\neq 2006} \beta_k \big(A_{i,2006} \cdot \mathbb{1}\{t = k\}\big)$, where each β_k captures the discontinuity at the threshold in year k compared to the 2006 baseline. Estimates are obtained using both OLS linear regressions to capture effects in levels and Poisson Pseudo-Maximum Likelihood estimation. The latter is particularly suited for skewed balance sheet variables with many zero observations, like in the case of bank loans, and allows proportional effects to be estimated while retaining zero outcomes.

We report results across multiple bandwidth choices, including fixed bandwidths ranging from 8 to 35 and data-driven optimal bandwidths selected using the `rdrobust` procedure of Calonico et al. (2017). Our preferred specification adopts the fixed bandwidth closest to the MSE-optimal bandwidth computed for the discontinuity in TFR stocks during the pre-reform period (2004–2006). Choosing the bandwidth based only on pre-reform data ensures that bandwidth selection is not influenced by post-treatment outcomes. Results are robust to alternative bandwidth choices, including the average of the pre- and post-reform MSE-optimal bandwidths as in Grembi et al. (2016), as well as fixed bandwidths[25] (see Section 5.1).

We begin by documenting the effects of the reform on firms' internal liquidity, verifying that the mandate reduces both TFR stocks and operating cash flows. We then examine how the shock propagates through firms' balance sheets. On the asset side, we analyze adjustments in current assets—including short-term liquid assets, inventories, and accounts receivable—as well as fixed assets, distinguishing between tangible and intangible components. This allows us to assess whether firms respond by drawing down liquidity buffers, compressing working capital, or scaling back investment. On the liability side, we examine accounts payable, equity, and financial debt. We pay particular attention to the composition of debt across bank and non-bank lenders, which allows us to assess whether firms reallocate across alternative funding sources (Cosci et al., 2015), and across short- and long-term maturities.

To study heterogeneity, we compare firms in the top and bottom quartiles (Q4 and Q1) of the distribution of key firm-level characteristics, such as the liquidity-to-assets ratio.[26] Specifically, we compute, for each firm, the average value of the relevant variable over the pre-reform period 2004–2006.[27] Based on this distribution, we construct a time-invariant firm-level indicator, H_i^{pre}, which equals one for firms in the top quartile (Q4) and zero for

[25] Calonico et al. (2017) show that bandwidth sensitivity analysis is informative only for bandwidths close to the MSE-optimal choice.

[26] We also perform robustness checks using median splits, without excluding observations in the middle quartiles (Q2 and Q3).

[27] Using the 2004–2006 average, rather than the 2006 level, reduces sensitivity to transitory shocks and mitigates mechanical mean-reversion concerns in the construction of high- and low-vulnerability groups.

firms in the bottom quartile (Q1). We then extend the baseline DiRD specification by interacting the treatment indicator with H_i^{pre}, as follows:

$$
\begin{aligned}
Y_{it} = \lambda_i + \lambda_{rt} + \lambda_{st} + \sum_{k \neq 2006} \beta_k \Big(A_{i,2006} \cdot \mathbb{1}\{t = k\} \Big) \\
+ \sum_{k \neq 2006} \delta_k \Big(S_{i,2006} \cdot \mathbb{1}\{t = k\} \Big) + \sum_{k \neq 2006} \xi_k \Big(S_{i,2006} \cdot A_{i,2006} \cdot \mathbb{1}\{t = k\} \Big) \\
+ \sum_{k \neq 2006} \theta_k \Big(H_i^{pre} \cdot \mathbb{1}\{t = k\} \Big) + \sum_{k \neq 2006} \iota_k \Big(H_i^{pre} \cdot A_{i,2006} \cdot \mathbb{1}\{t = k\} \Big) \\
+ \sum_{k \neq 2006} \alpha_k \Big(H_i^{pre} \cdot S_{i,2006} \cdot \mathbb{1}\{t = k\} \Big) \\
+ \sum_{k \neq 2006} \mu_k \Big(H_i^{pre} \cdot S_{i,2006} \cdot A_{i,2006} \cdot \mathbb{1}\{t = k\} \Big) + \varepsilon_{it},
\end{aligned} \tag{2}
$$

where β_k traces the year-specific treatment effect for firms with $H_i^{pre} = 0$, and ι_k measures the additional effect for firms with $H_i^{pre} = 1$. This equation implements a triple difference-in-discontinuities design, identifying subgroup-specific local treatment effects while allowing both the level and slope of the outcome–size relationship to vary flexibly over time within each subgroup.[28] Results are robust to estimating the baseline DiRD separately within each quartile of the heterogeneity variable.

To shed light on mechanisms, we first examine heterogeneity by financial constraints. The reform removed a stable source of internal finance that was, on average, cheaper than market credit (Figure 1). If severance-based liquidity alleviates financing frictions, the largest effects should be concentrated among firms with limited liquidity buffers, which would otherwise need to rely on external credit to finance marginal investment. Our baseline proxy for ex-ante financial constraints is the ratio of liquid assets to total assets.[29]

We next explore credit-supply heterogeneity, as Cingano et al. (2016) show that firms borrowing from banks more reliant on interbank funding were disproportionately exposed to the post-2007 tightening in credit supply. Following Adamopoulou et al. (2021), we construct a firm-level indicator of ex-ante exposure to banks dependent on wholesale funding, as described in Section 3.1.[30] Stronger balance-sheet effects among firms borrowing from these banks would suggest that the financing shock operates partly through a bank lending

[28] The additional interaction terms $\{\theta_k, \alpha_k, \mu_k\}$ allow subgroup-specific intercepts and size slopes to vary flexibly over time both below and above the cutoff, preserving the DiRD design's robustness to time-varying rotations in the outcome-size relationship within each subgroup.

[29] Because no single proxy is definitive, we also use alternative measures. Results are robust to the leverage ratio, with higher financial debt (bank plus non-bank debt) over total assets indicating tighter constraints (Hadlock and Pierce, 2010), and the Whited–Wu index (Whited and Wu, 2006), which combines firm-level variables from an Euler-equation investment model with financial frictions. According to this index, financially constrained firms exhibit low cash flows and asset bases, do not pay dividends, rely more heavily on long-term debt, and operate in high-growth industries that require greater external financing.

[30] Adamopoulou et al. (2021) show that this measure is uncorrelated with key bank characteristics—capital, profitability, non-performing loans, lending rates, or size—but negatively correlated with retail deposits and liquid assets, which are substitutes for interbank funding. This supports its interpretation as a structural indicator of banks' reliance on wholesale markets rather than differences in risk or performance.

channel.

Finally, we study heterogeneity related to production technology. If the liquidity shock increases the marginal cost of capital, we expect more pronounced investment responses among firms with higher capital intensity (Adamopoulou et al., 2021), measured by the capital–labor ratio.

While our baseline specification adopts a sharp DiRD framework, treating all firms above the 50-employee threshold as fully exposed to mandated TFR outflows and all firms below as retaining newly accrued TFR within the firm, treatment assignment is not perfectly sharp in practice. In 2007, approximately 12 percent of employees in firms below the 50-employee threshold redirected their TFR to pension funds (COVIP, 2008). This generates limited cash flow leakage within our control group (firms with 25–50 employees in 2006), attenuating estimated treatment effects.[31]

3.3 Sample & Manipulation Tests

To construct the sample, we keep firms observed in the integrated INPS–CERVED–CR dataset starting in 2004 and allow for firm exit from 2007 onward,[32] as treatment status is defined based on average employment in 2006. All baseline results reported in Section 4 remain robust when re-estimated on the balanced panel of firms surviving through 2010.

Table 1 reports pre-reform characteristics for firms in our baseline sample, which uses a symmetric 25-employee bandwidth around the 50-employee cutoff in 2006 as detailed in Section 3.2. Panel A summarizes balance-sheet and CR variables (the latter based on drawn credits), while Panel B reports employment and wage[33] variables from the *INPS Universo Imprese*. All monetary values are expressed in thousands of euros.

Consistent with the continuity assumption underlying the design, firms just above and below the 50-employee cutoff display remarkably similar pre-reform characteristics once outcomes are scaled by assets or cash flows. Short-term liquid assets, fixed assets, working-capital components, capital structure and exposure to interbank-reliant lenders align closely across groups. Profitability, productivity[34] and capital intensity are also comparable, as well as sectoral and regional distributions, reinforcing the validity of the local comparison. Mechanical differences emerge only in scale-dependent variables: firms above the threshold employ more workers—and therefore more managers, white-collar, and blue-collar employees—but wage rates by worker type are nearly identical across groups. Finally, the descriptive statistics highlight the quantitative importance of severance-based liquidity. Prior to the reform, TFR stocks amounted to roughly 8 percent of total assets, while annual TFR

[31] Leakage is smallest in the implementation year. Workers had until June 30, 2007 to make their irrevocable allocation decision, and automatic enrollment into pension funds occurred only at mid-year, implying that sub-threshold firms retained the majority of TFR flows during the first half of 2007.

[32] Because exit may itself be affected by the reform, conditioning on post-reform survival could introduce selection bias.

[33] Wages are proxied by total labor costs divided by the number of employees.

[34] We construct a revenue-based measure of total factor productivity (TFPR) as value added divided by a Cobb–Douglas aggregate of capital and labor inputs, with sector–year specific output elasticities. The labor elasticity equals the average labor share (labor costs over value added) within each two-digit ATECO sector and year, and the capital elasticity is one minus this share.

accruals represented about 26 percent of operating cash flow. These magnitudes are nearly identical for treated and control firms, further underscoring the comparability of firms at the cutoff.

Table 1: 2004–2006 Descriptive Statistics (Bandwidth 25)
Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.
The table reports descriptive statistics for an unbalanced panel of firms observed between 2004 and 2010. Based on their 2006 workforce size, firms are divided into two groups centered around the 50-employee threshold: a Treated group (50–75 employees) and a Control group (25–49 employees).

Variable	Treated	Control	Diff.
Panel A. CERVED-Credit Registry Variables			
TFR Stock / Assets	0.08	0.08	0.00***
TFR Flow / Cash Flow	0.26	0.25	0.00
Cash Flow / Assets	0.07	0.07	0.00
Liquidity/Assets	0.07	0.08	-0.01***
Inventories / Assets	0.16	0.17	-0.01***
Accounts Receivable / Assets	0.46	0.45	0.01***
Fixed Assets / Assets	0.24	0.24	-0.00***
Tangible Fixed Assets / Assets	0.20	0.21	-0.01***
Accounts Payable / Assets	0.27	0.27	0.00
Short-Term Bank Debt / Assets	0.13	0.13	0.01***
Long-Term Bank Debt / Assets	0.06	0.06	0.01***
Lenders' Interbank Exposure	0.12	0.11	0.01***
Non-bank Debt / Assets	0.05	0.05	0.00***
Equity / Assets	0.22	0.20	0.02***
Profits / Assets	0.01	0.01	0.00***
TFPR	3.24	3.20	0.05*
Value Added / Capital	7.12	6.50	0.63***
Value Added / Labor Cost	2.25	2.27	-0.01
Share: Other activities	0.03	0.03	0.00**
Share: Manufacturing	0.47	0.46	0.02***
Share: Construction	0.08	0.10	-0.02***
Share: Services	0.42	0.41	0.01
Share: North	0.65	0.63	0.02***
Share: Center	0.18	0.19	-0.01***
Share: South & Islands	0.17	0.18	-0.01***
Age	21.40	19.65	1.75***
Panel B. INPS Variables			
Labor costs	1333.48	739.11	594.36***
Employment - total	59.01	34.02	24.99***
Managers	0.66	0.31	0.35***
White collars	20.77	11.52	9.24***
Blue collars	35.75	20.96	14.79***
Apprentices	1.24	1.05	0.18***
Wages – total	22.48	21.53	0.95***
Managers	94.64	92.32	2.32***
White collars	26.34	24.93	1.41***
Blue collars	19.05	18.85	0.20***
Apprentices	13.43	13.08	0.35***
Labor Share	0.54	0.54	0.00
Capital-Labor Ratio	1.97	2.13	-0.16***
Observations	22,761	73,104	95,865

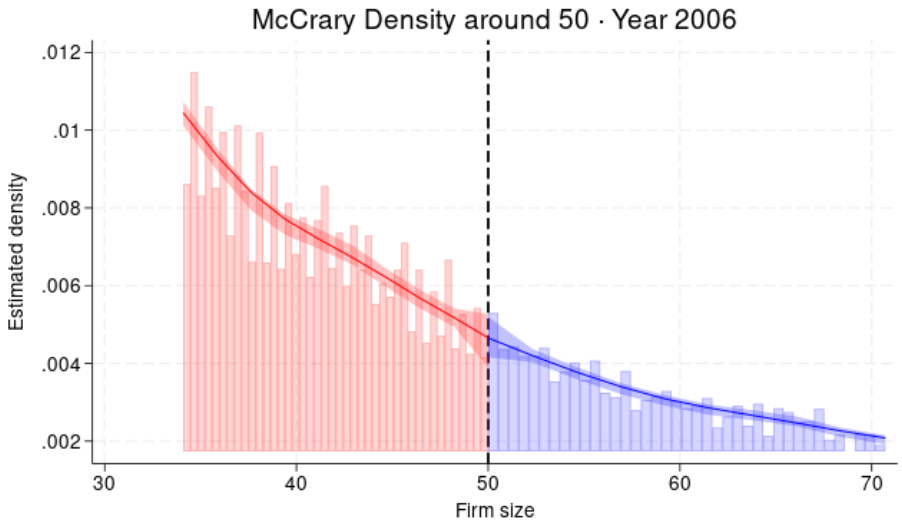
Diff. is defined as Treated minus Control. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$..

A key identification requirement is the absence of strategic manipulation of the running

variable. In our setting, this concern is inherently limited: treatment is assigned according to firms' average 2006 FTE employment, a measure announced only in late October 2006, leaving little scope for firms to adjust size in anticipation of the reform. Indeed, reducing average size through dismissals would have required firms with roughly 51 workers to dismiss more than five employees in November–December 2006, thereby triggering the collective dismissal procedure under Law 223/1991 which entails lengthy union consultations and makes quick adjustments highly unlikely.

We assess potential manipulation of the running variable using annual average headcounts from the *INPS Universo Imprese* archive. Figure A.1 displays the 2006 size distribution around the 50-employee cutoff and reveals no visible bunching just below the threshold. Figure 4 reports a McCrary density test for 2006. The estimated discontinuity in the density at the cutoff is small and statistically indistinguishable from zero, providing no evidence of sorting in the assignment year. Figure A.2 extends this analysis to annual average headcounts over the full sample period. In the pre-reform years, estimated density discontinuities remain statistically indistinguishable from zero. In the post-reform period, the density exhibits mild visual compression above the threshold; however, the estimated discontinuities remain imprecisely estimated and statistically insignificant. This pattern is consistent with entry dynamics, as newly established firms are subject to the TFR outflow mandate based on their employment size in the first year of activity. Importantly, these post-reform dynamics do not threaten identification: treatment status is defined exclusively based on firms active in 2006 and depends solely on their 2006 average employment.

Figure 4: McCrary Density Test Around the 50-Employee Cutoff (2006)
Source: INPS *Universo Imprese*.
The figure reports McCrary density tests based on firms' annual average employment in 2006. It displays the estimated discontinuity in the density of firm size at the 50-employee cutoff.



4 Results

4.1 Baseline

The dynamic DiRD estimates reveal a clear sequence of adjustments consistent with a binding and persistent cash flow shock (Figure 5). Treated firms—those just above the 50-employee cutoff in 2006—experience a large and steadily widening depletion of their severance payment accounts. Beginning in 2007, their TFR stock falls by roughly €35,000 per year relative to firms just below the threshold as new inflows cease (Table A.1). By 2008, the accumulated shortfall is already significant: TFR balances are 16.5%[35] lower relative to the 2006 cutoff discontinuity. By 2010, the cumulative shortfall reaches approximately €150,000—an overall 28.6% decline with respect to implied counterfactual levels.

This erosion of internal liquidity maps almost one-for-one into tighter operating budgets. Starting in 2007, treated firms' operating cash flows decline by about €50,000 per year with respect to the control group (Table A.1). The magnitude closely matches the reform's mechanical effect: statutory accrual rules imply a loss of 6.3% of the wage bill net of fiscal offsets. Given a pre-reform average wage bill of €900,000, this corresponds to €56,700 per year. Allowing for partial leakage in the control group—about 12% of employees in sub-threshold firms redirecting TFR to pension funds from 2007—yields an expected treated–control gap of roughly €50,000. The close alignment between this back-of-the-envelope calculation and the estimated cash-flow decline supports the quantitative plausibility of the estimates and the interpretation of the reform as a large, plausibly exogenous shock to internal finance.

Treated firms show lower outstanding amounts of liquid assets relative to the control group (Figure 6): dynamic DiRD estimates indicate reductions of €36,000 in 2007 (Table A.1), remaining at similar levels in 2008, and cumulating to €55,000 by 2010, overall corresponding with a 9.4% contraction (Table A.2). The sharp first-year decline reflects a mechanical effect of the reform: as TFR flows are no longer retained, liquid assets fall in the absence of immediate adjustments on other balance sheet items. In subsequent years, the decline stabilizes as firms adjust along other margins.

We find no evidence that firms compensate for the loss of TFR funds by increasing borrowing. Along short-term financing margins,[36] we detect no statistically significant effects on short-term bank loans (despite a positive coefficient in 2007; Table A.2) or on trade payables.[37] In contrast, long-term bank debt gradually declines relative to firms just below the threshold, reaching a reduction of €120,000 by 2010, which represents an overall contraction of 12%. We also find no evidence of substitution into non-bank financing—such as leasing or factoring—or equity issuance (Table A.1).

[35] The percentage impact of the treatment is calculated as $100 \times (e^{\beta} - 1)$, where β denotes the coefficient estimated via Poisson Maximum Likelihood.

[36] Firms could also manage short-term liquidity needs via working-capital adjustments, for instance by reducing inventories or accounts receivable. Nevertheless, our estimates show no statistically significant effects along these margins. Results are available upon request.

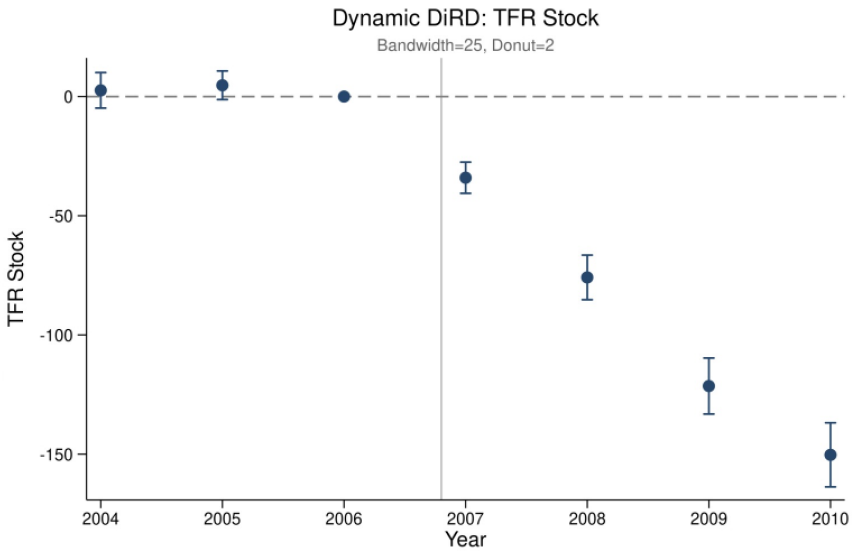
[37] Results are available upon request.

Figure 5: Dynamic DiRD Estimates: TFR Stock and Cash Flows

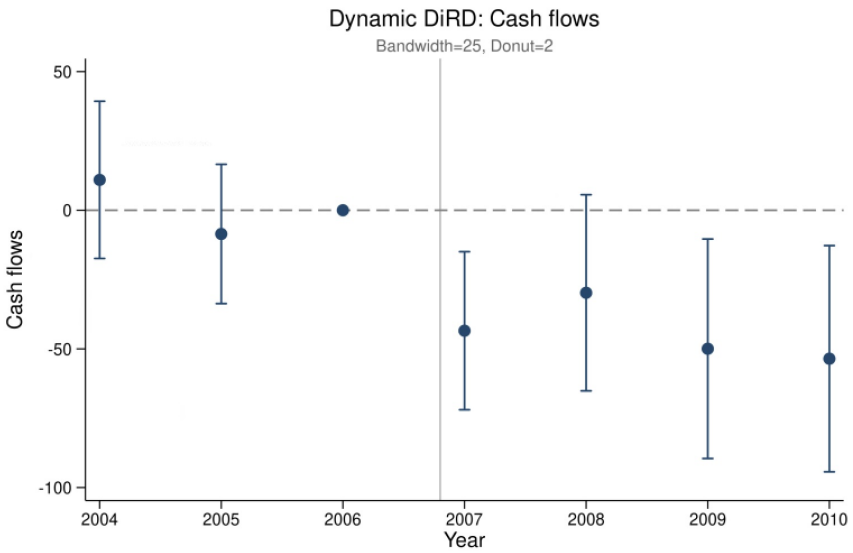
Source: CERVED – INPS (*Universo Imprese*).

The figure plots year-specific DiRD coefficients capturing the reform-induced discontinuity at the 50-employee threshold in 2006, estimated relative to the 2006 baseline. The vertical grey line marks the October 2006 announcement of the cutoff that determined treatment status. Each point reports a local-linear DiRD estimate obtained within a 25-employee bandwidth around the cutoff, with a donut of 2, firm-clustered SE and 95% CI.

(a) TFR Stock



(b) Operating Cash Flows



As liquidity tightens and external financing does not compensate, firms adjust along the capital margin, which inherently responds more slowly. Fixed assets start declining sharply in 2008 relative to firms just below the threshold (Figure 6), with dynamic DiRD estimates pointing to a 5% fall (Table A.2). In levels, treated firms hold approximately €160,000 less capital in 2008 relative to the 2006 baseline at the threshold. The gap with respect to the control group widens to about €220,000 by 2009—corresponding to a 7% reduction—and remains broadly stable in 2010. The fall in the stock of fixed assets primarily reflects lower net investment in 2008[38] and is driven by tangible[39] assets (property, plant and equipment; *PPE*), which decline by roughly 6% (€180,000) by 2009. We find no statistically significant effects on headcounts or labor costs, suggesting the presence of high labor adjustment costs (Oi, 1962; Sestito and Viviano, 2018).[40] Consequently, firms experience a gradual decline in the capital–labor ratio of roughly 7 percent relative to the 2006 baseline discontinuity.

Taken together, these patterns indicate that, as internal funds decline, firms adjust primarily by reducing new investment, leading to a contraction in the capital stock—especially PPE—without any compensating increase in external borrowing. A large literature shows that financially constrained firms' investment decisions depend strongly on internal funds and working capital availability (Fazzari et al., 1987; Himmelberg and Petersen, 1994). The observed joint decline in fixed assets and long-term bank debt further suggests that TFR's funds and bank credit act as complements rather than substitutes in financing investment. One possible interpretation of these results is provided by models with collateral constraints or limited pledgeability due to moral hazard (Holmstrom and Tirole, 1997; Calcagno et al., 2011),[41] in which banks finance only a fraction of investment and firms must fund the remaining share using internal funds or unsecured financing. In this sense, TFR effectively functioned as a form of low-cost unsecured financing that allowed firms to co-finance the portion of investment not covered by collateralized bank lending. When this source of funding is reduced, firms cannot substitute it with additional bank borrowing if constrained by available collateral.[42] Financing the same investment would require more expensive unsecured borrowing, raising the marginal cost of financing and leading firms to scale back investment and, consequently, their demand[43] for bank debt. A second amplification mech-

[38] Results on the flow of net investment are available upon request.

[39] We also find a reduction in intangible assets in Poisson specifications, although not statistically significant in levels, likely due to the skewed distribution of intangible assets and the large share of firms with zero reported intangibles, for which proportional changes are better captured by Poisson estimators. This is consistent with Bartram (2017), who show that internal funds linked to employee liabilities are particularly important for financing intangible investment, which is difficult to collateralize and therefore relies more heavily on internal liquidity than tangible investment. Results are available upon request.

[40] Results are available upon request.

[41] Calcagno et al. (2011) show that the 2007 reform, by diverting TFR flows away from firms, lowered pledgeable income available to satisfy banks' participation constraints. Their model delivers two equilibrium predictions: first, a reduction in internal liquidity generates a more-than-proportional contraction in investment; second, as pledgeable income declines, the demand for bank credit also falls. Both predictions closely match our empirical evidence. Endogenous bank monitoring further amplifies the interaction between internal liquidity and borrowing capacity.

[42] Asymmetric information makes external finance particularly difficult when investment returns are uncertain or difficult to monitor (Stiglitz and Weiss, 1981). Collateral helps mitigate these problems by serving both as a screening device under adverse selection and as an incentive device under moral hazard (Bester, 1985).

[43] This pattern could also reflect tighter credit supply if banks curtail lending to firms with diminished cash

anism operates through collateral. As investment declines and fails to offset depreciation, the stock of fixed assets shrinks, reducing the value of pledgeable collateral and further tightening borrowing capacity (Guiso, 2003; Almeida and Campello, 2007).

4.2 Channels

We next investigate three potential mechanisms underlying the contraction in fixed assets and long-term debt following the TFR reform: financial constraints, credit-supply constraints and differences in production structure. As detailed in Section 3, we extend the DiRD specification by interacting the treatment with quartiles of pre-reform firm characteristics: the liquidity-to-asset ratio (proxying internal financial constraints), lenders' interbank exposure (proxying supply-side fragility) and the capital-labor ratio (proxying production technology). These heterogeneity estimates are suggestive rather than strictly causal, as each proxy may correlate with other firm attributes. While differences across quartiles are not always statistically distinguishable, they provide informative evidence on how pre-existing conditions shape the response to the reform.

The evidence points to financial constraints as a key driver, with the strongest contractions in investment and long-term bank credit occurring among firms with low pre-reform liquidity-to-asset ratios (Figure 7). Results are robust to alternative proxies of financial constraints discussed in Section 3.[44] This heterogeneity speaks directly to the long-standing debate on investment–cash flow sensitivities. Since Fazzari et al. (1987), a strong correlation between investment and internal cash flow has been interpreted as evidence of financing constraints. Subsequent work, however, argued that such sensitivities may reflect measurement errors or cash flow proxying for expected profitability (Kaplan and Zingales, 1997; Erickson and Whited, 2000). Our setting addresses these endogeneity concerns:[45] the reform generates an exogenous, legally mandated liquidity shock that is orthogonal to firms' investment opportunities. The fact that investment declines most sharply among ex-ante liquidity-constrained firms supports the financing-constraints interpretation. The lack of statistically significant differences across constraint groups may reflect measurement error in commonly used proxies for financial constraints (Farre-Mensa and Ljungqvist, 2016) and the fact that our sample consists of SMEs. Unlike the publicly traded firms typically studied in the corporate finance literature, many firms in our sample classified as "unconstrained" may still face financing frictions, which reduces observable differences across groups (Saez et al., 2019).

A central question is whether the contraction in fixed assets and long-term debt reflects tighter credit supply, weaker credit demand, or both. While Calcagno et al. (2011)'s moral hazard framework emphasizes a demand-side mechanism,[46] supply-side frictions likely

buffers.

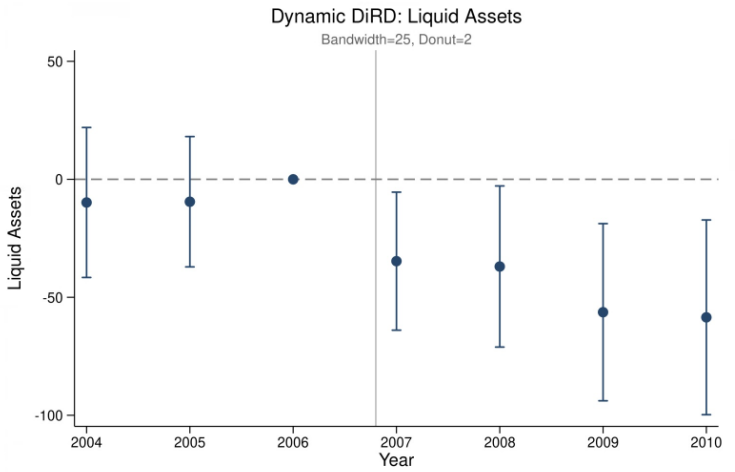
[44]Using leverage (financial debt over total assets) (Hadlock and Pierce, 2010) and the Whited–Wu index (Whited and Wu, 2006), we confirm that firms that were more financially constrained ex ante exhibit the largest investment contractions. Results are available upon request.

[45]Our design also improves on earlier pension-funding quasi-experiments, such as Rauh (2006), whose identification strategy has been questioned on comparability grounds by Bakke and Whited (2012).

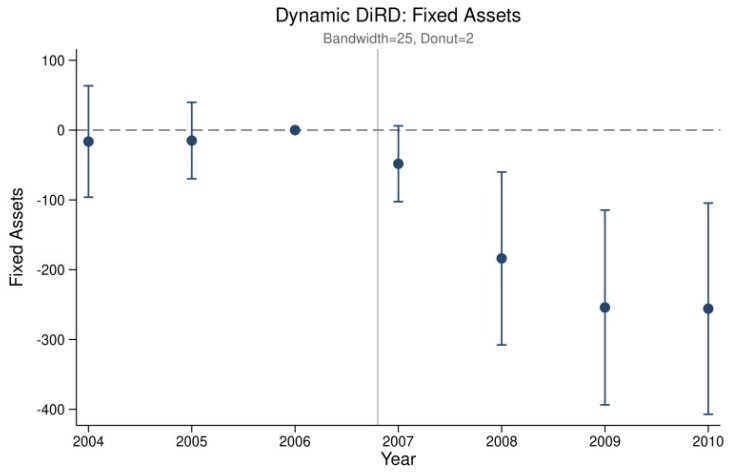
[46]By diverting TFR flows, the reform reduces pledgeable income, triggering more-than-proportional invest-

Figure 6: Dynamic DiRD Estimates: Liquidity, Fixed Assets and Long-term Bank Debt.
Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.
The figure plots year-specific DiRD coefficients capturing the reform-induced discontinuity at the 50-employee threshold in 2006, estimated relative to the 2006 baseline. The vertical grey line marks the October 2006 announcement of the cutoff that determined treatment status. Each point reports a local-linear DiRD estimate obtained within a 25-employee bandwidth around the cutoff, with a donut of 2, firm-clustered SE and 95% CI.

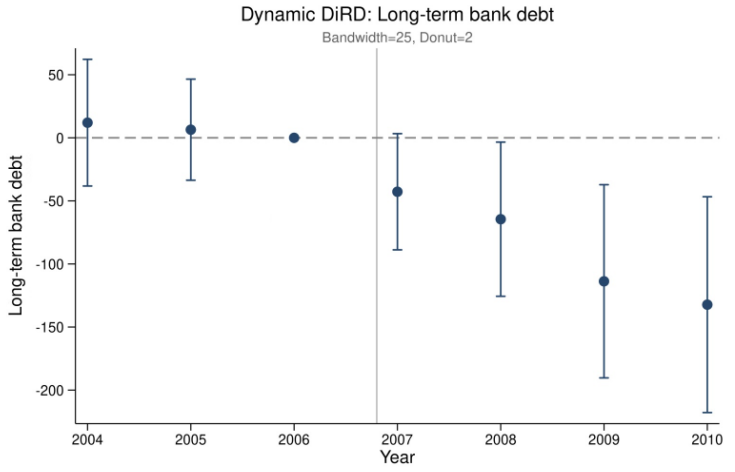
(a) Liquid Assets



(b) Fixed Assets



(c) Long-term Bank Debt



amplified the shock, especially amid the 2007–2008 crisis. Following Cingano et al. (2016), we exploit heterogeneity in lenders' balance-sheet fragility by computing each firm's exposure to interbank funding stress as the credit-weighted share of interbank funding across its 2006 lenders (3.1). Firms borrowing from banks with high pre-reform interbank funding exposure—more vulnerable to the wholesale funding freeze—face substantially steeper reductions in investment and long-term credit (Figure 7). This pattern is consistent with a supply-side amplification channel, whereby banks exposed to funding stress tighten lending disproportionately to firms whose internal liquidity buffers were weakened by the reform, possibly reflecting heightened concerns about default risk.

Heterogeneity in production technology further moderates the response (Figure 7). Firms with high capital–labor ratios experienced the strongest contractions in fixed assets, suggesting that the TFR-liquidity shock raised acutely the effective cost of capital for firms more reliant on fixed inputs. This evidence aligns with Adamopoulou et al. (2021), who show that increases in the cost of capital due to negative credit supply shocks reduce firms' investment and demand for credit, with stronger and more persistent effects among capital-intensive firms.

Overall, the heterogeneity results show that the reform's impact is far from uniform. The strongest adjustments occur among firms that were liquidity constrained, borrowed from banks vulnerable to interbank shocks and were more capital-intensive, suggesting that the reform mattered most where TFR served as a key and persistent liquidity buffer for Italian SMEs. More broadly, the evidence points to liquidity-driven mechanisms—rather than labor-wedge channels—as the central force shaping post-reform dynamics in debt and investment.

ment cuts. The accompanying debt reduction reflects a contraction in the scale of viable projects rather than higher credit prices, consistent with weakened demand.

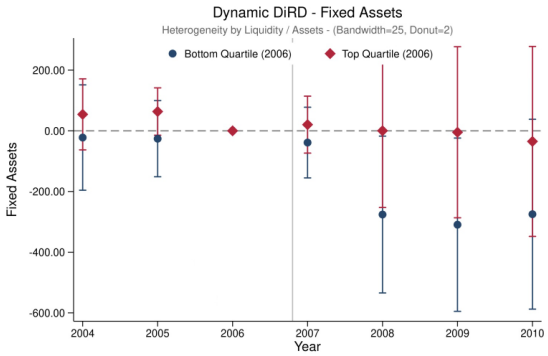
Figure 7: Dynamic *Estimates*: Heterogeneity by Quartiles of Firm Characteristics

Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.

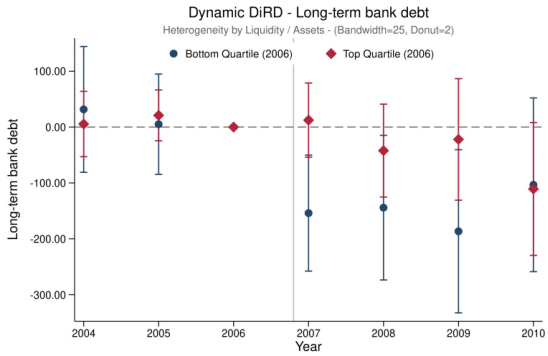
The figure reports year-specific heterogeneous DiRD coefficients obtained from equation (2), where the treatment is interacted with an indicator equal to one for firms in the top quartile (Q4) and zero for firms in the bottom quartile (Q1) of the pre-reform distribution of the relevant firm characteristic: (i) liquidity-to-asset ratio, and (ii) lenders' interbank exposure, and (iii) capital–labor ratio. The vertical black line marks the October 2006 announcement of the reform. Local-linear DiRD estimates with a 25-employee bandwidth around the 50-employee threshold, firm-clustered standard errors and 95% confidence intervals.

(i) Liquidity-to-Asset Ratio

(a) Fixed Assets

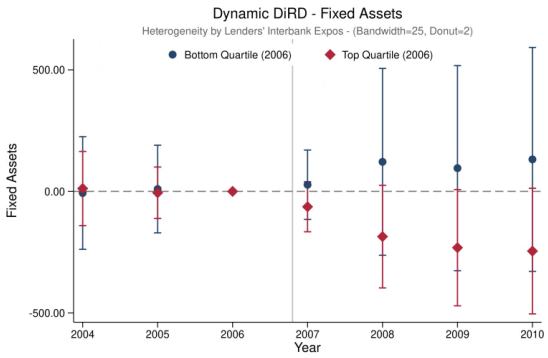


(b) Long-term Bank Debt

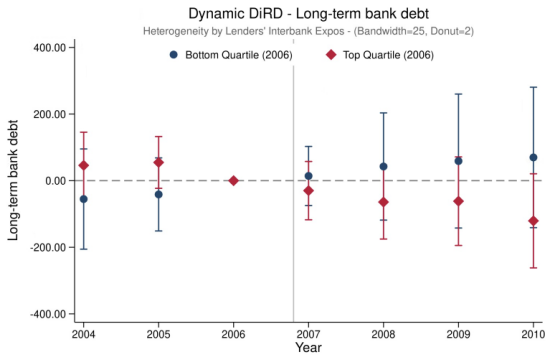


(ii) Lenders' Interbank Exposure

(c) Fixed Assets

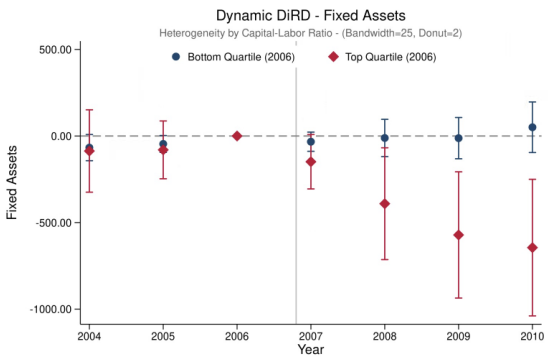


(d) Long-term Bank Debt

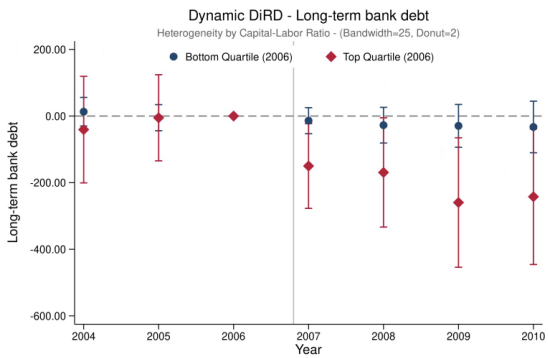


(iii) Capital–Labor Ratio

(e) Fixed Assets



(f) Long-term Bank Debt



5 Validity Tests

5.1 Sensitivity Analysis to Bandwidth Selection

Our baseline specification employs a fixed 25-employee bandwidth, in line with the MSE-optimal bandwidth for the pre-reform TFR stock. Figure 8 evaluates the sensitivity of our primary estimates to alternative bandwidth selections. For each outcome, we plot the static DiRD coefficient across a range of bandwidths, marking the pre-reform MSE-optimal choice with a vertical line. [47] The point estimates remain remarkably stable for the bandwidth ranging from 15 to 35, suggesting that our results are not sensitive to the specific window around the threshold or the inclusion of firms further from the cutoff. As expected, confidence intervals widen at smaller bandwidths as the sample size decreases, reflecting the standard bias–variance tradeoff inherent in local-polynomial estimators.

5.2 Regression-Discontinuity Design

We supplement our baseline DiRD estimates with graphical evidence from local-linear Regression Discontinuity (RD) specifications. For each year following the 2007 reform, we estimate the discontinuity at the 50-employee threshold using a first-difference specification and a 25-employee bandwidth. By centering the outcomes on their 2006 levels, we net out time-invariant firm-specific heterogeneity, providing a transparent visual counterpart to our panel estimates. Figures A.3, A.4, and A.5 present these cross-sectional RD plots for our core outcomes: TFR stocks, fixed assets and long-term bank debt.[48] The visual patterns closely mirror our baseline DiRD results: a sharp, precisely estimated discontinuity emerges at the threshold immediately following the reform and widens over time. The fact that these jumps are clearly discernible in the raw data, within a focused 25-employee bandwidth, confirms that our findings are not an artifact of the high-dimensional fixed effects. Instead, the evidence points to a robust, locally identified treatment effect driven by the 2007 liquidity mandate.

5.3 Placebo Tests

As a final robustness exercise, we re-estimate our baseline DiRD specification at placebo thresholds of 40 and 60 employees to ensure our results are not driven by misspecification or unobserved nonlinearities in the size–outcome relationship. These placebo cutoffs should yield null results, as they do not correspond to any change in the TFR mandate. As shown in Table A.3, the coefficients at these thresholds are generally statistically insignificant and lack the systematic post-reform trajectory observed at the true 50-employee cutoff. This confirms that the discontinuity at 50 is uniquely policy-induced and not an artifact of a

[47] Calonico et al. (2017) emphasize that the sensitivity analysis is informative only for bandwidths in a neighborhood of the MSE-optimal choice; away from this range, changes in estimates and confidence intervals are mechanically driven by the statistical properties of the estimator rather than by the underlying economic effect.

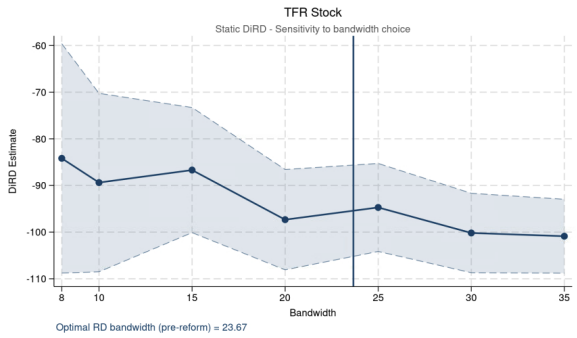
[48] RD plots on operating cash flows and liquid assets are available upon request.

Figure 8: Static DiRD Estimates by Bandwidth

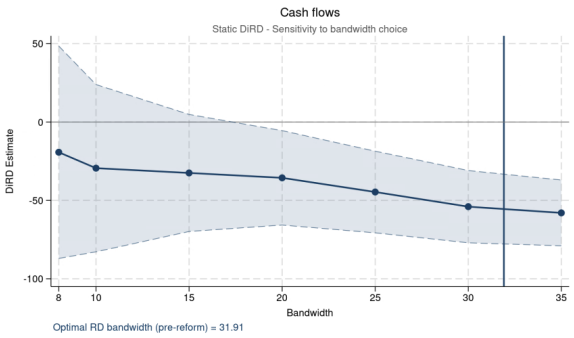
Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.

Each panel plots static DiRD estimates as a connected series of point estimates (solid blue line) with corresponding 95% confidence bands (dashed blue lines) over a grid of bandwidths (from 8 to 35) around the 50-employee cutoff, from 8 to 35. Estimates come from separate DiRD regressions with a donut of 2 employees. The vertical blue line marks the pre-reform MSE-optimal bandwidth computed following Calonico et al. (2017).

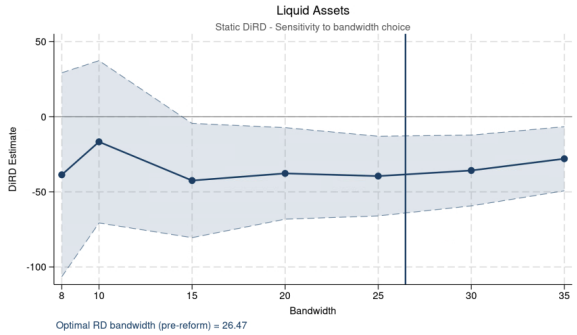
(a) TFR Stock



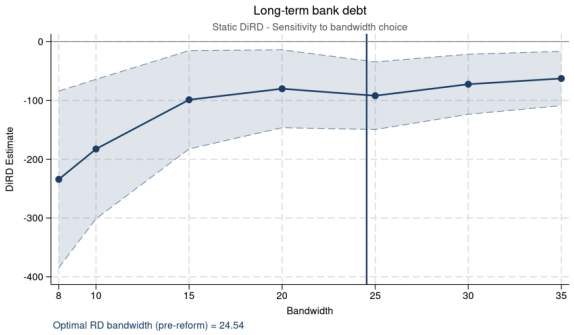
(b) Operating Cash Flows



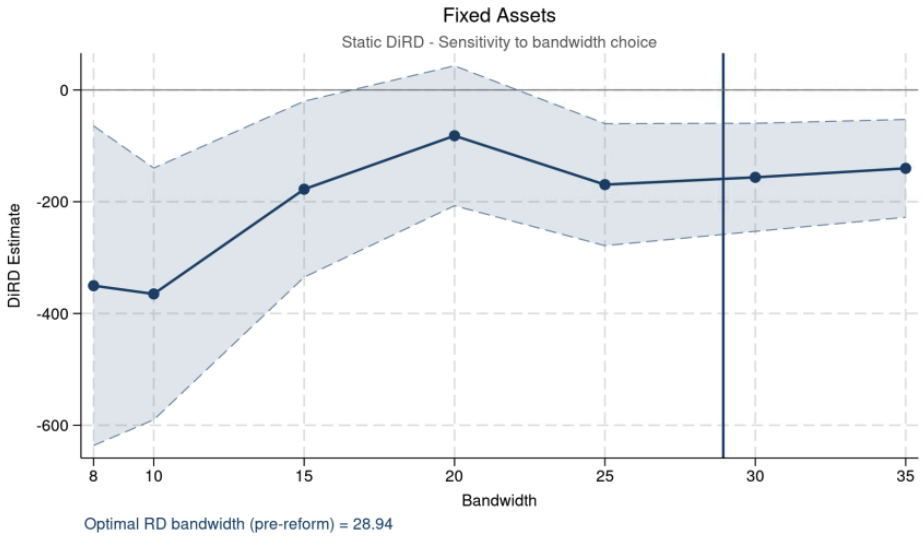
(c) Liquid Assets



(d) Long-term Bank Debt



(e) Fixed Assets



non-linear size gradient. We also verify the persistence of the treatment assignment rule. Because the 2007 reform determined treatment status based on firm size in 2006, post-reform discontinuities should only appear when using this specific "running variable". Consistent with this, when we estimate year-by-year local-linear RDs comparing the true running variable (firm size in 2006) with contemporaneous firm size, we find that the effects vanish or diminish significantly when the running variable is current size.[49] This result rules out confounding effects from other size-based regulations that depend on current employment levels rather than the 2006 baseline.

6 Conclusions

This paper provides causal evidence on the role of internal finance in shaping firms' real and financial decisions. We exploit the 2007 Italian TFR reform as a sharp, size-contingent shock to internal liquidity for small and medium-sized enterprises. By mandating that firms with more than 50 employees—based on predetermined 2006 employment—transfer newly accrued severance flows to a state-administered fund, the reform generated an exogenous and persistent reduction in internally retained cash flows of roughly 6.3 percent of the annual wage bill.

Using a Difference-in-Regression-Discontinuities design around the 50-employee threshold and matched administrative data on balance sheets, employment, and firm–bank credit relationships, we document an economically meaningful sequence of adjustments. Treated firms experience an immediate and sustained decline in TFR stocks and operating cash flows with respect to the control group. As liquidity buffers are depleted, however, firms do not substitute toward short-term borrowing, trade credit, or non-bank finance. Instead, relative to firms just below the threshold, long-term bank debt contracts and fixed investment declines sharply, with effects concentrated in tangible capital. The adjustment thus occurs primarily along the capital margin rather than through labor or working-capital reallocation.

These findings point to a framework in which internal and external finance jointly determine firms' investment capacity rather than acting as substitutes. Internal liquidity appears to support investment not only by providing cash resources but also by sustaining collateral accumulation and borrowing capacity. As a result, shocks to internal funds propagate through firms' balance sheets, affecting both investment and external borrowing. The joint contraction in internal funds, fixed assets, and long-term bank credit is difficult to reconcile with frictionless models. Consistent with financing frictions as a central transmission mechanism, the effects are strongest among ex-ante vulnerable firms, those exposed to bank credit-supply shocks during the 2008 crisis, and capital-intensive producers.

More broadly, our results speak to the long-standing investment–cash flow sensitivity debate. By leveraging a policy-induced liquidity shock orthogonal to firms' investment opportunities, we address the endogeneity concerns that complicate traditional Q-based

[49] Figures are available upon request

regressions and provide direct support for the financing-constraints view. This aligns with evidence from Italian SMEs' reliance on internal funds (Guiso, 2003) and complements studies on financing shocks' real effects (Chodorow-Reich, 2014; Berton et al., 2018).

The results also have broader implications for the design of severance and pension systems. Reforms that redirect severance accruals toward funded pension schemes may strengthen retirement savings but also remove a stable and low-cost liquidity buffer from SMEs. In economies characterized by persistent credit frictions, expanding such mandates—such as under the recent extensions of the Italian TFR mandate ($L.199/2025$) which progressively lowers the employment threshold for compulsory externalization—may unintentionally dampen capital accumulation unless accompanied by measures that mitigate financing constraints.

Future research could extend the analysis to assess whether the documented contraction in capital accumulation translates into persistent effects on firm growth, productivity, profitability and survival beyond 2010. A complementary avenue would be to leverage granular loan-application to more cleanly disentangle credit-demand from credit-supply responses following the reform. Finally, while Benzarti and Harju (2021) document simultaneous short-term declines in capital and labor from similar payroll tax shocks but cannot isolate liquidity from substitution effects, our setting highlights the central role of internal finance in shaping investment dynamics. This suggests the need for theoretical models that embed a CES production structure within environments featuring cash-flow- and/or asset-based borrowing constraints and explicit factor adjustment costs, allowing liquidity effects and input substitution to interact in a unified framework.

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A Appendix

A.1 Figures

Figure A.1: Size Histograms for Italian SMEs
Source: INPS *Universo Imprese*.
The figure plots the distribution of firms' annual average headcounts using the *INPS Universo Imprese* archive. The vertical red dashed line marks the 50-employee cutoff used for treatment assignment, while black dashed lines indicate the baseline analysis bandwidth. The sample consists of an unbalanced panel of firms observed between 2004 and 2010.

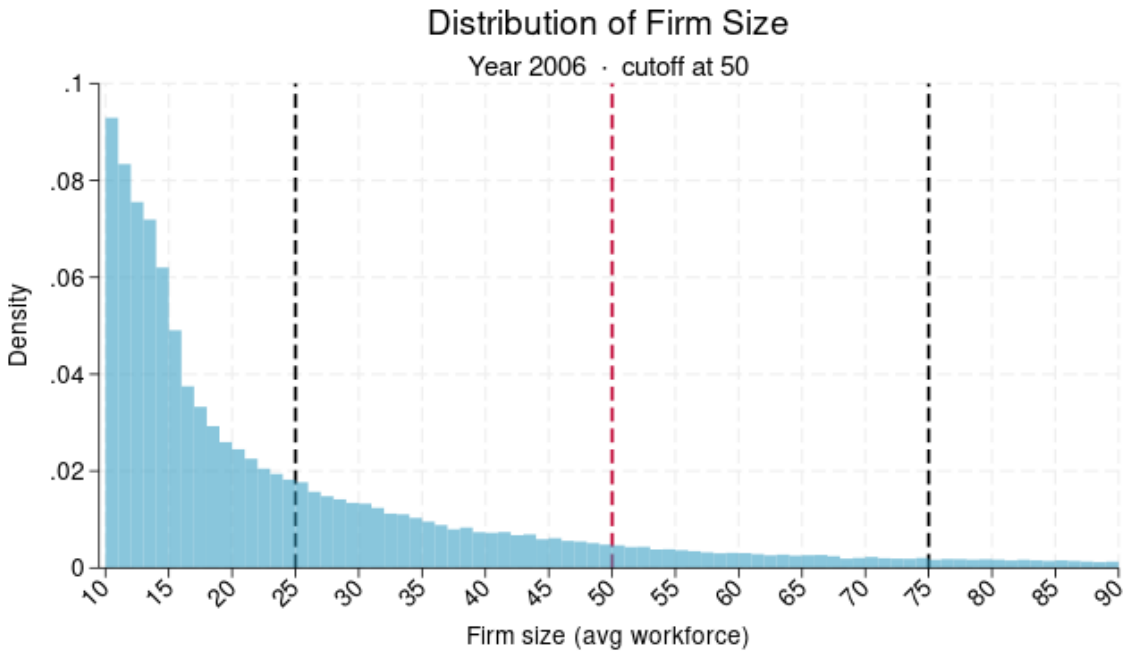
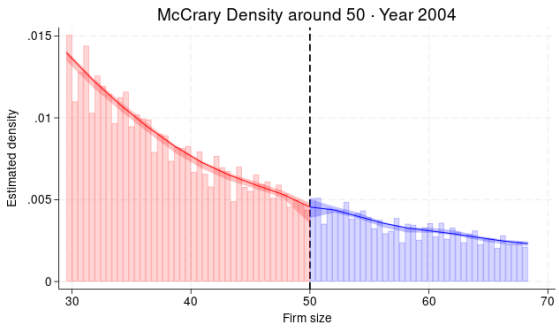
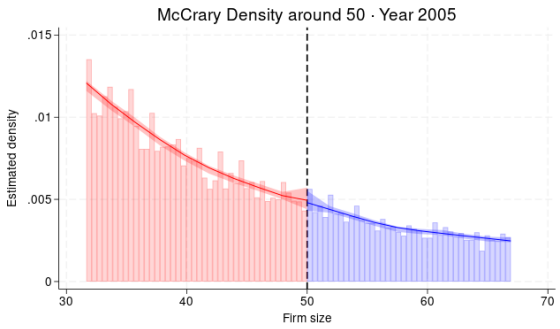


Figure A.2: McCrary Density Tests Around the 50-Employee Cutoff (2004–2009)
Source: INPS *Universo Imprese*.
The figure reports McCrary (2008) density tests based on firms' annual average employment. Each panel displays the estimated discontinuity in the density of firm size at the 50-employee cutoff for a given year. The sample consists of an unbalanced panel of firms observed between 2004 and 2009.

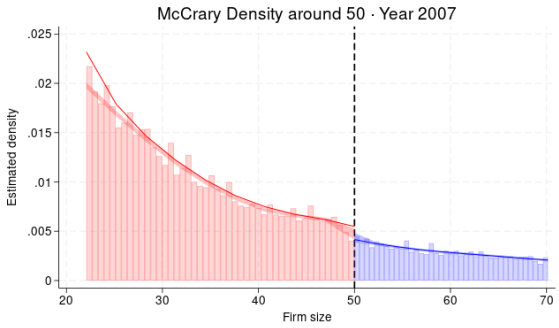
(a) 2004



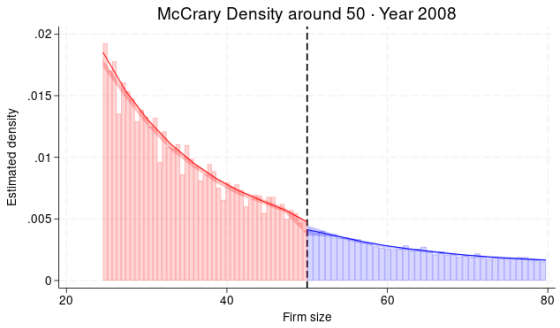
(b) 2005



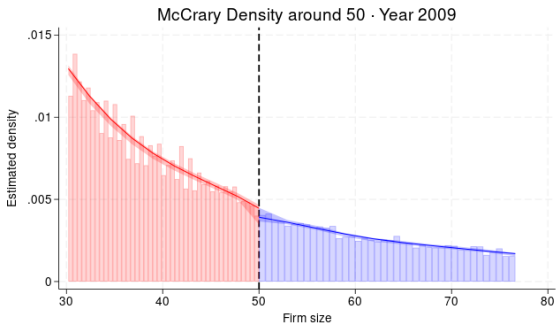
(c) 2007



(d) 2008



(e) 2009



(f) 2010

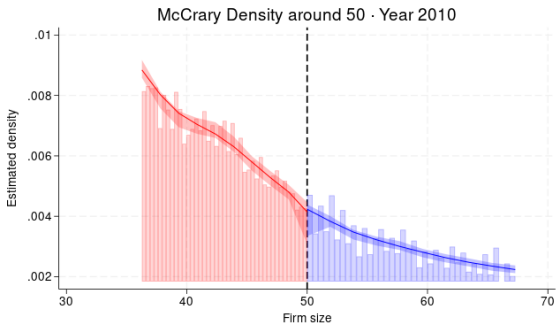
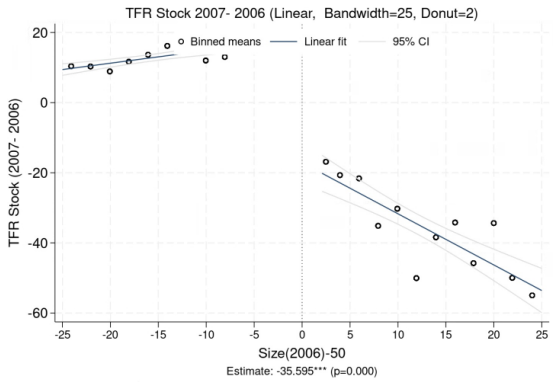
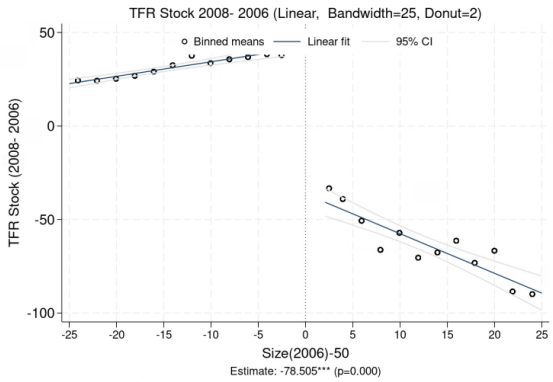


Figure A.3: Local-Linear RDD in First Differences: *TFR* Stock (Δ vs. 2006)
Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.
The figure shows the discontinuity in the change of the outcome variable relative to 2006, estimated using a local-linear RDD with a 25-employee bandwidth around the 50-employee threshold. Binned means and fitted local-linear trends are displayed. Standard errors are clustered at the firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

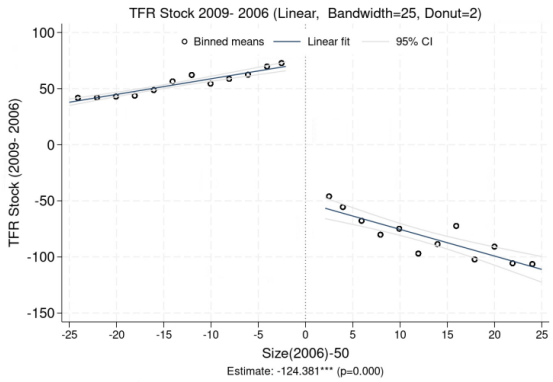
(a) 2007



(b) 2008



(c) 2009



(d) 2010

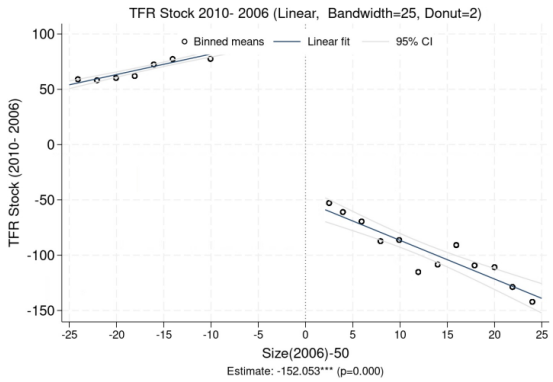
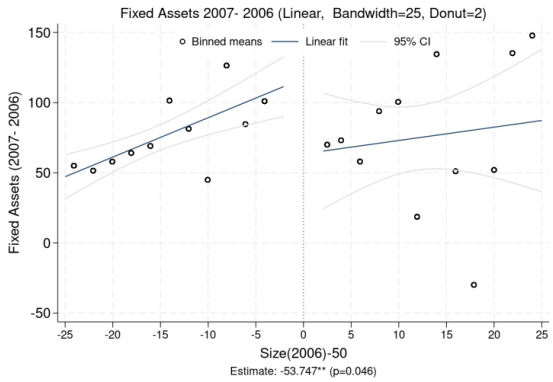
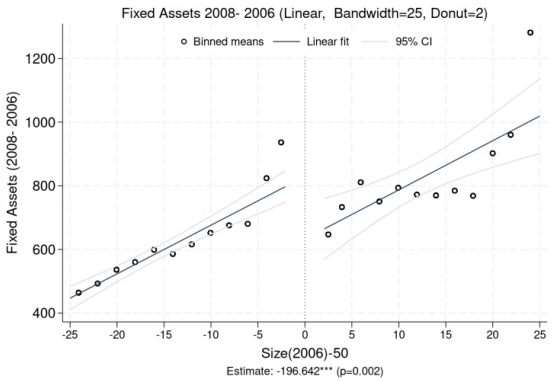


Figure A.4: Local-Linear RDD in First Differences: Fixed assets (Δ vs. 2006)
Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.
The figure shows the discontinuity in the change of the outcome variable relative to 2006, estimated using a local-linear RDD with a 25-employee bandwidth around the 50-employee threshold. Binned means and fitted local-linear trends are displayed. Standard errors are clustered at the firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

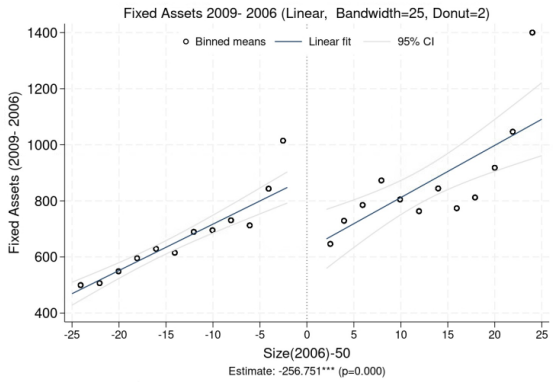
(a) 2007



(b) 2008



(c) 2009



(d) 2010

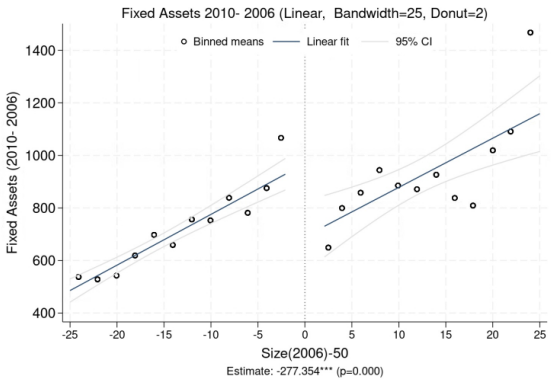
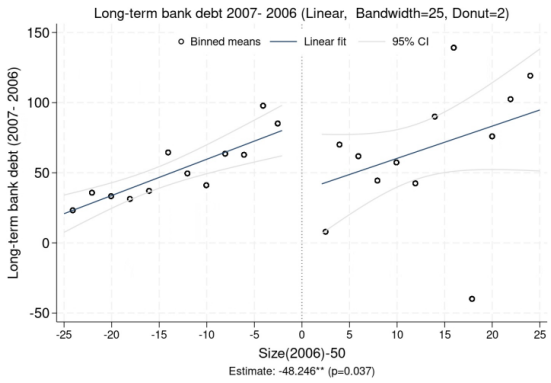
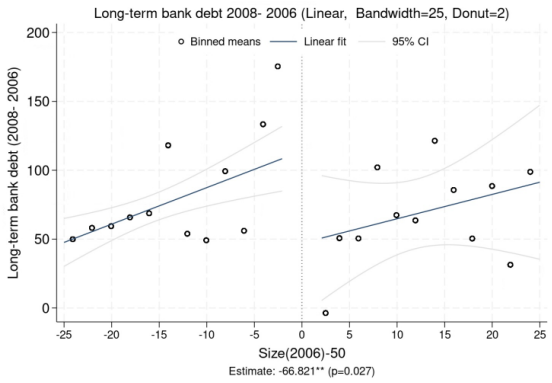


Figure A.5: Local-Linear RDD in First Differences: Long-Term Bank Debt (Δ vs. 2006)
Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.
The figure shows the discontinuity in the change of the outcome variable relative to 2006, estimated using a local-linear RDD with a 25-employee bandwidth around the 50-employee threshold. Binned means and fitted local-linear trends are displayed. Standard errors are clustered at the firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

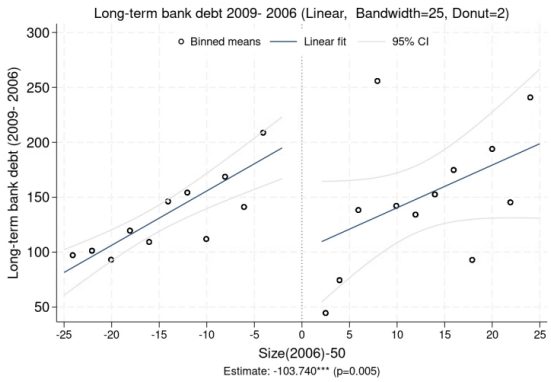
(a) 2007



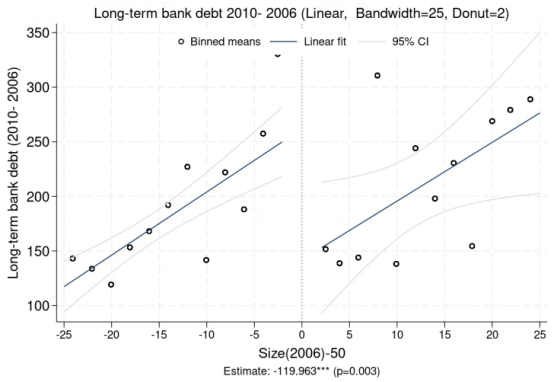
(b) 2008



(c) 2009



(d) 2010



A.2 Tables

Table A.1: Dynamic DiRD - Level Estimates

Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.

Panel A and B report the static and dynamic DiRD coefficients measuring the reform-induced discontinuity at the 50-employee threshold relative to the 2006 baseline. All estimates use a 25-employee bandwidth around the cutoff. Firm FE, Region–year FE, sector–year FE, and flexible size trends included throughout. SE are clustered at the firm level.

	Panel A: First Stage and Factor Inputs				
	(1) TFR Stock	(2) Cash flows	(3) Fixed Assets	(4) Tangible Fixed Assets	(5) Capital-Labor Ratio
Above×2004	1.7	7.6	-2.7	-34.6	-0.1
	(3.8)	(14.4)	(41.0)	(35.6)	(0.1)
Above×2005	4.0	-10.7	-20.9	-39.3	0.0
	(3.1)	(12.5)	(28.1)	(25.4)	(0.1)
Above×2007	-35.2***	-43.2***	-41.7	-28.1	-0.1*
	(3.3)	(14.1)	(27.0)	(22.9)	(0.1)
Above×2008	-76.4***	-33.8*	-159.0***	-118.8**	-0.2*
	(4.7)	(17.6)	(61.7)	(54.5)	(0.1)
Above×2009	-120.4***	-57.9***	-223.5***	-178.9***	-0.3*
	(5.8)	(19.7)	(69.7)	(61.2)	(0.1)
Above×2010	-150.4***	-62.3***	-220.9***	-171.3***	-0.1
	(6.6)	(20.4)	(75.3)	(65.5)	(0.2)
Above × Post	-94.3***	-47.5***	-148.2***	-95.6**	-0.2*
	(4.7)	(13.0)	(54.3)	(47.6)	(0.1)
Observations	204,369	204,369	204,369	204,369	203,184
Obs. Above Cutoff	45,223	45,223	45,223	45,223	44,988
Obs. Below Cutoff	159,146	159,146	159,146	159,146	158,196
Control Mean (2006)	275.46	370.11	1,534.65	1,306.65	2.01
	Panel B: Financing Structure				
	(6) Liquid Assets	(7) LT Bank Debt	(8) ST Bank Debt	(9) Non-Bank Debt	(10) Equity
Above×2004	-8.7	18.3	7.7	-23.3	-14.3
	(16.1)	(24.7)	(35.9)	(25.1)	(37.5)
Above×2005	-7.4	4.6	-18.9	-6.2	8.2
	(13.6)	(19.1)	(28.0)	(20.2)	(26.0)
Above×2007	-36.1**	-36.3*	28.7	-19.9	-25.6
	(14.5)	(22.0)	(30.3)	(20.4)	(29.1)
Above×2008	-38.9**	-59.6**	-15.7	-40.7	-111.3
	(17.0)	(29.5)	(43.2)	(27.2)	(74.2)
Above×2009	-51.6***	-97.3***	-34.9	-32.9	-105.4
	(18.7)	(37.0)	(45.8)	(30.5)	(87.3)
Above×2010	-55.0***	-118.3***	-9.2	-43.2	-73.6
	(20.5)	(41.7)	(51.4)	(32.9)	(99.3)
Above × Post	-39.5***	-83.0***	-2.6	-24.0	-75.8
	(13.2)	(27.9)	(36.3)	(23.1)	(67.5)
Observations	204,369	204,369	204,369	204,369	204,369
Obs. Above Cutoff	45,223	45,223	45,223	45,223	45,223
Obs. Below Cutoff	159,146	159,146	159,146	159,146	159,146
Control Mean (2006)	365.06	450.28	971.48	339.79	1,539.98
	Firm FE: Yes	Industry×Year FE: Yes		Region×Year FE: Yes	

Notes: Bandwidth = 25, Donut = 2. Clustered standard errors at the firm level in parentheses.

Base year: 2006 (normalized to zero). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Dynamic DiRD - Poisson Estimates

Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.

Panel A and B report the static and dynamic DiRD coefficients measuring the reform-induced discontinuity at the 50-employee threshold relative to the 2006 baseline. All estimates use a 25-employee bandwidth around the cutoff. Firm FE, Region–year FE, sector–year FE, and flexible size trends included throughout. SE are clustered at the firm level.

	Panel A: First Stage and Factor Inputs			
	(1) TFR Stock	(2) Fixed Assets	(3) Tangible Fixed Assets	(4) Capital-Labor Ratio
Above×2004	0.005	0.002	-0.016	-0.028
	(0.010)	(0.018)	(0.019)	(0.035)
Above×2005	0.012*	-0.004	-0.016	0.009
	(0.007)	(0.012)	(0.013)	(0.035)
Above×2007	-0.087***	-0.019	-0.013	-0.065*
	(0.008)	(0.011)	(0.011)	(0.036)
Above×2008	-0.181***	-0.052**	-0.039*	-0.090*
	(0.011)	(0.021)	(0.022)	(0.048)
Above×2009	-0.277***	-0.070***	-0.059**	-0.097**
	(0.013)	(0.024)	(0.024)	(0.049)
Above×2010	-0.337***	-0.073***	-0.058**	-0.046
	(0.015)	(0.026)	(0.026)	(0.058)
Above × Post	-0.221***	-0.054***	-0.032	-0.069*
	(0.011)	(0.020)	(0.020)	(0.041)
Observations	203,047	204,134	203,786	202,954
Obs. Above Cutoff	44,911	45,181	45,112	44,946
Obs. Below Cutoff	158,136	158,953	158,674	158,008
Control Mean (2006)	277.53	1,536.90	1,311.45	2.01
	Panel B: Financing Structure			
	(5) Liquid Assets	(6) LT Bank Debt	(7) ST Bank Debt	(8) Non-Bank Debt
Above×2004	-0.008	0.025	0.004	-0.047
	(0.034)	(0.040)	(0.026)	(0.047)
Above×2005	-0.013	0.013	-0.015	-0.011
	(0.028)	(0.030)	(0.020)	(0.038)
Above×2007	-0.068**	-0.062**	0.014	-0.044
	(0.029)	(0.030)	(0.019)	(0.036)
Above×2008	-0.074**	-0.086**	-0.011	-0.075
	(0.035)	(0.039)	(0.024)	(0.046)
Above×2009	-0.096***	-0.116***	-0.025	-0.067
	(0.037)	(0.044)	(0.028)	(0.051)
Above×2010	-0.099**	-0.130***	-0.004	-0.091*
	(0.039)	(0.047)	(0.031)	(0.054)
Above × Post	-0.077***	-0.110***	-0.002	-0.051
	(0.027)	(0.035)	(0.022)	(0.040)
Observations	204,073	141,658	179,531	150,348
Obs. Above Cutoff	45,169	32,227	40,446	35,770
Obs. Below Cutoff	158,904	109,431	139,085	114,578
Control Mean (2006)	365.69	667.98	1,118.27	474.84
Firm FE: Yes	Industry×Year FE: Yes	Region×Year FE: Yes		

Notes: Bandwidth = 25, Donut = 2. Clustered standard errors at the firm level in parentheses.
We omit cash flow and equity due to the presence of negative observations,
as the Poisson estimation is strictly defined for non-negative outcomes.
Base year: 2006 (normalized to zero). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Dynamic DiRD – Placebo Cutoff Tests

Source: CERVED – INPS (*Universo Imprese*) – Credit Registry.

Table reports year-specific Dynamic DiRD coefficients at the placebo cutoffs of 40 and 60 employees in 2006. All estimates use a 25-employee bandwidth around the cutoff. Firm FE, Region–year FE, sector–year FE, and flexible size trends included throughout. SE are clustered at the firm level.

	(1) TFR Stock	(2) Cash flows	(3) Fixed Assets	(4) LT Bank Debt	(5) Liquid Assets
	Panel A: Placebo Cutoff at 40				
Above×2004	1.7	-6.5	-1.4	7.1	4.0
	(2.7)	(10.1)	(28.3)	(17.7)	(11.7)
Above×2005	-1.9	5.7	-13.0	5.2	3.6
	(2.0)	(8.6)	(19.1)	(13.3)	(9.7)
Above×2007	17.0***	17.4*	15.5	30.1*	5.9
	(2.3)	(9.5)	(19.9)	(15.6)	(9.9)
Above×2008	32.3***	14.6	14.1	14.6	7.5
	(3.0)	(12.1)	(42.7)	(21.0)	(12.1)
Above×2009	51.7***	4.0	33.4	44.7*	21.8
	(3.7)	(13.7)	(47.0)	(25.8)	(13.4)
Above×2010	65.6***	14.8	58.0	54.2*	14.4
	(4.5)	(13.9)	(51.2)	(29.4)	(14.7)
Observations	372187	372187	372187	372187	372187
Obs. Above Cutoff	68916	68916	68916	68916	68916
Obs. Below Cutoff	303271	303271	303271	303271	303271
Control Mean (2006)	178.98	245.07	1049.68	304.84	269.71
	Panel B: Placebo Cutoff at 60				
Above×2004	4.2	14.3	82.7	13.8	-0.9
	(5.9)	(19.4)	(55.3)	(32.8)	(20.9)
Above×2005	2.3	17.1	57.3	16.4	-13.4
	(4.7)	(16.3)	(36.6)	(24.7)	(17.4)
Above×2007	-15.1***	-42.7**	-40.5	0.9	31.0*
	(5.1)	(17.8)	(38.3)	(29.1)	(18.0)
Above×2008	-20.8***	-22.5	-90.8	10.2	31.9
	(6.6)	(22.4)	(79.2)	(38.5)	(21.7)
Above×2009	-25.3***	-3.4	-57.7	-11.9	-1.6
	(8.1)	(26.1)	(85.7)	(46.5)	(24.2)
Above×2010	-38.2***	-7.9	-93.1	-13.3	12.4
	(9.3)	(26.6)	(93.3)	(52.3)	(26.6)
Observations	127698	127698	127698	127698	127698
Obs. Above Cutoff	33056	33056	33056	33056	33056
Obs. Below Cutoff	94642	94642	94642	94642	94642
Control Mean (2006)	365.28	487.25	1982.40	576.81	444.58
Firm FE: Yes	Industry×Year FE: Yes	Region×Year FE: Yes			

Notes: Bandwidth = 25, Donut = 2. Clustered standard errors at firm level in parentheses.

Base year: 2006 (normalized to zero). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.