The Marginal Propensity to Invest: Evidence from Italian Public Debt Settlements[‡]

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PRELIMINARY AND INCOMPLETE. NOT FOR CIRCULATION.

Abstract

How much do firms invest out of an additional dollar of cash flow? We answer this question by exploiting a natural experiment in Italy: a 2013 reform that released over €57 billion to settle overdue public-sector debts with private suppliers. Because the policy accelerated payment on obligations already incurred, it injected liquidity into firms without affecting their demand. We combine administrative income-statement data with the quasi-universe of public procurement contracts; this allows us to construct a shift-share instrument based on firms' exposure to public bodies that received different amounts of funding to settle their debts. We estimate a marginal propensity to invest (MPI) of 0.22–0.29: for each additional euro of liquidity, firms increase investment by up to 30 cents. We document large heterogeneity in MPIs: the investment response is stronger for highly leveraged firms and in capital-intensive sectors. Our estimates provide direct empirical discipline for macroeconomic models with heterogeneous firms and carry implications for the design of stabilization policies that operate through firms' cashflows.

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1 Introduction

Business investment is a key driver of aggregate fluctuations and a central channel through which macroeconomic shocks propagate. The sensitivity of firm investment to cash flows —the marginal propensity to invest— is therefore a crucial quantity for understanding the transmission of shocks to the real economy and for designing stabilization policies (Winberry et al., 2025).

Under the standard neoclassical framework, investment is determined by the expected marginal product of capital, and transitory movements in cash flows should have no effect on capital expenditure, holding investment opportunities fixed (Jorgenson, 1963, Tobin, 1969, Hayashi, 1982). Yet, a large class of models featuring financial frictions predicts that firms facing binding borrowing and equity issuance constraints will exhibit a positive sensitivity of investment to cash flows, as liquidity relaxes financial constraints that would otherwise prevent firms from undertaking profitable projects (Kiyotaki and Moore, 1997, Hubbard, 1998, Bernanke et al., 1999, Ottonello and Winberry, 2020).

Despite the importance of this question, credible empirical estimates of the causal effect of cash flows on investment have proved elusive. The central challenge is that firm-level variation in cash flows is typically correlated with shifts in demand, productivity, or other fundamental determinants of investment opportunities. A revenue windfall, for instance, may simultaneously signal strong future demand, making it difficult to isolate a pure cash-flow channel. As a result, OLS estimates of the investment–cash flow sensitivity are likely to be contaminated by omitted variable bias, and the extensive literature following Fazzari et al. (1988) has been subject to long-standing identification concerns (Kaplan and Zingales, 1997).

In this paper, we make progress on this question by exploiting a natural experiment that produced large and plausibly exogenous cash-flow shocks to Italian firms. In 2013, the Italian government enacted a reform that made available over €57 billion to local public administrations to settle outstanding commercial debts with private-sector suppliers. Because these resources were earmarked for payments on obligations already incurred, this reform constituted a pure liquidity injection that increased firms' cash holdings without directly affecting their current or future demand.[1]

We combine administrative income statements data on the universe of Italian limited liability companies with records on the quasi-universe of Italian public procurement contracts to construct a shift-share instrument that exploits variation in firms' pre-reform reliance on public administration bodies differentially exposed to the reform.

[1]The reform provided resources in the form of loans and hence did not generate additional spending capacity for public administrations, since these were subject to strict fiscal rules during the period we study.

Firms with greater exposure to treated bodies experienced a sharp increase in cash holdings but no corresponding rise in sales, confirming that the reform operated as a pure cash-flow shock and did not induce shifts in demand. Firm investment also rose significantly among exposed firms in the post-reform period, closely tracking the liquidity response and suggesting a Marginal Propensity to Invest (MPI) larger than zero.

To translate these reduced-form results into quantitative estimates of the MPI, we employ an instrumental variable approach. We estimate an elasticity of investment with respect to liquidity ranging from 0.62 to 0.78, implying an MPI between 0.22 and 0.29. For each additional euro of liquidity received, firms increased investment by up to 30 cents.

Because the reform affected a large cross-section of firms, our setting is well-suited to studying heterogeneity in MPIs along a number of different dimensions, thereby offering discipline for models of firm investment. Consistent with the predictions of models with financial frictions, we find that MPIs increase with firm leverage: highly leveraged firms display MPIs nearly an order of magnitude as large as those of firms at the bottom of the leverage distribution. We also document sizeable sectoral heterogeneity: firms operating in capital-intensive sectors exhibit significantly higher MPIs than those in labor-intensive sectors, consistent with a greater scope for adjusting the capital stock in response to a liquidity windfall.

Related literature. We contribute to two strands of literature. First, the literature characterizing the determinants of the transmission of macroeconomic shocks. A large and rapidly growing body of work has emphasized the role of household heterogeneity and the marginal propensity to consume as central objects governing how shocks propagate (McKay and Reis, 2016, McKay et al., 2016, Kaplan et al., 2018, Auclert et al., 2024). In turn, this has spurred extensive efforts to estimate marginal propensities to consume in the data (Parker et al., 2013, Boehm et al., 2025, Lewis et al., 2024, Colarieti et al., 2024). Although investment has long been recognized as a crucial determinant of economic fluctuations (Christiano et al., 2005, Auclert et al., 2020), the literature on the role of firm heterogeneity for aggregate dynamics remains comparatively underdeveloped, with some notable exceptions (Senga et al., 2017, Ottonello and Winberry, 2020, Winberry, 2021, Jeenas, 2025, Koby and Wolf, 2020, Winberry et al., 2025, Bellifemine et al., 2022, 2026). On the firm side, the marginal propensity to invest plays an analogous role to the marginal propensity to consume. Yet credible estimates of this object remain virtually absent, in large part

because of the identification challenges we discuss below.[2] We provide such estimates and characterize how they vary along dimensions predicted by workhorse models of firm investment.

In doing so, we also contribute to the long-standing debate on the role of financial frictions in firm investment. A central prediction of models with borrowing constraints is that financially constrained firms' investment should respond to transitory movements in cash flows (Kiyotaki and Moore, 1997, Bernanke et al., 1999). Testing this prediction empirically has been the subject of a long and contentious debate. Fazzari et al. (1988) documented a positive correlation between investment and cash flows, which they interpreted as evidence of financing constraints, but Kaplan and Zingales (1997) challenged this interpretation, and subsequent work showed that a positive investment–cash flow sensitivity can arise even in the absence of frictions (Erickson and Whited, 2000).[3] The fundamental problem is that firm-level variation in cash flows is typically correlated with investment opportunities, making it difficult to isolate the causal effect of liquidity on investment. Blanchard et al. (1994) characterize the response of firms' choices to cash windfalls, but they rely on a limited sample of 11 firms. We overcome this identification challenge by exploiting a reform that delivered a large, plausibly exogenous liquidity shock to a broad cross-section of firms without directly affecting their current or future demand conditions.

Outline. The rest of the paper is organized as follows. Section 2 describes the institutional context and the data used in our analysis. Section 3 presents our main empirical results and estimates for MPIs. Section 4 (work in progress) discusses the implications of our estimates for workhorse macroeconomic models of firm investment. Section 5 concludes.

2 Empirical Approach

In this section, we first discuss the institutional context that we leverage in empirical setup. We then describe the data used in our analysis.

[2]Some notable exceptions include Martin-Baillon (2021), who uses the semi-structural method developed in Blundell et al. (2008) to estimate MPIs, Melcangi (2019), who estimates the marginal propensity to hire out of cash flows, and Caggese and Mesters (2024), who develop a structural approach using sign restrictions to isolate pure liquidity shocks from standard balance sheet data.

[3]See Hubbard (1998) for a survey of this literature.

2.1 Institutional Context: The 2013 "Sblocca Debiti" Reform

Estimating MPIs requires a shock that affects firms' liquidity without simultaneously shifting their current or expected demand. Italy's 2013 settlement of overdue public-sector debts provides an ideal setting. We now describe the origins of the policy, its design, and the institutional features that make it well-suited for our identification strategy.

Background. Italy has historically been characterized by severe delays in public-sector payments to private suppliers. As of 2011, the average payment time for publicly procured goods and services was approximately 180 days, with delays beyond contractual terms averaging 90 days—among the longest in the European Union.[4] By the end of 2012, this had resulted in a stock of commercial debts owed by public administrations equivalent to 5.8% of Italian GDP, of which roughly 57% was overdue (Banca d'Italia, 2018).

In February 2011, the European Union adopted a directive requiring public authorities to settle invoices within 30 days.[5] In November 2012 the Italian government transposed this directive, with the new payment terms applying to contracts concluded from 2013 onward.[6]

The reform. Crucially, the 2012 directive only applied to the flow of *new* debts. To clear the backlog of the inherited *stock* of arrears, the Italian government enacted the *Decreto Sblocca Debiti* in April 2013, initially allocating approximately €40 billion over 2013–2014, later expanded to roughly €57 billion through subsequent decrees.[7] Resources were channeled through two mechanisms: relaxation of internal fiscal-rule constraints, which unlocked administrations' existing cash balances, and liquidity advances from Cassa Depositi e Prestiti and the Treasury to cash-strapped bodies. Eligibility was restricted to debts that were certain, liquid, and payable as of 31 December 2012 and payment priority followed a seniority rule, with older debts settled first. By late November 2013, €16.3 billion had reached final creditors, with an additional €8.1 billion already disbursed to debtor administrations awaiting final payment.[8]

Figure 1 illustrates the scale of the program. The stock of overdue commercial public-administration commercial debt, which had peaked at 3.3 percent of GDP in 2012 during

[4] See Intrum Justitia (2012).

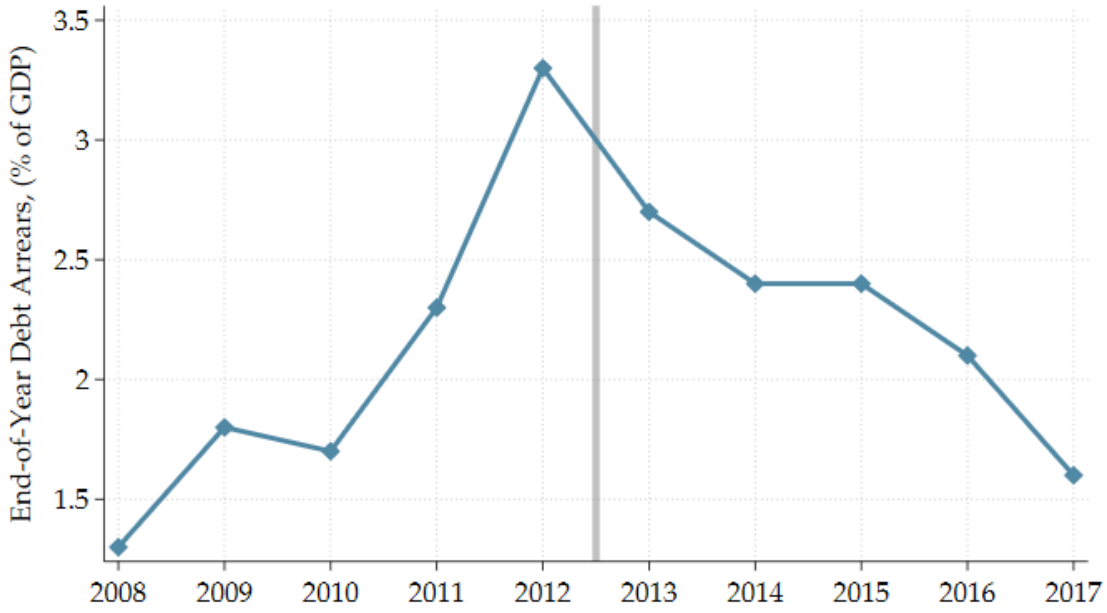
[5] Directive 2011/7/EU of 16 February 2011 on combating late payment in commercial transactions.

[6] Legislative Decree 192/2012 (D.Lgs. 192/2012), enacted on 9 November 2012.

[7] Decree-Law 35/2013, subsequently converted into Law 64/2013 on 6 June 2013. Subsequent decrees (DL 102/2013 and DL 66/2014) expanded the available resources.

[8] Source: Italian Ministry of Finance. See also D'Aurizio and Depalo (2016) for a more detailed description of the institutional setting.

Figure 1: Public Administration's Commercial Debt in Arrears



Note: end-of-year commercial debt in arrears owed by Italian public administration bodies, as a share of GDP. The grey vertical line shows the timing of the reform we study in this paper. Source: Banca d'Italia (2018)

the sovereign debt crisis, declined by more than half in the years following the reform.

The reform as a source of identification. Figure 1 suggests that the reform was effective in disbursing liquidity to private-sector creditors, providing a promising source of firm-level cash-flow variation with which to estimate the marginal propensity to invest. A natural concern, however, is whether administrations could have used the freed-up fiscal space to finance new procurement, thereby confounding the liquidity channel with demand effects. Several features of the institutional design made this unlikely. First, the resources made available to each administration were earmarked for individually identified claims payable as of 31 December 2012, and the Ministry of Finance only revised pre-existing spending ceilings on the basis of these verified claims. Second, upon receiving funds, administrations were required to immediately extinguish the listed debts and provide formal certification of each payment to the relevant audit body. The Ministry of Finance monitored disbursements with biweekly frequency. Non-compliant administrators faced personal fines and referral to the national audit court. Third, the liquidity advances provided were structured as loans, not transfers, requiring repayment by public bodies. Together, these features of the institutional design ensured that the reform raised

firms' liquid resources without generating additional demand for their output.

Our identification strategy exploits this setting by leveraging the fact that different firms were differentially exposed to the reform. Firms varied in their pre-reform reliance on different public administration bodies, and these bodies in turn received varying allocations under the decree. This heterogeneity in exposures generates plausibly exogenous firm-level variation in the size of the liquidity shock, which we formalize through the shift-share instrument described next.

2.2 Data

Our analysis combines three different data sources.

Company Accounts Data System. We obtain firm-level balance sheets and income statements for the universe of Italian limited liability companies from the Company Accounts Data System (henceforth, CADS). CADS is a proprietary database maintained by Cerved Group S.p.A. that compiles information from administrative records at the Italian Registry of Companies and from financial statements filed with the Chambers of Commerce. By law, firms file these statements annually. Our primary outcomes of interest are investment, liquidity, and labor expenses. We also draw on information regarding each firm's province of incorporation, leverage, and two-digit NACE sector classification.

Public Procurement Contracts. To construct a measure of firm exposure to different public administration bodies we rely on the data constructed in Bellifemine et al. (2026), which covers the quasi-universe of public procurement contracts in Italy with a nominal value above 40,000€. We extend the data to begin in 2007.[9] Importantly for our analysis, these data include identifiers for both the public body awarding the contract as well as the winning firm, together with the monetary value of the contract. Public procurement accounts for a substantial share of government activity, representing 36.3% of government consumption and 6.9% of GDP over the period 2012-2022.

Italian Ministry of Finance. To measure how different public administrations were affected by the reform, we scrape data from the Italian Ministry of Finance website on the resources made available to each public administration body to settle outstanding debts under the reform we study. These data allow us to observe the allocation of funds to

[9] Coverage before 2010 is limited.

each individual public body and hence to quantify each administration's exposure to the policy.

2.3 Constructing the Instrument

Our empirical strategy relies on a shift-share instrument that exploits variation in firms' pre-reform reliance on different public administration bodies, combined with differences in the resources each administration received under the reform. We describe each component in turn.

The shifts. We define the shift g_k for public administration k as the amount of resources made available under the reform to settle outstanding commercial debts, expressed as a share of the total value of procurement contracts awarded by administration k before 2013. This ratio captures the intensity of the liquidity injection relative to the scale of each administration's procurement activity. Administrations that received larger allocations relative to their contracting volume represent a stronger "treatment" for the firms they transact with.

The shares. We construct a measure of exposure of firm i to public administration k, s_{ik}, as the share of revenues from public procurement contracts awarded to firm i before 2013 coming from public administration k:

$$s_{ik} = \frac{Sales_{ik}}{\sum_{k'=1}^{K} Sales_{ik'}} \tag{1}$$

The instrument. As standard, we construct our shift-share instrument as a firm-level average of the public administration shifts g_k, weighted by firms exposure to public bodies s_{ik}:

$$z_i = \sum_{k'=1}^{K} s_{ik'} g_{k'} \tag{2}$$

Intuitively, z_i is larger for firms that derived a greater share of their pre-reform procurement revenues from administrations that received relatively generous allocations under the decree. We normalize z_i to have unit standard deviation to facilitate interpretation of the estimated coefficients.

Discussion of identification. Our identification strategy relies on three conditions.

The first is relevance: the instrument must affect firms' liquidity. We verify this condition in the next section, where we document a strong first-stage relationship.

The second is parallel trends: conditional on our controls, firms with different values of the instrument z_i would have exhibited similar investment and liquidity trajectories absent the reform. As we show below, we find no evidence of differential pre-trends in our event studies, consistent with the parallel trends assumption and suggesting that the reform was unanticipated.

The third condition is the exclusion restriction, requiring that z_i affects firm investment only through its effect on firm liquidity, conditional on our battery of controls. The most direct violation would be if the reform also shifted firms' current or expected future demand. We argued above that the institutional design of the reform rules this out, and we present further supporting evidence for this in the next section. Given our shift-share setting, we rely on the exogeneity in the shifts for identification, following Borusyak et al. (2022). Because the allocation of resources underpinning g_k was determined by the stock of each public administration's commercial debts, cross-sectional variation in this quantity reflects the interaction of several institutional factors on the public-sector side.[10] Crucially, these determinants of g_k are features of each public administration's institutional and fiscal environment, not of the firms that happened to supply goods and services to it. The most natural remaining threat is that firms sort non-randomly across public bodies along sectoral or geographic lines, which could induce a correlation between z_i and unobserved determinants of investment. We address this by showing our results are robust to the inclusion of sector$\times$time and province$\times$time fixed effects.

3 Empirical Results

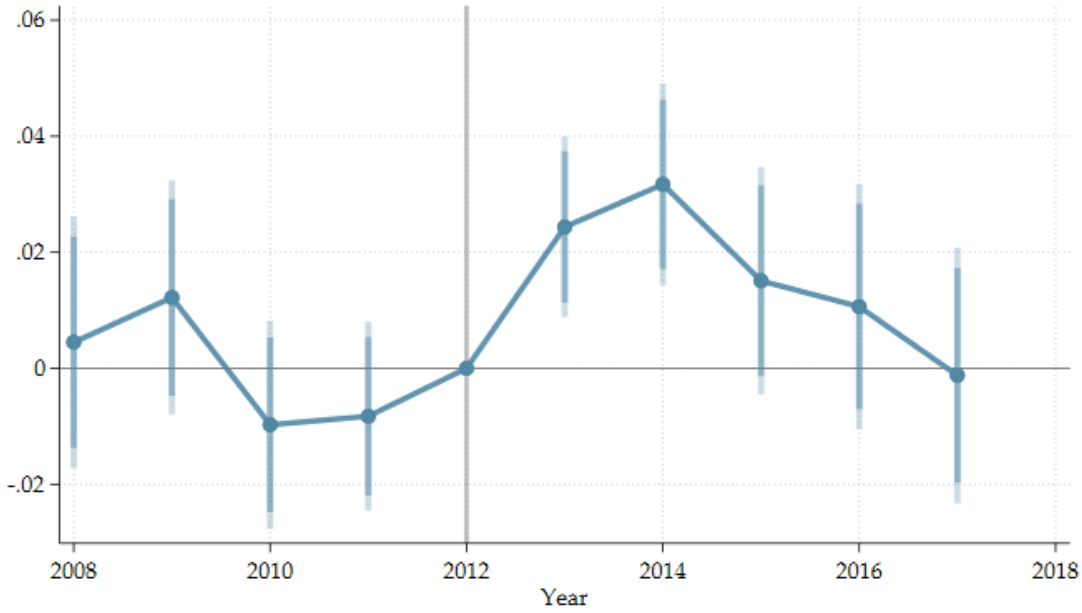
In this section we present our main empirical results. First, we show reduced-form event studies for the response of firm liquidity and investment to our natural experiment. Next, we turn to an instrumental variable specification to quantify MPIs. Finally, we explore heterogeneity in MPIs.

3.1 Reduced Form

We begin by documenting the dynamic effects of the reform on firm outcomes using an event-study design. This approach serves two purposes: it allows us to assess whether

[10] These include local fiscal capacity, administrative efficiency, and and the accumulated history of payment delays.

Figure 2: Response of Firm Liquidity



Note: estimated $\widehat{\beta}_h$ from the event study design in (3). The y-axis denotes the change in liquidity (relative to the base year 2012) in percentage points. Wide and thin lines represent 90% and 95% confidence bands, respectively. We cluster standard errors at the firm level.

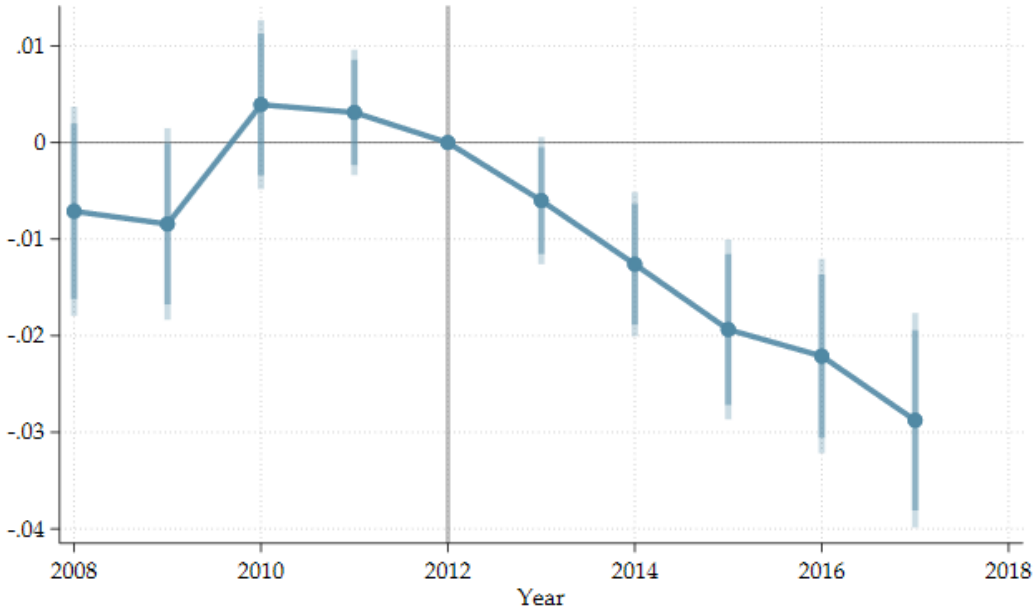
firms with different levels of exposure to the reform were on parallel trends prior to the policy, and it traces out the timing and persistence of the response. Specifically, we estimate the following specification:

$$\log Y_{it} = \alpha_i + \delta_t + \sum_{h=2008, h\neq 2012}^{2017} \beta_h \left(\mathbb{1}_{h=t} \times z_i\right) + \varepsilon_{it}, \tag{3}$$

where Y_{it} is our variable of interest (either liquidity or investment), α_i is a firm fixed-effect, δ_t is a time fixed effect and z_i is the shift-share instrument defined in (2) above. The coefficients of interest, β_h capture the differential response of firms with higher exposure to the reform in year h relative to the base year 2012. Standard errors are clustered at the firm level.

Figure 2 plots the estimated $\widehat{\beta}_h$ with firm liquidity as the dependent variable. Since liquidity is the channel through which the reform is expected to affect firm behavior, this specification serves as a first stage, capturing the direct effect of the debt repayment program on firms' cash availability. Two features of the figure are worth noting. First, the pre-reform coefficients are close to zero and statistically insignificant, consistent with the absence of differential trends in liquidity across firms with different exposure prior to the

Figure 3: Response of Credit versus Customers



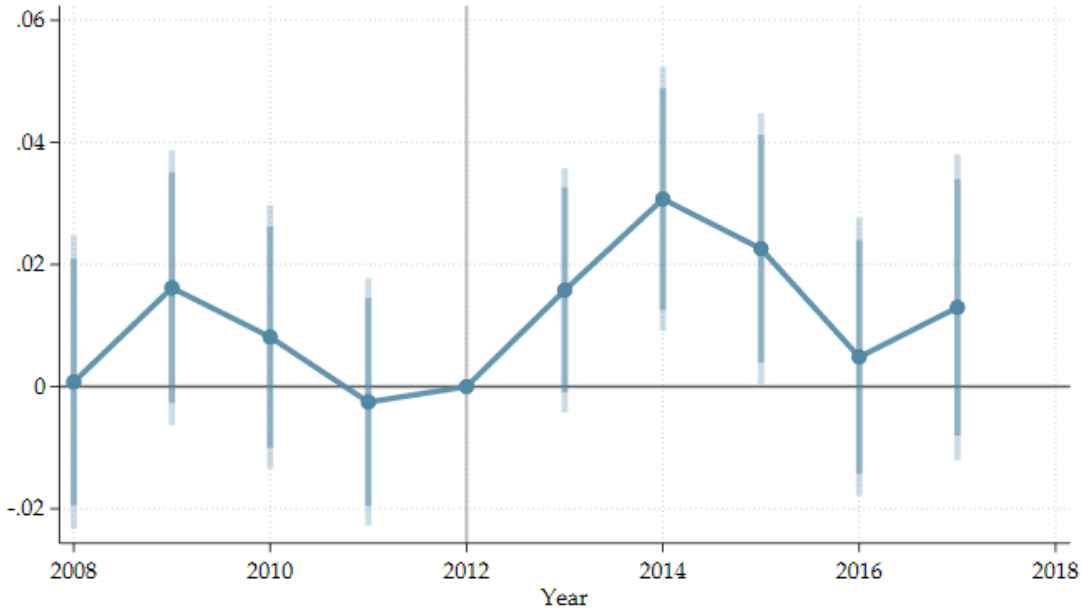
Note: estimated $\widehat{\beta}_h$ from the event study design in (3). The y-axis denotes the change in outstanding credit (relative to the base year 2012) in percentage points. Wide and thin lines represent 90% and 95% confidence bands, respectively. We cluster standard errors at the firm level.

policy. Second, liquidity rises sharply for more exposed firms in the aftermath of the reform, as public administrations began settling their outstanding commercial debts. The effect is economically significant: at peak, a one-standard-deviation increase in z_i raises liquidity by approximately 3%, and the effect persists for up to three years before gradually fading.

Figure 3 describes the mechanism behind the increase in liquidity, by showing estimates of (3) with trade receivables as the dependent variable. Consistent with the nature of the reform, outstanding trade credits decline for firms more exposed to public administrations that received larger allocations to settle their commercial debts. This confirms that the reform operated through the intended channel: public administrations used the funds to pay down arrears owed to their private-sector suppliers, reducing the stock of receivables on firms' balance sheets and converting them into liquid assets. This decline in receivables documented in Figure 3 is thus the mirror image of the liquidity increase in Figure 2.

Finally, Figure 4 presents the event-study estimates for our primary outcome of interest: firm investment. This specification constitutes the reduced form of our analysis, linking the reform directly to our primary outcome of interest. The pattern closely mirrors

Figure 4: Response of Firm Investment



Note: estimated $\widehat{\beta}_h$ from the event study design in (3). The y-axis denotes the change in investment (relative to the base year 2012) in percentage points. Wide and thin lines represent 90% and 95% confidence bands, respectively. We cluster standard errors at the firm level.

that of liquidity: the pre-reform coefficients are again small and statistically insignificant, consistent with the absence of differential pre-trends in investment. Following the reform, investment rises markedly for more exposed firms, with the response building gradually and reaching a peak of approximately 3% for a one-standard-deviation increase in z_i. The effect persists for up to three years before dissipating, closely tracking the dynamics of the liquidity response. The joint effect of the reform on firm liquidity and investment suggests an MPI larger than zero, which we quantify formally in the next subsection.

Robustness. In Appendix A, we validate the interpretation that the reform operated as a pure cash-flow shock rather than through a demand channel. First, we show that procurement expenditures by public administrative bodies did not respond to the reform, consistent with the institutional design of the program, that made resources available in the form of loans to settle pre'existing debts and did not increase spending capacity for future projects. Second, we find that firm revenues were similarly unaffected by the reform, displaying flat behavior both prior and after 2012.

Table 1: Marginal Propensity to Invest

	Narrow Window			Broad Window		
	(1)	(2)	(3)	(4)	(5)	(6)
log Liquidity$_{it}$	0.775***	0.758***	0.680**	0.767***	0.737***	0.620**
	(0.233)	(0.243)	(0.299)	(0.195)	(0.215)	(0.260)
Kleibergen-Paap F-stat.	25.09	22.67	14.26	31.99	25.81	15.89
Observations	93,452	93,416	93,409	141,466	141,414	141,404
Year FEs	✓	✓	✓	✓	✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓
Province × Year FEs		✓	✓		✓	✓
Sector × Year FEs			✓			✓
MPI	0.291	0.284	0.255	0.288	0.276	0.233

Note: instrumental variable estimates from (4). The first column "Narrow window" reports estimates based on the 2011-2014 sample. The second column "Broad window" reports estimates based on the wider 2010-2015 sample. The row MPI reports the implies MPI computed according to (5). Standard errors in parenthesis clustered by firm.

3.2 IV: Estimating MPIs

The reduced-form results above suggest that the reform raised investment through a liquidity channel, but they do not pin down the magnitude of the marginal propensity to invest. To do so, we now estimate an instrumental variable (IV) regression. Specifically, we run the following specification:

$$\log Y_{it} = \alpha_i + \delta_t + \gamma \log \widehat{Liquidity}_{it} + u_{it}, \tag{4}$$

where $\log \widehat{Liquidity}_{it}$ denotes log liquidity instrumented with the shift-share instrument interacted with a post-reform dummy, $z_i \times Post_t$, and all other variables are defined as before. Standard errors are clustered at the firm level.

The estimated $\widehat{\gamma}$ from (4) represents an *elasticity* of log investment to log liquidity. To translate this into a dollar-MPI we multiply it by the average investment-to-liqudity ratio $\varrho \equiv \frac{1}{I}\sum_{i=1}^{I} \frac{Investment_i}{Liquidity_i}$ in our sample:

$$\widehat{MPI} = \widehat{\gamma} \times \varrho \tag{5}$$

Table 1 presents the results. Columns (1) to (3) are based on a narrow time window (2011–2014) around the reform, while columns (4) to (6) use a broader window (2010–

2015). Within each panel, we progressively saturate the model with province×year and 2-digit sector×year fixed effects. The instrument is strong throughout, with the Kleibergen-Paap F-statistic ranging from 14 to 32. Across specifications, we estimate an elasticity of investment with respect to liquidity of approximately 0.62–0.78, consistent with a back-of-the-envelope comparison of the reduced-form and first-stage estimates in Figures 2 and 4. The point estimates are largest in the baseline specification and decline somewhat as we add finer fixed effects, though they remain statistically significant throughout. Combining these elasticities with the average investment-to-liquidity ratio in our sample, the implied MPI ranges from approximately 0.22 to 0.29, as reported in the bottom row of Table 1.

Robustness. One concern with our baseline specification is that instrumenting liquidity may introduce an upward bias to our estimates of MPIs. If firms channel part of the liquidity injection into investment, the first-stage effect on measured liquidity will be attenuated, inflating the IV estimate. To address this, in Appendix A we re-estimate the IV specification using credits to customers as the endogenous variable, which is not mechanically affected by investment spending. The resulting MPIs are in the same range as those in Table 1, between 0.15 and 0.26 depending on the specification. In Tables A.2 and A.3 we also report the first stage estimates as well as the estimates from a simple OLS regression of investment on liquidity.

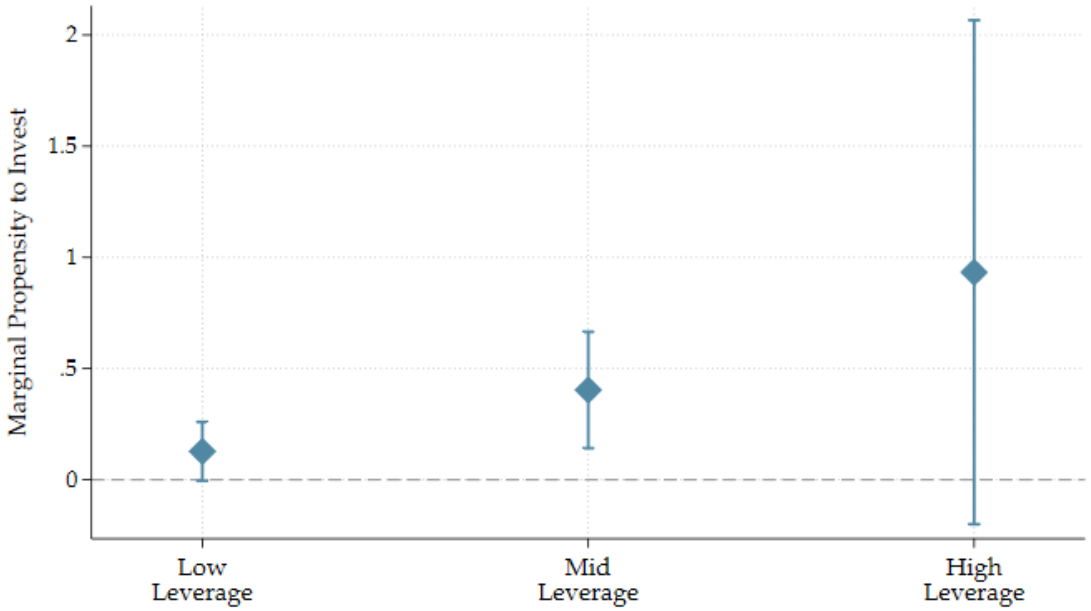
3.3 Heterogeneity in Marginal Propensities to Invest

We now explore heterogeneity in the marginal propensity to invest along two dimensions: firm-level financial constraints and sectoral characteristics.

Financial Frictions and MPI Heterogeneity. A central prediction of canonical models of firm investment under financial frictions is that the sensitivity of investment to cash flows should be highest among financially constrained firms. To test this prediction, we split firms into terciles based on their within-sector leverage and re-estimate (4) separately for each subsample, computing the implied MPI according to (5).[11] Figure 5 plots the results. The estimated MPIs increase monotonically with leverage: firms in the bottom tercile invest roughly €.12 per additional euro of liquidity, compared with approximately €.42 for the middle tercile and €.93 for the top tercile. The point estimate for the most leveraged firms is, however, imprecisely estimated, with a confidence interval that spans zero. The overall pattern is nonetheless consistent with the view that financial frictions amplify the

[11] We define leverage bins within two-digit NACE sectors to account for systematic differences in capital structure across industries.

Figure 5: MPIs and Financial Frictions



Note: MPIs estimates by leverage. We split our sample in 3 bins based on leverage. We then re-estimate (4) on each subsample and compute MPIs according to (5) for each of them. Bands show 90% confidence intervals. Standard errors are clustered at the firm level.

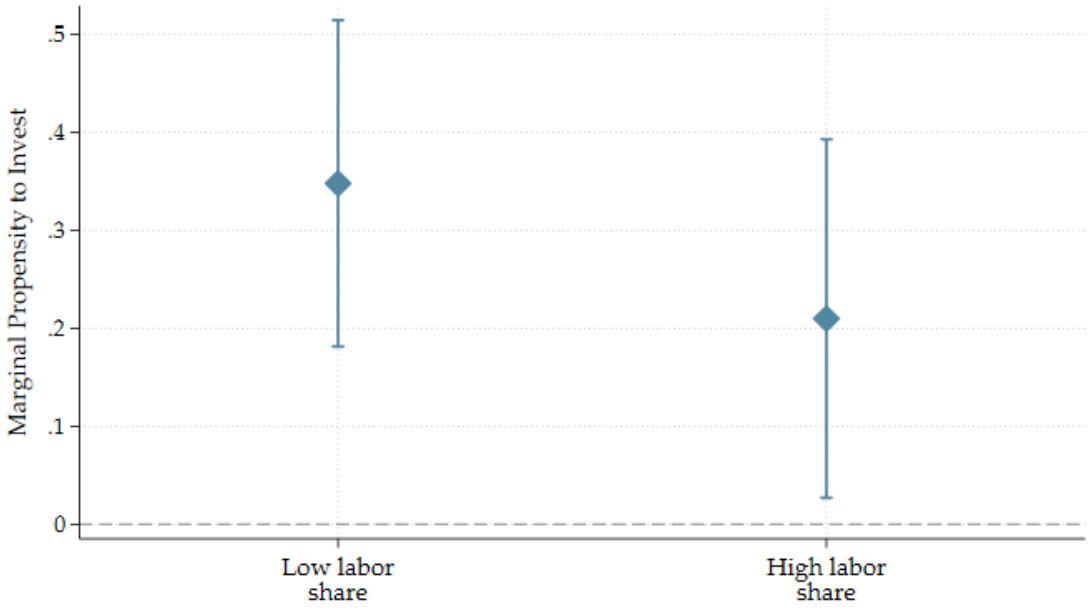
investment response to liquidity shocks, and that the aggregate MPI is disproportionately driven by constrained firms.

Sectoral Heterogeneity. We next investigate whether MPIs vary with the production technology of the sector in which a firm operates. To do so, we split sectors into two groups based on their average labor share in total costs and re-estimate (4) for each subsample. Figure 6 plots the results. Firms in low-labor-share sectors—where production is more capital-intensive—exhibit a higher MPI of approximately €.35, compared with roughly €.21 for firms in high-labor-share sectors. These results suggests that liquidity windfalls translate more readily into investment in sectors where capital is a larger factor of production, consistent with a greater scope for adjusting the capital stock in response to a cash-flow shock.

4 Model

Work in progress

Figure 6: MPIs and Sectoral Characteristics



Note: MPIs estimates by sectoral labor intensity. We split 2-digit NACE sectors in our sample in 2 bins based on the sectoral labor share in total cost. We then re-estimate (4) on each subsample and compute MPIs according to (5) for each of them. Bands show 90% confidence intervals. Standard errors are clustered at the firm level.

5 Conclusion

We provide the first causal estimates of the marginal propensity to invest of firms by exploiting a natural experiment that generated exogenous liquidity shocks to Italian firms without affecting their demand. Our estimates imply that firms channel roughly one quarter of each euro of additional cash flow into investment. The response is concentrated among highly leveraged firms and in capital-intensive sectors, consistent with financial frictions playing a first-order role in the transmission of liquidity shocks to investment. These results carry direct implications for the design of stabilization policy. Programs that accelerate payment of existing government obligations can stimulate private investment at relatively low fiscal cost, and their effectiveness depends critically on the financial health of the firms they reach. More broadly, our estimates suggest that the distribution of firm balance-sheet positions is a key state variable for predicting the real effects of policies that operate through the cash-flow channel.

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Online Appendix

Marco Bellifemine **Adrien Couturier**

A Appendix to Section 2

A.1 Response of Public Spending

A potential concern with our identification strategy is that the reform may have relaxed public administrations' budget constraints, leading to an increase in procurement spending that would confound the pure cash-flow channel with a demand effect. We test for this directly by estimating the analogue of (3) at the administrative body level:

$$\log Y_{jt} = \alpha_j + \delta_t + \sum_{h=2010, h\neq 2012}^{2017} \beta_h \left(\mathbb{1}_{h=t} \times g_j\right) + \varepsilon_{jt}, \tag{A.1}$$

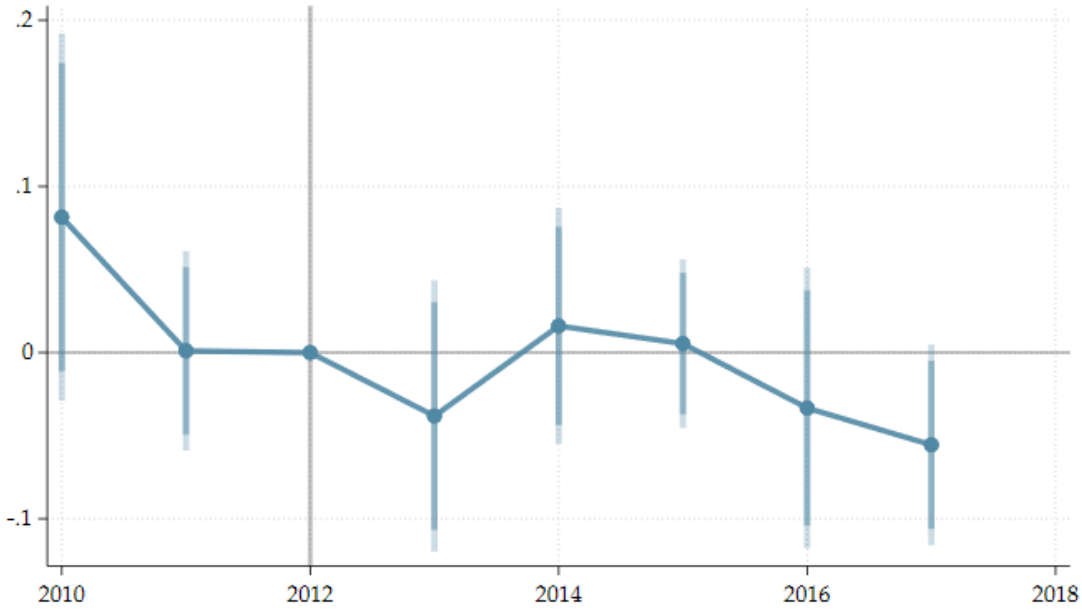
where Y_{jt} is procurement expenses from administrative body j in year t, g_j is the shift defined in Section 2 and all other variables are defined as in (3).

Figure A.1 plots the estimated $\widehat{\beta}_h$. The coefficients are small, statistically insignificant, and show no discernible pattern before or after the reform. This null result is consistent with the institutional design of the program: the funds were earmarked to settle pre-existing commercial debts and did not expand administrations' spending capacity going forward. The absence of a spending response reinforces the interpretation of our instrument as capturing a pure liquidity shock to firms, rather than a shift in public demand for their goods and services.

A.2 Response of Firm Revenues

To further rule out the possibility that the reform operated through a demand channel, we estimate (3) with firm revenues as the outcome variable. If the settlement of public debts had stimulated demand for firms' output, we would expect revenues to rise differentially for more exposed firms. Figure A.2 plots the estimated $\widehat{\beta}_h$ coefficients. Two features of the figure are worth noting. First, firms with different exposure to the reform exhibit no differential trends in sales prior to the policy, further supporting the parallel trends assumption. Second, revenues do not respond differentially to the reform either: the post-reform coefficients remain small and statistically insignificant. Together with the absence of a public

Figure A.1: Response of Procurement Spending by Administrative Bodies



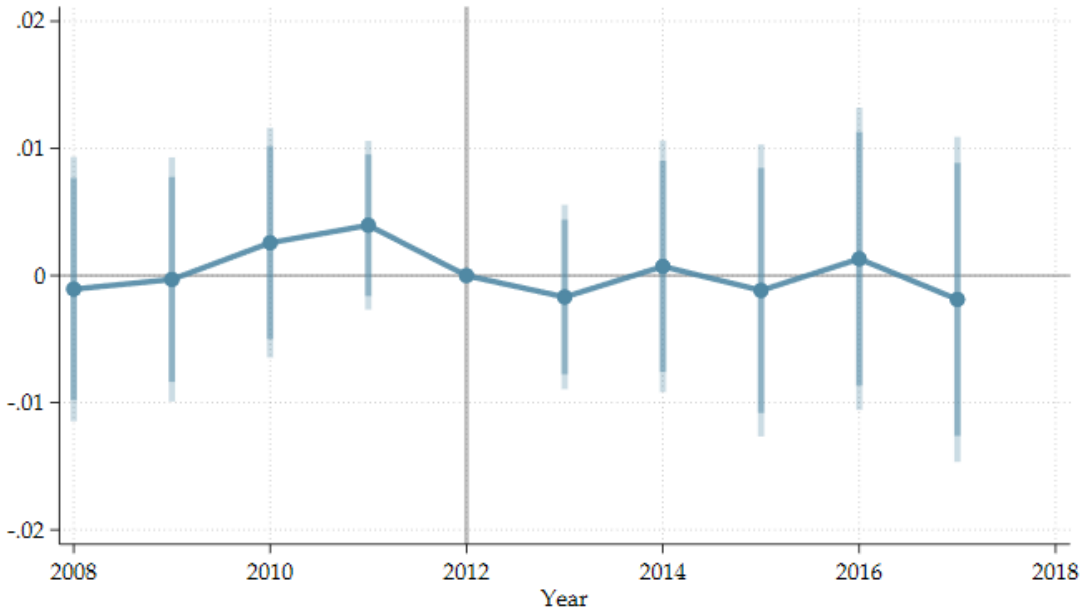
Note: estimated $\widehat{\beta}_h$ from the event study design in (A.1). The y-axis denotes the change in public procurement spending (relative to the base year 2012) in percentage points. Wide and thin lines represent 90% and 95% confidence bands, respectively. We cluster standard errors at the administrative body level.

spending response documented in Figure A.1, this evidence reinforces the interpretation that the reform operated as a pure cash-flow shock to firms without generating additional demand.

A.3 Instrumenting Credits to Customers

In this Section we re estimate our instrumental variable specification (4) using credits to customers, rather than liquidity, as the endogenous instrumented variable. Table A.1 presents the results. The estimates for MPIs are in the same ballpark as our baseline results, although slightly smaller in magnitude, ranging between .15 to .26.

Figure A.2: Response of Firm Revenues



Note: estimated $\widehat{\beta}_h$ from the event study design in (3). The y-axis denotes the change in revenues (relative to the base year 2012) in percentage points. Wide and thin lines represent 90% and 95% confidence bands, respectively. We cluster standard errors at the firm level.

Table A.1: Marginal Propensity to Invest – Credits to Customers

	Narrow Window			Broad Window		
	(1)	(2)	(3)	(4)	(5)	(6)
log Credits$_{it}$	-2.334***	-2.233***	-1.656**	-2.134***	-1.867***	-1.332**
	(0.698)	(0.721)	(0.652)	(0.611)	(0.586)	(0.549)
Kleibergen-Paap F-stat.	26.16	23.45	23.02	29.21	28.16	24.98
Observations	97,503	97,466	97,459	146,941	146,885	146,875
Year FEs	✓	✓	✓	✓	✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓
Province × Year FEs		✓	✓		✓	✓
Sector × Year FEs			✓			✓
MPI	0.256	0.245	0.182	0.244	0.213	0.152

Note: instrumental variable estimates from (4). The first column "Narrow window" reports estimates based on the 2011-2014 sample. The second column "Broad window" reports estimates based on the wider 2010-2015 sample. The row MPI reports the implies MPI computed according to (5). Standard errors in parenthesis clustered by firm.

Table A.2: Instrumental Variable – First Stage

	Narrow Window			Broad Window		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Post}_t \times \text{SS}_i$	0.047***	0.046***	0.038***	0.050***	0.048***	0.039***
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)
Observations	113,843	113,799	113,791	170,568	170,490	170,479
R^2	0.807	0.808	0.809	0.759	0.761	0.762
Year FEs	✓	✓	✓	✓	✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓
Province × Year FEs		✓	✓		✓	✓
Sector × Year FEs			✓			✓

Note: estimates for the first-stage regression of (4). The dependent variable is log liquidity. The first column "Narrow window" reports estimates based on the 2011-2014 sample. The second column "Broad window" reports estimates based on the wider 2010-2015 sample. The row MPI reports the implies MPI computed according to (5). Standard errors in parenthesis clustered by firm.

Table A.3: Marginal Propensity to Invest – OLS estimates

	Narrow Window			Broad Window		
	(1)	(2)	(3)	(4)	(5)	(6)
$\log \text{Liquidity}_{it}$	0.033***	0.032***	0.030***	0.052***	0.051***	0.047***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Observations	96,021	95,985	95,978	145,399	145,347	145,337
R^2	0.771	0.773	0.774	0.733	0.735	0.737
Year FEs	✓	✓	✓	✓	✓	✓
Firm FEs	✓	✓	✓	✓	✓	✓
Province × Year FEs		✓	✓		✓	✓
Sector × Year FEs			✓			✓
MPI	0.012	0.012	0.011	0.020	0.019	0.018

Note: instrumental variable estimates from (4). The first column "Narrow window" reports estimates based on the 2011-2014 sample. The second column "Broad window" reports estimates based on the wider 2010-2015 sample. The row MPI reports the implies MPI computed according to (5). Standard errors in parenthesis clustered by firm.