Liquidity as Competitive Advantage: The Role of Intangibles

Carlo Altomonte* Monica Morlacco† Tommaso Sonno‡ Domenico Favoino§

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Abstract

We show that short-term liquidity can be a source of competitive advantage by enabling firms to invest in intangible assets. Our analysis leverages a French reform that capped payment delays in trade credit contracts, which generated quasi-experimental variation in corporate liquidity across manufacturing firms. Higher liquidity led to significantly greater investment in intangibles, which, in turn, raised markups and market shares. These results suggest a strategic role for liquidity in shaping firm performance, indicating that initial financial conditions can have lasting effects on productivity and market structure.

*Department of Social and Political Sciences, Bocconi University; carlo.altomonte@unibocconi.it
†Department of Economics, University of Southern California; morlacco@usc.edu
‡Department of Economics, University of Bologna, CEP-LSE; tommaso.sonno@unibo.it
§Erasmus School of Economics; domenico.favoino@gmail.it

1 Introduction

Intangible capital has become a major driver of economic activity in advanced economies. Assets such as intellectual property, brand equity, and organizational know-how increasingly determine a firm's value, supplanting traditional inputs like physical capital and labor. Firms that invest in and effectively manage intangible assets are better positioned to scale, differentiate, and adapt to shifting demand, contributing to long-run profitability and resilience (Haskel and Westlake, 2018).

A large literature shows that intangible investment is strongly associated with product market performance, both domestically and internationally.[1] Much less is known, however, about the forces that drive such investment. While it is well established that investment responds to financial conditions, we show that short-term liquidity is particularly crucial for intangibles. To establish this link, we exploit a French policy reform that generated quasi-experimental variation in trade-credit rules and use it to identify how liquidity shocks shape intangible investment and, ultimately, competitive advantage. Our findings indicate that short-term financing can have persistent effects on firm performance by influencing the allocation of intangible capital and, in turn, altering product market dynamics.

Our analysis is guided by studies showing a link between intangibles and firm performance through several channels. Intangibles can lower marginal costs and raise fixed costs, creating strategic advantages that deter entry (De Ridder, 2024). They can shift demand through advertising and customer capital (Morlacco and Zeke, 2021). They can exhibit increasing returns when embedded in information-based technologies that scale with size (Lashkari et al., 2024). While distinct in mechanism, these frameworks share the prediction that greater intangible intensity of firms is associated with stronger competitive positions.

These models typically abstract from financial constraints. In practice, intangible assets are difficult to finance externally because they have limited collateral value and are difficult to price, which makes them poorly suited to debt financing (Hall and Lerner, 2010). Therefore, short-term liquidity from internal sources becomes essential to maintain investment (Fazzari

[1] We review the relevant literature in detail below.

et al., 1988). This yields three testable implications: (i) under binding liquidity constraints, an exogenous shock to short-term liquidity should raise intangible investment; (ii) higher intangible investment should strengthen competitive position; and (iii) because tangible assets are more easily collateralized and more sensitive to interest rate conditions (David and Gourio, 2023), tangible investment should respond less to the same shock.

We test these predictions using a 2009 French reform that capped domestic trade-credit terms at 60 days. The policy tightened internal liquidity for downstream buyers and relaxed it for upstream suppliers that had previously extended payment periods longer than the 60-day cap (Beaumont and Lenoir, 2023). We combine this setting with administrative data on French manufacturing firms from 2004 to 2014.

We begin by testing for liquidity constraints in intangible investment. Our main outcome variable is the book value of intangibles, with changes over time serving as a proxy for investment. We implement a difference-in-differences design that exploits firms' initial distance from the 60-day cap (first difference) and compares investment behavior before and after the reform (second difference). Because the sign of the liquidity shock depends on whether a firm is a net supplier or a recipient of trade credit, treatment is defined by net trade-credit exposure. All specifications include firm fixed effects and financial controls to address concerns that initial distance from the cap may be correlated with underlying investment opportunities. Supporting the validity of this approach, event-study estimates show no differential pre-trends between treated and control firms.

Improved short-term liquidity has a sizable impact on intangible investment. Moving a firm from the 25th to the 75th percentile of net exposure raises the stock of intangibles by 3–6 percentage points, depending on the specification. The effects are robust across alternative treatment definitions, measures of intangibles, regression weights, and control sets. These findings suggest that liquidity constraints are binding in financing intangibles, underscoring the importance of short-term credit in facilitating investment in these strategic assets.

We next turn to product market outcomes, instrumenting intangible investment with the reform-induced liquidity shock. Consistent with theory, higher intangible investment translates into stronger competitive positions. Our baseline estimates suggest that a 10 percent increase

in intangible investment raises markups, our primary measure of competitive advantage, by about 2 percent. We corroborate this result through a range of robustness checks. We also show that the effect holds when using market share and total exports as proxies for competitive advantage. These variables provide complementary accounting-based measures of firms' market performance and are less subject to measurement concerns.

A natural concern is measurement error in our proxies for intangibles and markups. For intangibles, we rely on the book value of intangible fixed assets in Orbis, which primarily captures assets acquired in business combinations. Changes in this stock may therefore reflect restructuring rather than new investment. To address this limitation, we construct a flow-based proxy from operating expenditures not tied to production and report parallel estimates. Both measures align well with Compustat benchmarks at the industry level, and the investment response to the liquidity shock is strongest in sectors with high intangible intensity, which is difficult to reconcile with an acquisition-only interpretation.

Markups are measured using the cost-based approach of De Loecker and Warzynski (2012), which infers markups from the ratio of output elasticities to input expenditure shares. Two issues are worth noting in our context. First, the treatment of intangibles in production-function estimation can significantly bias elasticities (Traina, 2018). Second, reliance on revenue data may introduce price-related biases (De Loecker and Goldberg, 2014). We address these concerns by estimating alternative production functions that incorporate intangibles either as inputs or as productivity shifters, by using input cost shares as proxies for elasticities that bypass production-function estimation, and by considering additional balance-sheet-based indicators of competitive strength. The core findings remain stable across all approaches.

Finally, we examine the exclusion restriction. Our design requires that the liquidity shock influence competitive outcomes only through intangible investment. A potential concern is that the shock might also affect tangible capital or borrowing capacity. Consistent with our third testable prediction, we find no effect on physical investment and no post-reform divergence between treated and control firms in indicators of access to external finance. These results reinforce the credibility of our identification strategy and confirm that the relevant channel operates through intangibles.

Overall, the evidence demonstrates that short-term financing, such as trade credit, is a strategic input into the accumulation of intangible capital. By determining which firms are able to accumulate these strategic assets, liquidity conditions shape competitive dynamics and influence the long-run distribution of productivity and market shares, with implications for aggregate efficiency.

Related Literature This paper contributes to a growing literature on the relationship between financial conditions and firm competitiveness. Most directly, it advances our understanding of how short-term liquidity shapes competitive outcomes, with a particular focus on intangible investment as a key transmission channel.

Prior research highlights the strategic role of liquidity in product markets.[2] Cash-rich firms tend to gain market share, particularly in competitive industries (Fresard, 2010), while heavy debt burdens can impair product market performance (Campello, 2003, 2006). Liquidity constraints also shape firms' pricing behavior during downturns (Chevalier and Scharfstein, 1995; Gilchrist et al., 2017).

While much of the theory emphasizes price-based mechanisms, such as predatory pricing or threats of future price cuts (Chevalier and Scharfstein, 1995; Benoit, 1984), recent work underscores non-price responses. Granja and Moreira (2023) shows that financial frictions hinder product innovation, and Duval et al. (2020) finds that financially constrained firms cut intangible investment during the global financial crisis. We contribute to this literature by showing that intangible investment is a key margin through which short-term liquidity, such as trade credit, affects firm behavior.

We also contribute to the broader literature on how financing frictions shape investment. We show that while intangible investment responds strongly to shocks to liquidity, tangible investment remains largely unaffected. This pattern is consistent with differences in how the two asset types are financed. Falato et al. (2023) shows that financing frictions and adjustment costs lead firms to accumulate precautionary cash, a mechanism that may help explain the higher marginal propensity to invest in intangibles we observe. Our findings also complement

[2]See Brander and Lewis (1986); Phillips (1995); Chevalier (1995) for early contributions.

David and Gourio (2023), who shows that intangible investment is less sensitive to monetary policy shocks, reinforcing the idea that it relies on distinct financing channels.

Our findings also contribute to the literature on finance and exports. Prior work shows that credit access improves firms' ability to compete in foreign markets (Amiti and Weinstein, 2011; Manova, 2008, 2013; Paravisini et al., 2015; Chaney, 2016). We extend this literature by identifying a new mechanism in the finance-export link: intangible-driven competitive advantage. Our results also highlight a reverse channel in the trade–innovation relationship. While previous studies emphasize how trade stimulates innovation and investment (Bloom et al., 2016; Mayer et al., 2014, 2021), we show that innovation, particularly through intangible investment, can enhance firms' competitiveness in international markets.

Finally, we speak to a growing literature on the role of trade credit in firm behavior. Trade credit is a key source of short-term liquidity, especially for small and medium-sized firms (Petersen and Rajan, 1997; Wilner, 2000; Smith, 1987), and can serve as a competitive tool, particularly in export markets (Demir and Javorcik, 2018). Beaumont and Lenoir (2023) studies the same French reform we exploit and shows that tightening trade credit limits firms' ability to reconnect with customers post-downturn. We complement their analysis by demonstrating that easing trade credit constraints also enables firms to invest in intangibles that enhance their long-term competitive position.

2 Conceptual Framework

This section outlines the conceptual framework that motivates our empirical analysis, linking liquidity, intangible investment, and competitive advantage. Rather than presenting a formal model, we review existing literature to describe the economic mechanisms underlying our main hypotheses.

We proceed in three parts. Section 2.1 reviews theories that link intangibles to firm performance and competitive advantage. Section 2.2 examines how liquidity constraints shape firms' ability to invest in intangibles and why such investment is likely to be the key margin through which short-term liquidity shocks affect firm outcomes. Section 2.3 discusses some challenges

in the empirical implementation of the key theoretical predictions.

2.1 Intangibles and Firm Performance

A growing literature argues that investment in intangible capital enhances firm competitiveness, either by increasing markups or expanding market shares. We summarize three recent contributions that formalize this relationship through distinct channels.

De Ridder (2024) models intangible capital as a productivity-enhancing input that lowers marginal costs. Firms face a trade-off between a fixed investment and a proportional reduction in variable costs. In a Bertrand pricing setting, firms with greater adoption of intangibles can charge higher markups and gain market shares. Over time, this cost advantage deters entry and weakens rivals' innovation incentives, reinforcing incumbents' dominance.[3]

Morlacco and Zeke (2021) emphasizes demand-side intangibles such as brand equity and customer capital. In this setting, firms compete for customer loyalty through advertising, which shifts demand away from competitors. The resulting externality is stronger for larger firms, which have more to gain from customer retention and are more likely to invest in advertising. This dynamic leads to asymmetric investment, allowing dominant firms to consolidate their market share over time.

Lashkari et al. (2024) focuses on heterogeneous IT adoption as a source of increasing returns to scale. Larger firms exhibit higher elasticity of demand for IT inputs and are better able to spread fixed costs over greater output. As IT prices fall, these firms benefit disproportionately, gaining a cost advantage that reinforces their market position and fosters industry concentration.

Despite differences in emphasis, these theories share a common implication: intangible capital strengthens competitive advantage, either by reducing costs or shifting demand. In each case, early investment delivers persistent gains, either through cost leadership, customer lock-in, or scalability, creating first-mover advantages that encourage early adoption. This motivates our first empirical prediction:

Testable Prediction 1. *All else equal, firms that invest more in intangibles gain a compet-*

[3]See also Hsieh and Rossi-Hansberg (2023) for a related framework in service sectors.

itive advantage through larger market shares or markups.

2.2 How Liquidity Enables Intangible Investment

Liquidity constraints disproportionately affect investment in intangibles. The mechanism operates through three features that interact with liquidity and distinguish intangibles from tangibles: financing frictions, adjustment costs, and strategic value.

First, as discussed in the literature review, intangible assets have limited pledgeability and are difficult to value, trade, or redeploy. Firms therefore rely more on internal cash flow or short-term credit to fund intangible investment, making these expenditures particularly sensitive to fluctuations in liquidity.

Second, although both types of investments often entail large upfront expenditures, many intangibles (for example, software development or advertising) can be scaled incrementally. Tangible investments, such as machinery and structures, are typically lumpier less responsive to small changes in financing conditions (Abel and Eberly, 1999; Caballero and Engel, 1999). Modest liquidity shocks can thus unlock intangible spending without triggering immediate changes in physical capital.

Third, intangible investment is naturally suited to shift demand and build customer capital. These features raise the payoff to allocating marginal liquidity to intangibles, especially when financing constraints might otherwise delay or prevent such spending.

Taken together, these features yield two testable predictions:

Testable Prediction 2. *If a firm faces a binding liquidity constraint, an exogenous increase in liquidity increases intangible investment.*

Testable Prediction 3. *If a firm faces a binding liquidity constraint, the response of intangible investment to a liquidity increase exceeds the response of tangible investment.*

2.3 From Theory to Data

Before turning to the empirical analysis, we highlight two main challenges in mapping the theory to the data.

The first challenge is measurement. Intangibles and markups are not directly observable, and their empirical proxies are subject to debate. Given data constraints, we rely on firms' balance sheets to construct proxies. For intangibles, we use a stock measure based on the book value of intangible fixed assets in Orbis and a flow measure based on operating expenditures not tied to production. These proxies are necessarily coarse and do not separately capture categories such as software, R&D, brand capital, or customer capital. Consequently, our analysis speaks to broad predictions rather than specific mechanisms. For competitive advantage, our primary outcome is the cost-based markup following De Loecker and Warzynski (2012), complemented by market share and total exports constructed from accounting data. Section 3.5 provides further details on these variables and discusses the related measurement challenges.

The second challenge is endogeneity. Liquidity, intangible investment, and product market outcomes may be jointly determined, making causal inference difficult. We address this by exploiting a French reform of trade credit terms that generated quasi-experimental variation in firms' access to short-term liquidity. The reform reallocated liquidity across firms in a plausibly exogenous manner, depending on their initial payment terms and whether they were net suppliers or recipients of trade credit. This setting supports a difference-in-differences design that identifies the effect of liquidity on investment under standard parallel-trends assumptions, and it also provides an instrument for analyzing intangible investment in product market outcomes.

3 Empirical Strategy

Having outlined the conceptual challenges, we now turn to the empirical strategy we use to test the predictions developed in Section 2. Section 3.1 introduces the data. Section 3.2 describes the institutional context of the reform and the construction of the firm-level liquidity shock. Section 3.3 and Section 3.4 introduce our main estimating equations. Finally, Section 3.5 provides further detail on the measurement of the key variables.

3.1 Data

Our empirical analysis draws on a panel of French manufacturing firms from 2004 to 2014, based on Orbis data compiled by Bureau van Dijk.[4] This dataset provides detailed firm-level information on financial statements and production activities, allowing us to construct key variables such as intangible investment, markups, and liquidity.[5]

We restrict the sample to firms reporting manufacturing as their primary activity and drop observations with missing data on key variables required to construct markups and intangible investment, including sales, material costs, labor compensation, and capital expenditure. To ensure consistent coverage before and after the policy reform and mitigate concerns about changing sample composition, we limit the baseline sample to firms active in 2004 and continuously observed through at least 2012. The resulting panel is representative of the size distribution of French manufacturing firms at the two-digit industry level.[6]

3.2 Institutional Context and Liquidity Shock

In August 2008, the French government enacted a law capping trade credit payment terms at sixty days, effective January 1, 2009. The reform applied to all firms operating under the French commercial code and was part of a broader effort to modernize the economy. Enforcement was swift and uniform across the country, managed by the Ministry of the Economy's regional Directorates (Beaumont and Lenoir, 2023).

To capture firm-level exposure to the reform, we follow the literature and construct measures of payment timing. The average time to receive payments, or *Days Sales Outstanding (DSO)*, is calculated as:

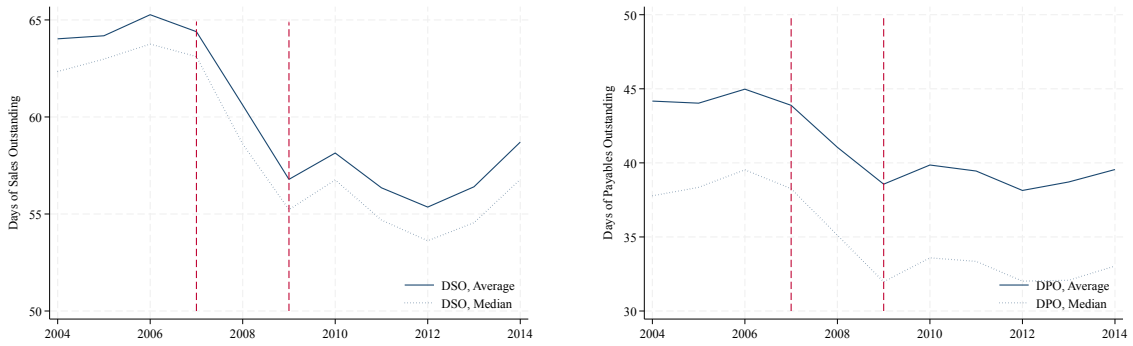
$$\text{DSO}_{it} = \frac{\text{Accounts Receivable}_{it}}{\text{Sales}_{it}} \times 365. \tag{1}$$

[4]We use a dataset assembled as part of the EU-financed Horizon 2020 project MICROPROD. The project and deliverables are described here: https://cordis.europa.eu/project/id/822390.

[5]Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez (2017) use similar Orbis data for Spain to examine the role of firm-level financial frictions in shaping aggregate productivity.

[6]We apply sampling weights based on firm size, industry, and year, using data from Eurostat's Structural Business Statistics. All results are reported using both weighted and unweighted specifications.

Figure 1: Evolution of DSO and DPO, 2004–2014



(a) DSO (b) DPO

Notes: The panels plot the evolution of average and median Days Sales Outstanding (DSO) and Days Payable Outstanding (DPO) between 2004 and 2014. The policy reform was voted in 2008 and implemented in 2009. Red markers indicate the pre-reform year (2007) and the post-reform year (2009).

Likewise, *Days Payable Outstanding (DPO)*, which measures the average time to make payments, is defined as:

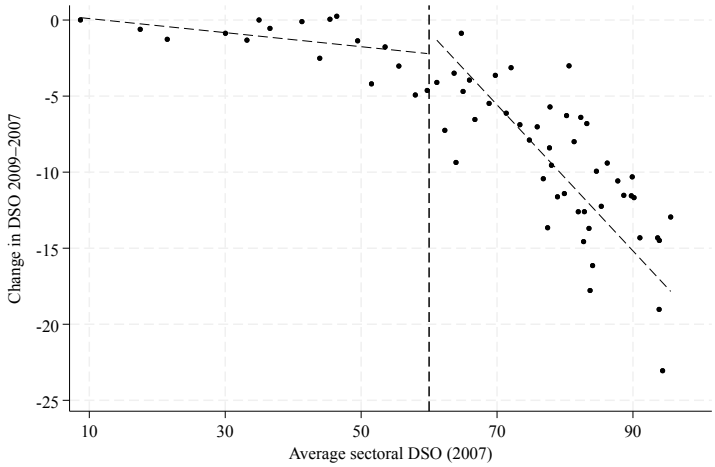
$$\text{DPO}_{it} = \frac{\text{Accounts Payable}_{it}}{\text{Sales}_{it}} \times 365. \tag{2}$$

Before the reform, the average DSO was approximately 65 days (standard deviation: 43), while the DPO was around 45 days (standard deviation: 30). These statistics suggest that many firms extended trade credit beyond the 60-day limit and would therefore be directly affected by the new regulation.

Figure 1 illustrates the sharp decline in DSO following the reform, with average payment terms falling from about 65 days in 2007 to 57 days in 2009. DPO also declined, though more modestly, consistent with a negative liquidity shock for payers. These patterns suggest that the policy generated significant liquidity shocks across firms.

Figure 2 shows the effect of the reform on DSO across firms. For readability, firms are first grouped into percentiles of their sector's 2007 DSO. The x-axis reports the average initial DSO for each percentile bin, and the y-axis shows the mean change in firm-level DSO between 2007 and 2009. A sharp kink appears at the 60-day threshold: industries with longer initial payment periods experienced significantly larger declines in DSO, consistent with a stronger positive

Figure 2: Impact of the Policy on Payment Days (2007–2009)



Notes: The figure plots the change in average firm-level DSO between 2007 and 2009 as a function of initial 2007 sectoral DSO. Firms are first grouped into percentiles of sectoral DSO in 2007, and each point plots the mean change within a percentile bin against its average initial DSO. The dashed lines report linear fits estimated separately for sectors below and above the 60-day policy cap.

liquidity shock. Appendix Figure A.3 presents analogous results for Days Payable Outstanding (DPO), showing that industries with initial DPO near or above the 60-day threshold were more strongly affected.[7]

As a placebo test, Appendix Figures A.4 and A.5 plot changes in DSO and DPO between 2004 and 2007. In this pre-reform period, there is no systematic relationship between initial payment terms and subsequent changes: the points are essentially flat and show no discernible patterns. This evidence supports the interpretation that the post-2007 variation is policy-induced rather than driven by pre-existing trends.

We define a firm's liquidity shock using pre-policy information on payment terms:

$$\text{Liquidity Shock}_i^{\text{pre}} = \max\left\{\max\left\{\text{DSO}_i^{\text{pre}} - 60,\ 0\right\} - \max\left\{\text{DPO}_i^{\text{pre}} - 60,\ 0\right\},\ 0\right\}. \qquad (3)$$

Here, $\text{DSO}_i^{\text{pre}}$ and $\text{DPO}_i^{\text{pre}}$ refer to the days sales outstanding and payable outstanding, respec-

[7]For DPO there is less variation prior to 2007, which makes the evidence less clear-cut, but the results remain consistent with the expected directional effect.

tively, in 2007, the year preceding the reform.[8]

We consider two baseline treatment definitions. The first is a binary treatment indicator:

$$T_{1,i} = \mathbb{I}\left\{\text{Liquidity Shock}_i^{\text{pre}} > 0\right\}, \tag{4}$$

which equals one if firm i experienced a net improvement in liquidity due to the reform and zero otherwise. Under this definition, approximately 50 percent of firms are treated.

The second definition captures the intensity of the shock:

$$T_{2,i} = \ln(1 + \text{Liquidity Shock}_i^{\text{pre}}), \tag{5}$$

where we take the logarithm of the liquidity shock to reduce skewness and facilitate the interpretation of coefficients as semi-elasticities.

3.3 Difference-in-Differences Strategy

To estimate the causal effect of liquidity on intangible investment, we employ a difference-in-differences (DiD) identification strategy. Specifically, we compare changes in intangible assets before and after the 2009 policy reform across firms with different levels of exposure to the resulting liquidity shock. The key identifying assumption is that, absent the reform, treated and control firms would have exhibited parallel trends in intangible accumulation.

We estimate the following specification:

$$\ln Y_{it} = \alpha + \beta \cdot \text{Post}_t \times T_{j,i} + \mathbf{X}_{it}'\gamma + c_i + \delta_t + \varepsilon_{it}, \quad j \in \{1, 2\}, \tag{6}$$

where the dependent variable Y_{it} denotes intangible capital.

The coefficient of interest, β, captures the differential change in intangible capital for treated firms relative to control firms following the reform. The treatment indicator $T_{j,i}$ is defined according to one of two specifications described in equations (4) and (5), and Post_t is an indicator for the post-reform period, beginning in 2009. The vector $\mathbf{X}_{it}$ includes firm-level liquidity

[8] We discuss robustness tests to this baseline measure of the liquidity shock in Section 4.2.1.

controls. In our main specifications, these consist of either baseline (2004) values or time-varying values of current liabilities and cash holdings, each of which is interacted with the post-reform dummy. All regressions include firm fixed effects c_i and year fixed effects δ_t. Standard errors are clustered at the firm level.

A key concern in identifying treatment assignment is that it may be correlated with firms' future outcomes, potentially violating the parallel trends assumption. While the inclusion of firm fixed effects and controls for pre-reform liquidity positions helps account for time-invariant heterogeneity and baseline financial conditions, they do not rule out dynamic selection. To address this, we conduct formal tests for differential pretrends in intangible accumulation between treated and control firms. As discussed below, the absence of pre-existing trends provides support for the validity of our DiD design (see details in Online Appendix C).

3.4 Instrumental Variable Strategy

We then test the second hypothesis, which posits that firms investing more heavily in intangible assets gain a competitive advantage, as reflected in higher markups or market shares. A central concern in estimating this relationship is endogeneity: unobserved demand or productivity shocks may simultaneously drive both intangible investment and competitive advantage, leading to biased estimates. To address this issue, we implement an IV strategy that uses the trade credit reform as a source of quasi-experimental variation in intangible investment. This approach allows us to isolate the causal effect of intangibles on pricing power, leveraging the exogenous shift in liquidity induced by the policy shock.

We consider the following specification:

$$\ln \mu_{it} = \alpha + \beta \ln Y_{it} + c_i + \delta_t + \epsilon_{it} \tag{7}$$

where $\ln \mu_{it}$ denotes the log markup or market share of firm i at time t, and the remaining terms are as above, with Y_{it} denoting intangible investment. We run 2SLS regressions on (7), instrumenting intangibles with the liquidity shock, specifically $\text{Post}_t \times T_{j,i}$ with $j = \{1, 2\}$, including the same set of controls for liquidity as in the main estimating equation (6).

3.5 Outcome Variables and Measurement Challenges

Before presenting the results, we describe how we measure our key outcome variables, specifically intangible investment and markups, and discuss the main measurement challenges along with the strategies we use to address them and assess robustness.

3.5.1 Intangible Capital

Our empirical analysis relies on two proxies for intangible activity: a stock measure drawn from firms' balance sheets and a flow measure constructed from operating expenditures. Following standard practice, we interpret changes in the stock-based measure as a proxy for intangible investment. This convention allows us to report and interpret results using both measures in parallel throughout the analysis.

The stock-based proxy is the book value of intangible assets reported in Orbis.[9] This measure provides a transparent indicator available across firms and years. However, it comes with several limitations. Most notably, it primarily captures acquired intangible assets, including goodwill from business combinations (Bajgar et al., 2020), and may understate internally generated investment. In addition, the variable is either missing or recorded as zero for approximately one quarter of firms, potentially reflecting misreporting or the absence of capitalized intangibles. Finally, variation in this measure may partly reflect corporate restructuring activity rather than true investment activity.

To address these concerns, we construct a second (flow-based) proxy that captures intangible-related spending using firms' operating expenses not directly tied to production. Specifically, we use the following accounting identity:

$$\underbrace{\text{Intangible expenses} + \text{Other operating expenses}}_{\text{Operating Expenses}} = \text{Revenues} - \text{Variable Costs} - \text{Operating Profits},$$

In this identity, other operating expenses include items such as rent, insurance, and inventory costs. Since all components on the right-hand side are observed, we use the residual to

[9] The corresponding variable is `IFASS` (Intangible Fixed Assets), defined as "all intangible assets such as formation expenses, research expenses, goodwill, development expenses, and all other expenses with a long-term effect."

approximate intangible-related operating expenditures. Although this measure aggregates different types of spending, many of these components are known to move together with intangible activity.[10]

We acknowledge that this proxy also has important limitations. In particular, it may capture acquisition-related costs if these are expensed rather than capitalized.[11]

Although both proxies provide broad coverage and are consistently defined across firms and years, neither offers a direct measure of intangible investment. Previous studies often use more targeted indicators, such as R&D expenditures or specific components of SG&A, which more accurately capture intangible activity (Eisfeldt and Papanikolaou, 2013; Peters and Taylor, 2017). Unfortunately, due to limited reporting in our dataset, these variables are unavailable for the majority of firms in our sample.

To assess the credibility of our proxies and address the concern that they might instead reflect acquisition activity, we consider an indirect validation strategy. First, we test whether our Orbis-based proxies are correlated with benchmark measures of intangible intensity, constructed from Compustat at the industry level. Second, we classify industries according to their average intangible intensity using these Compustat benchmarks and test whether our estimated effects are stronger in more intangible-intensive sectors.[12] If our proxies meaningfully capture intangible accumulation, we expect both patterns to hold. Results from both validation exercises are presented below.

3.5.2 Markups and Competitive Advantage

Markups are not directly observable from firm balance sheets due to the absence of marginal cost and price data. We estimate them using the cost-based approach of De Loecker and Warzynski (2012), which infers markups as the ratio of the output elasticity of a variable input to its

[10] Consistent with this interpretation, we find a positive but modest correlation with the book value of intangible assets. An OLS regression controlling for firm and industry-year fixed effects yields a coefficient of 0.12, significant at the 1 percent level.

[11] Following the 2008 revision to IFRS 3, the cost of acquiring a business is capitalized, but transaction-related expenditures, such as legal, advisory, and due diligence fees, are expensed. These costs are typically small relative to total operating expenditures.

[12] We measure intangible intensity at the industry level using the methodology in Morlacco and Zeke (2021).

revenue share:

$$\mu_{it} = \frac{\theta_{it}^V}{\alpha_{it}^V}, \quad \text{where} \quad \alpha_{it}^V \equiv \frac{E_{it}^V}{R_{it}},$$

with E_{it}^V denoting expenditure on input V and R_{it} firm revenues. We treat materials as the flexible input. Revenue shares are directly observed, and output elasticities are estimated from firm-level production functions.

While the cost-based approach to markup estimation is widely used, it raises two key concerns related to the estimation of output elasticities. The first concerns the treatment of intangible capital in production function estimation. Because intangibles affect both productivity and cost structure, omitting them may bias elasticity estimates and lead to spurious correlations between markups and intangible investment (Traina, 2018). This issue is particularly relevant in our context, where we aim to estimate the causal effect of intangible investment on firm performance.

The second concern stems from the use of revenue data. Since prices and quantities are not separately observed in balance-sheet data, relying on revenues as a proxy for output can distort elasticity estimates (De Loecker and Goldberg, 2014; Bond et al., 2021). While recent evidence suggests that such biases primarily affect the level of markups rather than their dispersion or correlation with firm outcomes (De Ridder et al., 2021), it remains important to test the robustness of our results to these potential distortions.

To address these concerns, we estimate a range of markup measures that vary in both functional form and in the treatment of intangible inputs in production function estimation. Our baseline measure is based on a Translog production function in which intangible capital enters as a TFP-enhancing input. For robustness, we also estimate a Cobb-Douglas version with the same treatment of intangibles, as well as alternative Translog specifications in which intangibles either enter as a separate input or are excluded altogether.[13]

To further address concerns about output price bias, we construct two additional markup measures using cost-share approximations of output elasticities. These alternatives assume

[13]In additional results omitted for brevity, we estimate Cobb-Douglas versions of the latter two specifications and find nearly identical markups, suggesting that under Cobb-Douglas assumptions, the treatment of intangibles has a limited impact.

Table 1: Summary Statistics

	Nr. Obs.:	Mean	St. Dev.	P25	Median	P75
Intangibles						
Intangible Assets	388,492	49,826	95,600	0	8,000	53,000
Intangible (Operating) Expenses	359,025	826,280	1,529,256	96,000	247,000	765,000
Competitive Advantage						
Markup (Baseline)	114,179	1.52	0.28	1.29	1.48	1.72
Markup (Cost Shares)	349,682	1.70	0.93	1.08	1.44	1.99
Covariates						
Current Liabilities	365,385	862,640	1,702,558	99,000	252,000	744,000
Cash Holdings	351,812	216,011	358,914	19,000	72,000	236,000
Accounts Receivable (Credit)	374,846	382,209	780,431	26,908	88,000	329,661
Accounts Payable (Debt)	386,766	509,051	1,001,445	28,407	138,915	473,000

Notes: Summary statistics on key variables of interest. Observations are trimmed at the variable level by setting values below the 3rd or above the 97th percentile to missing; statistics are computed on the remaining sample. Variable definitions are in the text.

constant returns to scale and calculate markups as the ratio of revenue to total costs, with and without intangible expenditures included in the denominator. By avoiding production function estimation, these measures are unaffected by unobserved prices, allowing us to test the sensitivity of our results to price-related biases.

Finally, to validate our interpretation that higher markups reflect improvements in product market performance rather than measurement artifacts, we also report results using two independent indicators: firm-level market share within four-digit NACE industries,[14] and total exports.

Online Appendix B provides further detail on the construction of these markup measures and their underlying assumptions. The consistency of results across all specifications strengthens confidence that our findings are not driven by biases in any single estimator.

3.5.3 Summary Statistics

Table 1 presents summary statistics for the main variables in our baseline sample. As is typical in firm-level datasets, we observe substantial dispersion across all dimensions. Liquidity conditions vary widely, as seen in the distributions of current liabilities, cash holdings, and trade credit. The latter, measured by accounts payable and receivable, constitutes a significant source of financing: for the median firm, trade credit accounts for roughly 15% of annual turnover,

[14]Industry totals are calculated from our sample and may be mismeasured due to incomplete coverage.

underscoring its role as a key short-term funding mechanism.

Indicators of intangible activity also show considerable heterogeneity. As discussed earlier, 25% of firms report zero intangible assets, reflecting a key limitation of the stock-based indicator, which likely underrepresents internally generated intangibles. The alternative, flow-based measure displays greater dispersion, consistent with broader variation in firms' intangible spending patterns.

To evaluate the credibility of these indicators, we compare them to external benchmarks of intangible intensity derived from U.S. Compustat data. Specifically, we compute two industry-level measures, averaged at the 4-digit NAICS level: (i) R&D expenditures as a share of sales, capturing knowledge-based investment, and (ii) SG&A expenses as a share of sales, reflecting broader investments in organizational capital, marketing, and brand equity. These are based on pre-reform data and treated as structural industry characteristics that are plausibly stable across countries. Table A.4 summarizes the distribution of these indices across industries. Appendix Figures A.1 and A.2 show that both our Orbis-based indicators correlate positively with the Compustat benchmarks at the industry level. These correlations support the view that our proxies capture meaningful cross-firm variation in intangible activity and validate their use in the empirical analysis.

Finally, we observe substantial dispersion in estimated markups. The baseline markup, derived from a Translog production function, is available for roughly one-third of the sample due to input data limitations and the exclusion of outliers. To address concerns about sample selection and measurement error, we also report a second markup measure based on a cost-share approximation of output elasticities. This alternative serves both as a robustness check, as discussed earlier, and as a way to extend our analysis to a broader set of firms, since it does not rely on production function estimation.

4 Results

This section presents our main empirical findings, organized around the three testable predictions introduced in Section 2. Section 4.1 tests Prediction 2, which states that improved

liquidity should increase intangible investment under binding constraints. Section 4.2 expands on this result by examining robustness and heterogeneity across firms and industries. Section 4.3 tests Prediction 1, showing that firms with higher intangible investment charge higher markups, consistent with a link between intangibles and competitive advantage. Finally, Section 4.4 rules out alternative channels and tests Prediction 3, confirming that liquidity primarily affects intangibles rather than tangibles.

4.1 Liquidity and Intangibles: Evidence

We begin by validating our DiD strategy by testing for pre-trends. Online Appendix C includes all the details. Using the stock-based measure of intangibles, Figure C.1 shows that pre-reform coefficients are small and statistically insignificant across all specifications, indicating no differential trends between treated and control firms.

Figure C.2 presents analogous results for the flow-based proxy, defined as the log of operating expenses demeaned at the four-digit industry–year level to net out sectoral shocks. This transformation preserves within-industry variation while removing macro and sectoral trends. The demeaned specification eliminates spurious pre-trends observed in the raw series, and we therefore adopt it as our preferred flow-based proxy in the rest of the analysis.

Table 2 reports results from our main DiD specification (equation (6)) estimated using weighted regressions; unweighted estimates are provided in Appendix Table A.1. Columns (1) through (4) use our stock-based proxy of intangible capital as the dependent variable, while Columns (5) through (8) use the (demeaned) log of operating expenses.

Across all specifications, the DiD coefficients are positive and statistically significant, indicating that improved liquidity is associated with higher intangible investment. The magnitudes are also economically meaningful. Column (3) shows that moving from the 25th to the 75th percentile of the $T_{2,i}$ distribution (a change of 3.41 units) increases intangible investment by approximately 3 percentage points ($\approx 0.009 \times 3.41$). Column (7) presents the corresponding estimate using our flow-based proxy. Despite relying on a different measure, the estimated effect is similar in magnitude, supporting the interpretation that improved liquidity facilitates

Table 2: Liquidity Shocks and Intangible Investment

Dependent Variable:	ln(Intan$_{it}$)				ln(Op Exp$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$T_{1,i}\times Post_t$	0.027**	0.066***			0.032***	0.037***		
	(0.011)	(0.010)			(0.004)	(0.004)		
$T_{2,i}\times Post_t$			0.009***	0.019***			0.009***	0.010***
			(0.003)	(0.003)			(0.001)	(0.001)
Nr. Obs.	238,461	228,782	238,461	228,782	339,760	333,857	339,760	333,857
R^2	0.897	0.901	0.897	0.901	0.962	0.967	0.962	0.967
Fixed Effects	Firm; Year							
Controls	base	cont	base	cont	base	cont	base	cont
Weights	Yes							

Notes: This table reports DiD estimates from equation (6). All regressions are estimated by OLS with weights and include firm and year fixed effects. Columns (1)–(4) use our stock-based proxy of intangible capital as the dependent variable, while Columns (5)–(8) use the log of operating expenses, demeaned at the 4-digit industry-year level. $T_{1,i}$ and $T_{2,i}$ denote the binary and continuous treatment definitions in (4) and (5), respectively. "Base" specifications include 2004 liquidity controls interacted with the post dummy; "Cont" specifications add time-varying liquidity controls. Robust standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

intangible investment.[15]

4.2 Robustness and Validation

We test the robustness of our baseline estimates through a series of complementary checks.

4.2.1 Alternative Treatment Definitions

Our baseline treatment variable defines as "treated" all firms that experienced a positive net liquidity shock in 2007, as in equation (3). Two potential concerns arise. First, truncating net trade credit exposure at zero may understate variation, since firms below the 60-day threshold could still face meaningful shifts in trade credit conditions. Second, relying on a single pre-reform year (2007) may introduce measurement error. We address both concerns with two alternative constructions.

Non-truncated measure. We first redefine the liquidity shock without truncating the net difference between DSO and DPO:

$$\text{Liquidity Shock}_i^{\text{pre}} = \text{DSO}_i^{\text{pre}} - \text{DPO}_i^{\text{pre}}. \tag{8}$$

[15]Because the dependent variable in Column (7) is demeaned, the coefficient reflects a proportional change relative to the sample mean.

This specification captures both tightening and loosening of trade credit and is not mechanically tied to the 60-day threshold.[16] Figure C.3 shows that pre-trends remain absent under this definition, and Appendix Table A.2 confirms that the DiD estimates are virtually identical to the baseline.

Multi-year average (2004–2007). Next, to mitigate noise from relying on 2007 alone, we compute the shock using average DSO and DPO values over 2004–2007. Figure C.4 again shows no systematic pre-trends, though estimates are somewhat noisier, consistent with reduced idiosyncratic variation from averaging. Appendix Table A.3 reports the corresponding DiD results, which closely match the baseline estimates in Table 2.

4.2.2 Additional Checks

We further test robustness by varying several aspects of the empirical design. Appendix Figure A.6 plots the coefficient of interest (β from equation (6)) across 48 alternative specifications that: (i) add firm-level productivity (TFPR) and compare time-invariant with time-varying liquidity controls; (ii) use both stock- and flow-based proxies for intangible investment; (iii) apply binary and continuous treatment definitions; (iv) exclude the financial crisis years (2008–2010); and (v) rely on the full unbalanced panel of firms.

Estimates are highly stable across all variations. Nearly all are positive and statistically significant, with only three exceptions in the unbalanced sample. Most fall well within the confidence interval of our baseline estimate (Table 2, column 1), reinforcing the reliability of our results.

4.2.3 Industry Heterogeneity

A potential concern raised in Section 3.5.1 is that our baseline estimates may be biased if the proxies for intangible investment captured acquisition activity rather than new investment. To address this, we use cleaner industry-level indicators of intangible intensity from Compustat and test whether the effect of liquidity shocks is stronger in more intangible-intensive sectors.

[16] While the 60-day benchmark reflects the reform's statutory cap, using it mechanically may add noise since we only observe proxies for trade credit conditions rather than contractual terms.

Table 3: Heterogeneity by Industry-level Intangible Intensity

Dependent Variable:	$\ln(\text{Intan}_{it})$					
	(1)	(2)	(3)	(4)	(5)	(6)
$T_{1,i} \times \text{Post}_t$	0.027**	−0.028	0.021			
	(0.011)	(0.026)	(0.013)			
$T_{1,i} \times \text{Post}_t \times \text{SG\&A Int}_s$		0.286***				
		(0.107)				
$T_{1,i} \times \text{Post}_t \times \text{R\&D Int}_s$			0.560**			
			(0.284)			
$T_{2,i} \times \text{Post}_t$				0.009***	−0.009	0.007*
				(0.003)	(0.008)	(0.004)
$T_{2,i} \times \text{Post}_t \times \text{SG\&A Int}_s$					0.097***	
					(0.033)	
$T_{2,i} \times \text{Post}_t \times \text{R\&D Int}_s$						0.194**
						(0.094)
Observations	238,461	219,464	219,464	238,461	219,464	219,464
R^2	0.897	0.897	0.897	0.897	0.897	0.897
Fixed Effects	Firm; Year					
Controls	base	base	base	base	base	base
Weights	Yes					

Notes: This table reports estimates from equation (9). The dependent variable is the (log of the) stock-based proxy of intangible capital. Measures of SG&A Intensity and R&D Intensity at the 4-digit NACE industry are based on Compustat data. All regressions are weighted and include baseline (time invariant) controls interacted with time dummies. Standard errors, clustered at the firm level, are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Evidence of larger effects in such sectors would support the interpretation that our results reflect investment in intangibles rather than M&A activity.

We estimate a triple-difference specification that interacts the treatment effect with industry-level intangible intensity:

$$\ln Y_{it} = \alpha + \beta_1 \cdot \text{Post}_t \times Tj, i + \beta_2 \cdot \text{Post}_t \times Tj, i \times \psi_s + \text{Post}_t \times X'_{it}\gamma + c_i + \delta_t + \varepsilon_{it}, \qquad (9)$$

where ψ_s denotes either SG&A or R&D intensity. This specification tests whether treatment effects are amplified in sectors where intangibles are more central, thereby helping to validate our empirical strategy.

Table 3 reports results using the stock-based proxy for intangible capital as the dependent variable. The flow-based proxy is excluded, as demeaning at the industry–year level mechanically removes sectoral heterogeneity.

We find that the liquidity shock has a significantly larger impact on intangible investment in sectors with higher SG&A and R&D intensity. This is difficult to reconcile with a purely

Table 4: Markups, Intangibles and Liquidity

Dependent Variable:	ln μ_{it}			
	(1)	(2)	(3)	(4)
ln Intan$_{it}$	0.183***		0.042**	
	(0.069)		(0.020)	
ln Op Exp$_{it}$		0.227***		0.117**
		(0.062)		(0.058)
Nr. Obs.	114,365	114,226	116,894	116,740
Fixed Effects	Firm; Year			
Controls	base		cont.	
Weights	Yes			
F-stat	37.04	122.7	160.6	114.1

Notes: This table reports 2SLS estimates of the effect of intangible investment on markups. The dependent variable is the log of the (baseline measure of) markup of firm i at time t(See Section 3.5.2 for details). We consider both our stock-based (ln Intan$_{it}$) and flow-based (ln Op Exp$_{it}$) intangible proxies as regressors. Both are instrumented using the liquidity shock from the binary treatment variable, i.e., Post$_t \times T_{1,i}$. Columns (1) and (2) include baseline (time invariant) controls interacted with time dummies, while Columns (3) and (4) include time-varying controls. Regressions are weighted and include firm and year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

mechanical M&A interpretation: if our measure mainly captured acquisitions, there would be no clear reason for the effects to vary with sectoral intangible intensity. In fact, acquisitions in intangible-intensive sectors usually involve more expensive targets, which would work against finding stronger effects. Instead, the results align more naturally with our intangible interpretation: firms that depend more heavily on intangible capital increase their investment more sharply when financing constraints are eased.

4.3 From Investment to Competitive Advantage

Table 4 reports estimates from the baseline IV specification in equation (7), where the dependent variable is the log (baseline) markup of firm i in year t. The key explanatory variable is intangible investment, proxied alternatively by our two measures and instrumented with the liquidity shock defined by the binary treatment interaction Post$_t \times T_{1,i}$.

Across specifications, the coefficients on intangible investment are positive and statistically significant, implying that firms investing more heavily in intangibles charge higher markups. The instruments perform well, with first-stage F-statistics comfortably above the conventional Stock and Yogo (2005) threshold of 10.

Columns (1) and (2), which include only baseline controls, imply that a 10% increase in

intangible investment is associated with an increase of approximately 2% in markups. Columns (3) and (4) add continuous controls for liquidity and yield slightly smaller point estimates, but the effects remain statistically significant and economically meaningful.

Appendix Tables A.5 and A.6 report corresponding estimates using the continuous treatment definition and an unweighted specification, respectively. The results remain highly consistent across these alternative estimation strategies.

4.3.1 Alternative Markup Definitions

As discussed in Section 3.5.2, a key concern is that our baseline results may be influenced by the construction of markups. In particular, estimated markups might be mechanically correlated with intangible capital or biased due to the absence of firm-level price data in the estimation of output elasticities.

Appendix Table A.7 replicates the baseline IV specification from equation (7) using six alternative markup measures and two additional proxies for competitive advantage, with log intangible capital as the key explanatory variable. Appendix Table A.8 presents analogous estimates using our flow-based proxy for intangible investment. In both tables, columns (1)–(12) report results across the six markup definitions, while columns (13)–(16) examine market shares and total exports. Even-numbered columns include baseline controls interacted with the post-reform indicator, whereas odd-numbered columns use time-varying controls. All regressions are weighted; unweighted results are almost identical.

Across all six markup definitions, the results remain robust. Point estimates are highly significant and broadly similar in magnitude, with somewhat larger effects when markups are based on cost-share approximations of output elasticities. Columns (13)–(16) further show that intangible investment is positively associated with market shares and total exports.

These patterns suggest that neither intangible measurement error nor price-related biases drive our findings, reinforcing the view that the observed link between intangibles and markups reflects improvements in competitive outcomes.

4.4 Testing for Alternative Channels

We now examine whether the observed effects of the liquidity shock on firm performance operate primarily through intangible investment, rather than through other investment or financing channels. This analysis serves two purposes. First, it provides a direct test of the exclusion restriction underlying our IV strategy, verifying that intangible capital is the primary channel through which liquidity influences markups. Second, it offers empirical validation of Testable Prediction 3 from Section 2.2, which posits that liquidity shocks should raise intangible investment more than tangible investment.

Appendix Figure C.5 replicates our event study analysis using tangible capital as the outcome. Across specifications, both pre- and post-reform coefficients are relatively small and statistically insignificant. This contrasts sharply with our earlier findings on intangible investment, showing that the liquidity shock triggered a disproportionate response in intangibles over and above tangible capital, consistent with the theoretical mechanisms discussed in Section 2.2. This placebo test strongly supports the idea that liquidity unlocked investment mainly along the intangible margin.

We further corroborate this result using DiD regressions with additional outcomes, including long-term debt and leverage. The idea is that trade credit reform might have influenced other firm-level decisions that could also enhance markups, even in the absence of intangible investment. For instance, improved borrowing capacity could have enabled firms to finance expansion or acquisitions, thereby strengthening their market position. Alternatively, the reform may have enhanced firms' bargaining power by shifting payment terms in their favor, allowing them to capture a larger share of surplus. If such channels were active, we would expect to see corresponding effects on debt or leverage metrics.

Table 5 summarizes the results. Most estimated effects are small and statistically insignificant; when significant, the coefficients are negative. Notably, this includes a modest but statistically significant decline in tangible assets, suggesting that liquidity may have been reallocated from physical to intangible assets at the margin.[17]

[17] One interpretation is that firms facing improved liquidity shifted resources toward more scalable, strategic investments in intangibles, potentially crowding out physical capital. We do not pursue this mechanism further,

Table 5: Exclusion Restriction Test: Alternative Channels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation:	DID OLS							
Dep. Variable:	ln Tang. Assets$_{it}$		ln Fixed Assets$_{it}$		ln Long-Term Debt$_{it}$		ln DTA$_{it}$	
$T_{1,i}\times Post_t$	−0.017**		0.001		−0.030**		−0.003	
	(0.008)		(0.008)		(0.015)		(0.014)	
$T_{2,i}\times Post_t$		−0.003		0.000		−0.006		0.002
		(0.002)		(0.002)		(0.004)		(0.004)
Nr. Obs.	261,118	261,118	375,899	375,899	206,632	206,632	206,631	206,631
R^2	0.929	0.929	0.925	0.925	0.747	0.747	0.598	0.598
Fixed Effects	Firm; Year							
Controls	Base							
Weights	Yes							

Notes: This table reports estimates from equation (6), testing for alternative channels that might violate the exclusion restriction. The dependent variable is log tangible assets in columns (1)–(2), log fixed assets in (3)–(4), log long-term debt in (5)–(6), and log debt-to-assets ratio in (7)–(8). All regressions include firm and year fixed effects, are weighted, and include baseline controls. Standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Taken together, these findings confirm that the effects of the liquidity shock were specific to intangible investment. They support the exclusion restriction underlying our IV design, provide empirical validation for Testable Prediction 3 in Section 2.2, and reinforce our interpretation that improved access to short-term liquidity primarily enhances competitive advantage by enabling greater investment in intangibles.

Moreover, the absence of significant effects on tangible assets, long-term debt, or leverage further challenges an acquisition-based interpretation of our results, a concern we explicitly raised earlier in the paper. If the observed increase in intangible assets were primarily driven by mergers and acquisitions, we would expect to observe corresponding increases in fixed assets or debt, since acquisitions typically involve both tangible and intangible capital and are often financed externally. Yet we find no such patterns: tangible assets, fixed assets, and leverage remain flat or even decline slightly following the reform.

While we cannot entirely rule out the possibility that some acquisitions contributed to the observed increase in intangibles, the lack of broader balance sheet expansion, combined with the industry-level heterogeneity documented in Section 4.2.3, makes it difficult to reconcile with an M&A-based explanation. Instead, the evidence suggests that firms are using improved liquidity to scale up internal investment in intangible capital, rather than reallocating resources through corporate restructuring.

but it is consistent with the lack of post-reform trends in tangible capital shown in Appendix Figure C.5.

5 Concluding Remarks

This paper provides new evidence on the role of short-term financial conditions in shaping firms' investment decisions and product market outcomes. The findings underscore the importance of liquidity in enabling firms to invest in intangible assets, a class of capital that has become increasingly central to firm performance and market dynamics.

A key finding is that the allocation of intangible investment is influenced not only by fundamentals but also by access to finance. When external financing is limited, internal liquidity becomes the binding constraint. As a result, even temporary liquidity shocks can have persistent effects on investment and market structure. Policies that ease such constraints can alter the composition of investment and resource allocation.

The results also point to a broader role for financial conditions in shaping competitive dynamics. While product market outcomes are often modeled as functions of technology or market structure, our findings suggest that financial frictions contribute to the evolution of competition by influencing which firms are able to scale and differentiate. This opens up several directions for future research. One is to quantify the general equilibrium implications of liquidity constraints for aggregate productivity and markup dispersion. Another is to examine how institutional features shape the distributional effects of liquidity constraints across firms and industries.

Ultimately, these findings underscore the potential value of integrating financial considerations into policy discussions on innovation and industrial development. In economies where intangible capital plays a growing role, strengthening the financial infrastructure that supports such investment may not only promote firm-level growth but also improve allocative efficiency. We view the study of the distributional consequences of such interventions, both within and across countries, as a promising area for future research.

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Online Appendix

"Liquidity as Competitive Advantage: The Role of Intangibles"

Carlo Altomonte, Monica Morlacco, Tommaso Sonno, Domenico Favoino

This Appendix is organized as follows. Section A contains additional tables and figures referenced in the text. Section B describes our markup estimation strategy and the construction of the different markup measures. Section C provides additional details on the analysis of pre-trends.

A Additional Figures and Tables

Table A.1: Liquidity and Intangibles: Unweighted Regressions

Dependent Variable:	ln(Intan$_{it}$)				ln(Op Exp$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$T_{1,i}\times$Post$_t$	0.025**	0.060***			0.021***	0.029***		
	(0.010)	(0.010)			(0.004)	(0.003)		
$T_{2,i}\times$Post$_t$			0.008***	0.018***			0.006***	0.008***
			(0.003)	(0.003)			(0.001)	(0.001)
Nr. Obs.	252,143	241,810	252,143	241,810	363,090	356,615	363,090	356,615
R^2	0.892	0.896	0.892	0.896	0.963	0.968	0.963	0.968
Fixed Effects	Firm; Year							
Controls	base	cont	base	cont	base	cont	base	cont
Weights	No							

Notes: This table reports DiD estimates of the effect of liquidity shocks on intangible investment. All specifications are estimated by OLS without weights. Columns (1)–(4) use our stock-based proxy of intangible capital as the dependent variable; columns (5)–(8) use the demeaned log of operating expenses instead. $T_{1,i}$ and $T_{2,i}$ correspond to the binary and continuous treatment definitions in (4) and (5), respectively. "Base" refers to 2004 liquidity controls interacted with the post dummy; "Cont" includes time-varying liquidity controls. All regressions include firm and year fixed effects. Standard errors, clustered at the firm level, are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Liquidity and Intangibles: No Truncation of Trade Credit Exposure

Dependent Variable:	ln(Intan$_{it}$)				ln(Op Exp$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T$_{1,i}$×Post$_t$	0.020*	0.054***			0.033***	0.031***		
	(0.010)	(0.010)			(0.004)	(0.004)		
T$_{2,i}$×Post$_t$			0.006**	0.015***			0.008***	0.009***
			(0.003)	(0.003)			(0.001)	(0.001)
Nr. Obs.	238,461	228,782	238,461	228,782	339,760	333,857	339,760	333,857
R^2	0.897	0.901	0.897	0.901	0.962	0.967	0.962	0.967
Fixed Effects	Firm; Year							
Controls	base	cont	base	cont	base	cont	base	cont
Weights	Yes							

Notes: This table reports DiD estimates of the effect of liquidity shocks on intangible investment. Liquidity shock is based on equation (C.4), and does not truncate the net shock to DSO and DPO at zero in the pre reform years. Columns (1)–(4) use our stock-based proxy of intangible capital as the dependent variable; columns (5)–(8) use the demeaned log of operating expenses instead. $T_{1,i}$ and $T_{2,i}$ correspond to the binary and continuous treatment variables defined in equations (4) and (5), respectively. "Base" includes pre-policy (2004) liquidity controls interacted with the post dummy; "Cont" includes time-varying liquidity controls. All regressions are estimated by OLS and weighted. Robust standard errors clustered at the firm level are reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Liquidity and Intangibles: Multi-Year Average Treatment Definition

Dependent Variable:	ln(Intan$_{it}$)				ln(Op Exp$_{it}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T$_{1,i}$×Post$_t$	0.025**	0.069***			0.034***	0.037***		
	(0.011)	(0.010)			(0.004)	(0.004)		
T$_{2,i}$×Post$_t$			0.007**	0.020***			0.009***	0.010***
			(0.003)	(0.003)			(0.001)	(0.001)
Nr. Obs.	238,461	228,782	238,461	228,782	339,760	333,857	339,760	333,857
R^2	0.897	0.901	0.897	0.901	0.962	0.967	0.962	0.967
Fixed Effects	Firm; Year							
Controls	base	cont	base	cont	base	cont	base	cont
Weights	Yes							

Notes: This table reports DiD estimates of the effect of liquidity shocks on intangible investment using the multi-year average treatment definition. Liquidity shocks are based on the average of DSO and DPO from 2004–2007. Columns (1)–(4) use our stock-based proxy of intangible capital as the dependent variable; columns (5)–(8) use the demeaned log of operating expenses instead. $T_{1,i}$ and $T_{2,i}$ correspond to the binary and continuous treatment variables defined in equations (4) and (5), respectively. "Base" includes pre-policy (2004) liquidity controls interacted with the post dummy; "Cont" includes time-varying liquidity controls. All regressions are estimated by OLS and weighted. Robust standard errors clustered at the firm level are reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Intangible Intensity Across Industries

Industry (NACE Rev. 4)		SG&A / Sales	R&D / Sales
10	Food Products	0.31	0.00
11	Beverages	0.24	0.01
13	Textiles	0.22	0.02
14	Wearing Apparel	0.24	0.01
15	Leather Products	0.30	0.01
16	Wood Products	0.24	0.01
17	Pulp, Paper, & Products	0.13	0.01
18	Printing and Publishing	0.21	0.03
19	Coke and Refined Petroleum	0.14	0.02
20	Chemicals and Products	0.28	0.03
21	Pharma Products	0.55	0.56
22	Rubber & Plastic Products	0.22	0.02
23	Non-metallic Mineral Products	0.17	0.00
24	Basic Metals	0.19	0.01
25	Fabricated Metal Products	0.16	0.01
26	Computer & Electronics	0.32	0.06
27	Electrical Equipment	0.21	0.02
28	Machinery & Equipment n.e.c.	0.21	0.02
29	Motor Vehicles, (Semi-)Trailers	0.13	0.01
30	Other Transport Equipment	0.17	0.03
31	Furniture	0.20	0.01
32	Other Manufacturing	0.35	0.04
33	Repair and Installation of Machinery and Equipment	0.19	0.04

Notes: The table reports intangible intensity calculated from Compustat data for all U.S. firms from 2004 to 2007. The Overall Intangible intensity is measured as firm-level SG&A over total sales. The R&D intensity is measured as firm-level R&D expenses over total sales. Firm-level estimates are aggregated at the sector level by taking the median-firm intangible intensity ratio of each sector. Standard concordance tables from eurostat.com are used to convert NAICS industries in Compustat to NACE industries in Orbis. Source: authors' calculations on Compustat data.

Table A.5: Intangibles and Markups: Continuous Treatment

Dependent Variable:	ln μ_{it}			
	(1)	(2)	(3)	(4)
ln Intan$_{it}$	0.165***		0.036*	
	(0.063)		(0.020)	
ln Op Exp$_{it}$		0.229***		0.103*
		(0.068)		(0.059)
Nr. Obs.	114,365	114,226	116,894	116,740
Fixed Effects	Firm; Year			
Controls	base		cont.	
Weights	Yes			
F-stat	39.19	104.0	161.3	105.8

Notes: This table reports 2SLS estimates of the effect of intangible investment on markups. The dependent variable is the log of the markup of firm i at time t, constructed as detailed in Section 3.5.2. We consider both our stock-based (ln Intan$_{it}$) and flow-based (ln Op Exp$_{it}$) proxies for intangible capital as regressors. Both are instrumented using the liquidity shock from the continuous treatment variable, i.e., $\text{Post}_t \times T_{2,i}$. Columns (1) and (2) include baseline (time invariant) controls interacted with time dummies, while Columns (3) and (4) include time-varying controls. Regressions are weighted and include firm and year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Intangibles and Markups: Unweighted Regressions

Dependent Variable:	ln μ_{it}			
	(1)	(2)	(3)	(4)
ln Intan$_{it}$	0.164**		0.044**	
	(0.065)		(0.020)	
ln Op Exp$_{it}$		0.263***		0.136**
		(0.083)		(0.066)
Nr. Obs.	114,612	114,471	117,144	116,988
Fixed Effects	Firm; Year			
Controls	base		cont.	
Weights	No			
F-stat	34.63	75.05	131.0	82.41

Notes: This table reports 2SLS estimates of the effect of intangible investment on markups. The dependent variable is the log of the markup of firm i at time t, constructed as detailed in Section 3.5.2. We consider both our stock-based (ln Intan$_{it}$) and flow-based (ln Op Exp$_{it}$) proxies for intangible capital as regressors. Both are instrumented using the liquidity shock from the binary treatment variable, i.e., $\text{Post}_t \times T_{1,i}$. All regressions include firm and year fixed effects but are estimated without weights. Robust standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Alternative Markup Definitions and Performance Measures

Dep. Var.:	$\ln \mu_{it}$							
	TL (baseline)		CD (Approach 1)		TL (Approach 2)		TL (No Intan.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Intan$_{it}$	0.183***	0.042**	0.278**	0.094**	0.191***	0.044**	0.164**	0.032
	(0.069)	(0.020)	(0.123)	(0.039)	(0.072)	(0.020)	(0.067)	(0.020)
Nr. Obs.	114,365	116,894	121,427	124,155	114,308	116,833	114,243	116,786
Controls	base	cont	base	cont	base	cont	base	cont
Weights	Yes							
F test	37.04	160.6	30.87	148.1	35.95	157.6	33.94	154.2

Dep. Var.:	$\ln \mu_{it}$				Other Performance Measures			
	Cost Share		Cost Share (+ Intan)		Market Share		Exports	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
ln Intan$_{it}$	0.805**	0.269***	0.610**	0.186***	0.919**	0.417***	0.589	0.483
	(0.379)	(0.057)	(0.297)	(0.049)	(0.389)	(0.079)	(1.044)	(0.428)
Nr. Obs.	225,726	222,047	225,726	222,047	238,461	228,782	103,945	100,684
Controls	base	cont	base	cont	base	cont	base	cont
Weights	Yes							
F test	20.43	162.5	20.43	162.5	24.22	161.4	5.275	27.74

Notes: This table reports 2SLS estimates of the effect of intangible investment on markups. The explanatory variable is our stock-based proxy for intangible capital, instrumented using the liquidity shock from the binary treatment variable, i.e., $\text{Post}_t \times T_{1,i}$. The dependent variables are constructed as follows: "TL" indicates a Translog production function; "CD" indicates a Cobb–Douglas production function. "Approach 1" treats intangible capital as a TFP-enhancing input; "Approach 2" includes it directly as a factor in the production function. "TL (No Intan)" excludes intangibles entirely. "Cost Share" approximates the output elasticity of materials using the observed material cost share, thereby avoiding production function estimation. See Appendix B.3 for further details on markup construction. Columns (11)–(16) report results using market share and total firm exports as dependent variables. "Base" and "Cont" denote the baseline and extended control sets, respectively. All regressions include firm and year fixed effects and are weighted. Robust standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

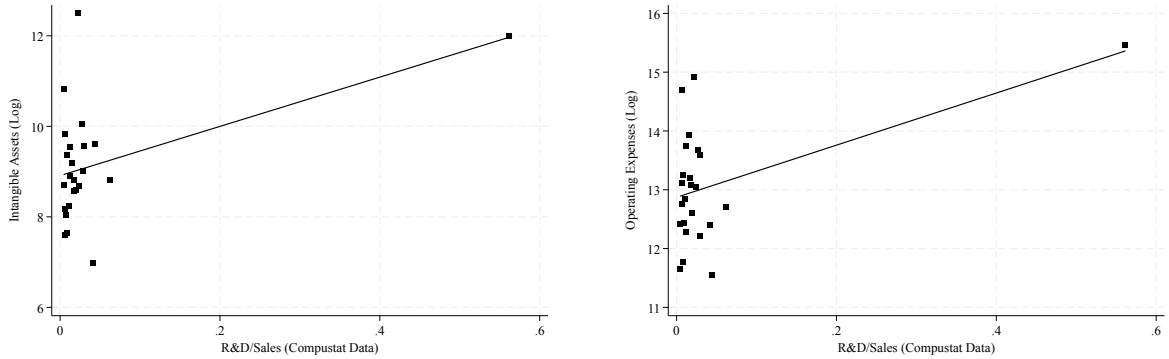
Table A.8: Alternative Markup Definitions and Performance Measures

Dep. Var.:	$\ln \mu_{it}$							
	TL (baseline)		CD (Approach 1)		TL (Approach 2)		TL (No Intan.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Op Exp$_{it}$	0.227***	0.117**	0.336***	0.270**	0.232***	0.121**	0.191***	0.084
	(0.062)	(0.058)	(0.122)	(0.120)	(0.063)	(0.058)	(0.057)	(0.054)
Nr. Obs.	114,226	116,740	121,263	123,974	114,169	116,679	114,103	116,631
Controls	base	cont	base	cont	base	cont	base	cont
Weights				Yes				
F test	122.7	114.1	101.7	96.20	123.8	113.4	128.2	123.8

Dep. Var.:	$\ln \mu_{it}$				Other Performance Measures			
	Cost Share		Cost Share (+ Intan)		Market Share		Exports	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
ln Op Exp$_{it}$	0.598***	0.488***	0.454***	0.340***	0.800***	0.858***	36.990	2.406*
	(0.102)	(0.075)	(0.091)	(0.068)	(0.078)	(0.064)	(380.883)	(1.298)
Nr. Obs.	331,117	325,410	331,117	325,410	339,760	333,857	136,236	134,263
Controls	base	cont	base	cont	base	cont	base	cont
Weights				Yes				
F test	251.1	351.3	251.1	351.3	220.2	348.1	0.0361	23.57

Notes: This table reports 2SLS estimates of the effect of intangible investment on markups. The explanatory variable is our flow-based proxy for intangible investment, instrumented using the liquidity shock from the binary treatment variable, i.e., $\text{Post}_t \times T_{1,i}$. The dependent variables are constructed as follows: "TL" indicates a Translog production function; "CD" indicates a Cobb–Douglas production function. "Approach 1" treats intangible capital as a TFP-enhancing input; "Approach 2" includes it directly as a factor in the production function. "TL (No Intan)" excludes intangibles entirely. "Cost Share" approximates the output elasticity of materials using the observed material cost share, thereby avoiding production function estimation. See Appendix B.3 for further details on markup construction. Columns (11)–(16) report results using market share and total firm exports as dependent variables. "Base" and "Cont" denote the baseline and extended control sets, respectively. All regressions include firm and year fixed effects and are weighted. Robust standard errors clustered at the firm level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

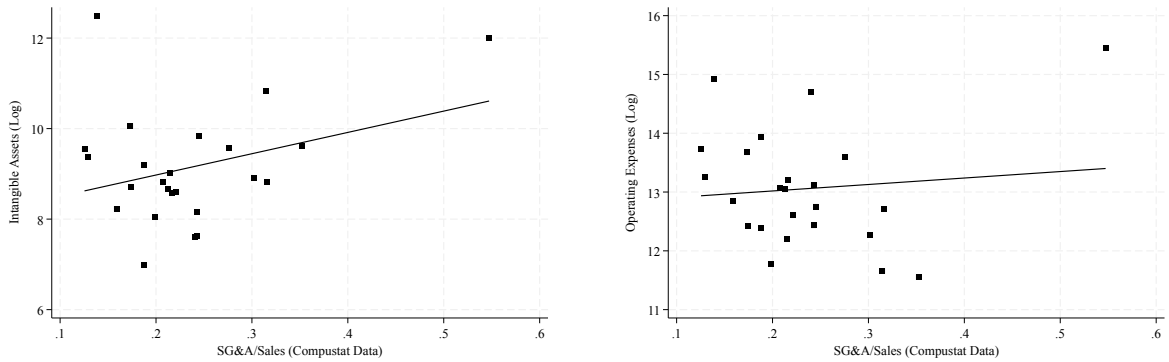
Figure A.1: Validation of Intangible Proxies Using Industry-Level R&D Intensity



(a) Intangible Assets (Log) (b) Operating Expenses (Log)

Notes: Each panel shows the relationship between our Orbis-based proxies of intangible activity (vertical axis), computed as the median at the 2-digit NACE level, and industry-level R&D intensity (horizontal axis), computed from Compustat data as the median R&D-to-sales ratio. Each dot represents a 2-digit industry. A linear fit is shown in black. The raw correlations are 0.45 and 0.52, respectively.

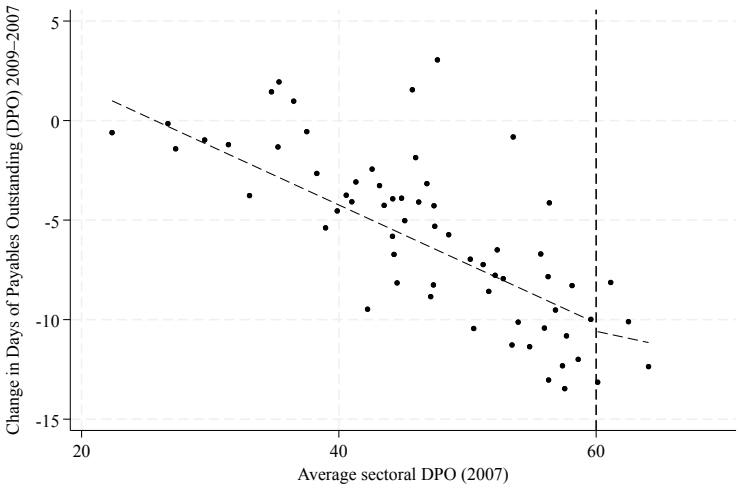
Figure A.2: Validation of Intangible Proxies Using Industry-Level SG&A Intensity



(a) Intangible Assets (Log) (b) Operating Expenses (Log)

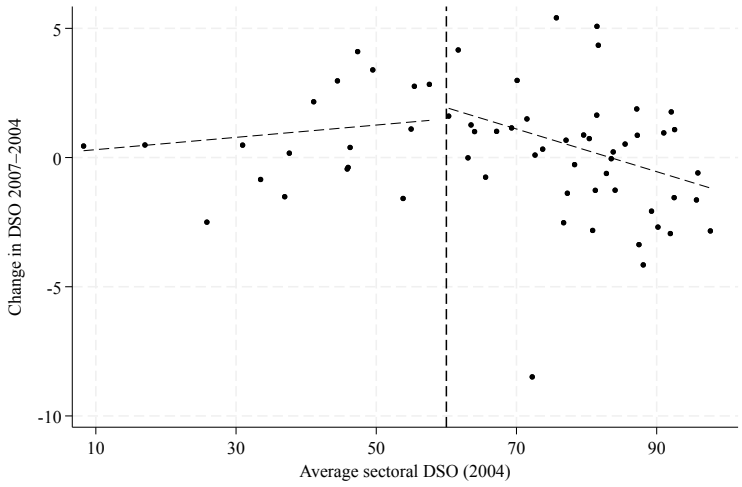
Notes: Each panel shows the relationship between our Orbis-based proxies of intangible activity (vertical axis), computed as the median at the 2-digit NACE level, and industry-level SG&A intensity (horizontal axis), computed from Compustat data as the median SG&A-to-sales ratio. Each dot represents a 2-digit industry. A linear fit is shown in black. The raw correlations are 0.21 and 0.02, respectively.

Figure A.3: Impact of the policy on DPO (2007-2009)



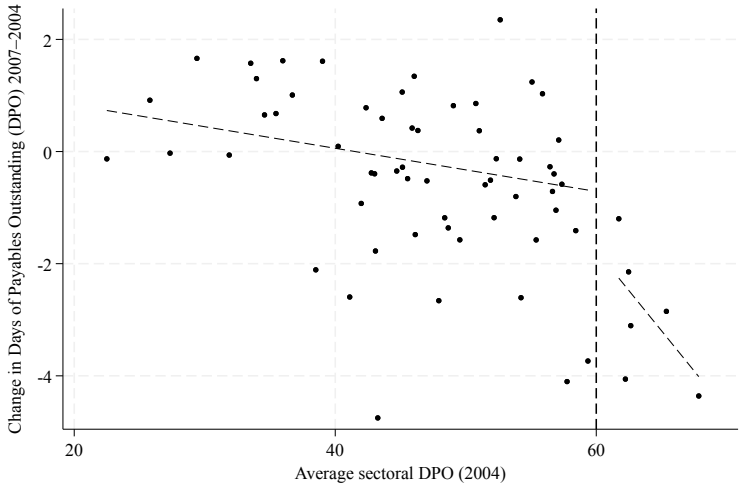
Notes: The figure plots the change in average firm-level DPO between 2007 and 2009 as a function of initial 2007 sectoral DPO. Firms are first grouped into percentiles of sectoral DPO in 2007, and each point plots the mean change within a percentile bin against its average initial DPO. The dashed lines report linear fits estimated separately for sectors below and above the 60-day policy cap.

Figure A.4: Impact of the policy on DSO, Placebo (2004-2007)



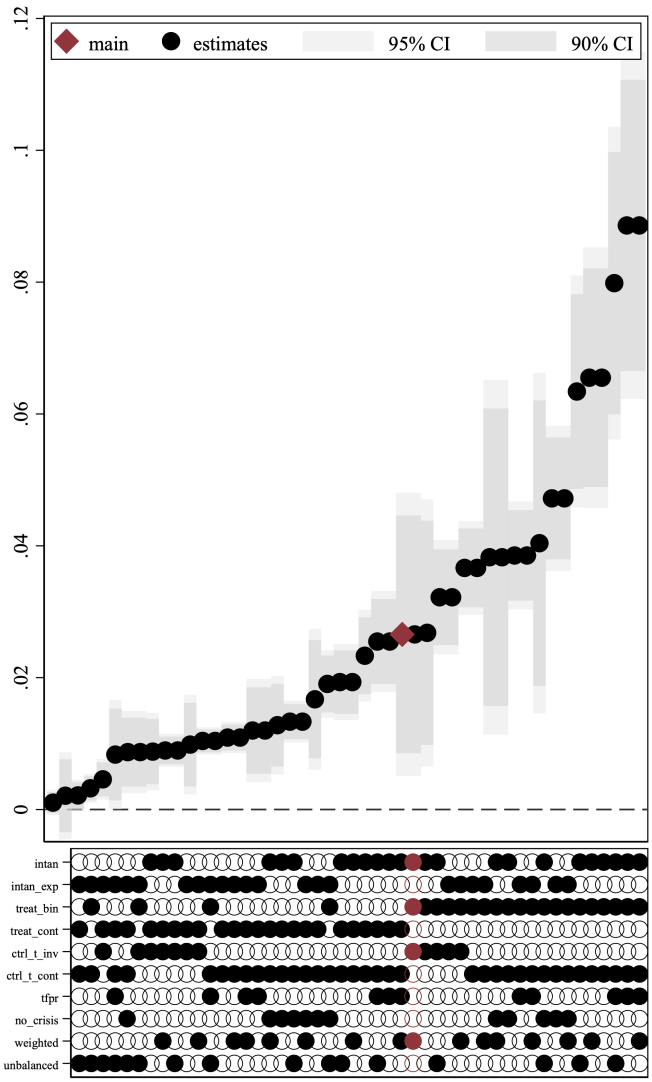
Notes: The figure plots the change in average firm-level DSO between 2004 and 2007 against initial 2004 sectoral DSO. Firms are grouped into percentiles of their sector's 2004 DSO, and each point shows the mean change within a percentile bin against its average initial DSO. Dashed lines report linear fits estimated separately below and above the 60-day threshold. Because this period predates the reform, no systematic relationship is expected, and the figure serves as a placebo test of our exposure measure.

Figure A.5: Impact of the policy on DPO, Placebo (2004-2007)



Notes: The figure plots the change in average firm-level DPO between 2004 and 2007 against initial 2004 sectoral DPO. Firms are grouped into percentiles of their sector's 2004 DPO, and each point shows the mean change within a percentile bin against its average initial DPO. Dashed lines report linear fits estimated separately below and above the 60-day threshold. Because this period predates the reform, no systematic relationship is expected, and the figure serves as a placebo test of our exposure measure.

Figure A.6: Liquidity and Intangible Investment: Robustness



Notes: This figure displays specification curves for 48 variants of regression equation (**??**), where the dependent variable is either (i) log intangible assets from the balance sheet or (ii) log intangible expenditures, defined as demeaned operating expenses unrelated to variable production. Each point corresponds to a different specification, with 90% and 95% confidence intervals. The red diamond denotes the baseline specification (column 1 of Table **??**). Specification features (below the graph) indicate whether controls, sample restrictions, or alternative measures were included. The features are as follows: 'intan' refers to the baseline intangible measure; 'intan_exp' measures (log) demeaned operating expenses. The terms 'treat_bin' and 'treat_cont' indicate binary and continuous treatment, respectively. 'ctrl_t_inv' and 'ctrl_t_cont' denote time-invariant and time-varying controls. 'tfpr' stands for productivity control; 'no_crisis' excludes the years 2008–2010 from the analysis. 'unbalanced' refers to the full unbalanced sample of firms. All specifications include firm and year fixed effects. The figure was constructed using the STATA package `speccurve`.

B Estimation of Firm-Level Markups

This section outlines our procedure for estimating firm-level markups. Section B.1 describes how we estimate production functions using firm-level balance sheet data. We depart from standard approaches by explicitly incorporating intangible expenditures, either as productivity-enhancing inputs (A1) or as direct inputs in production (A2). This choice is motivated by recent theories linking intangibles to rising markups, summarized in Section 2. Accounting for intangibles is essential for obtaining unbiased estimates of output elasticities and, by extension, of markups and firm productivity (See, e.g., De Loecker et al., 2020; Traina, 2018).

Section B.2 describes how we compute firm-level markups using the estimated output elasticity of materials and observed expenditure shares. It also outlines the construction of alternative markup measures based on different production function specifications, which we use to assess the robustness of our findings.

B.1 Production Function Estimation

We estimate production functions separately for each two-digit industry. Our benchmark specification is a Translog production function in three inputs: materials, labor, and capital. We treat materials as a flexible input, while labor and capital are treated as dynamic inputs.

For each industry s, we specify the production function as follows:

$$\begin{aligned} q_{it} = &\beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + \beta_{kk} k_{it}^2 + \beta_{ll} l_{it}^2 + \beta_{mm} m_{it}^2 \\ &+ \beta_{kl} k_{it} l_{it} + \beta_{lm} l_{it} m_{it} + \beta_{km} k_{it} m_{it} + z_{it} + \epsilon_{it}, \end{aligned} \tag{PF1}$$

where lowercase letters denote logs of the corresponding variables. Here, q_{it} is (log) output,

k_{it}, l_{it}, and m_{it} are (log) capital, labor, and materials, respectively. The term $z_{it} = \ln Z_{it}$ captures firm-level productivity, and ϵ_{it} is an error term capturing measurement error or transitory productivity shocks.

We consider two approaches to incorporating intangibles into the production process.

Approach 1 (A1): Intangibles as productivity-enhancing inputs In the first approach, we model firm productivity z_{it} as a function of a firm-specific component and its investment in intangibles:

$$\begin{aligned} z_{it} &= \omega_{it} + \phi(s_{it}, \omega_{it}) \\ &= h(\omega_{it}, s_{it}), \end{aligned}$$

where s_{it} denotes log intangible expenditures. The function $\phi(s_{it}, \omega_{it})$ captures the possibility that firms investing more in intangibles experience higher productivity. This formulation allows for complementarity between intangible investments and firm-specific productivity, in line with recent theories of intangible capital (e.g., De Ridder, 2024; Hsieh and Rossi-Hansberg, 2023).

Approach 2 (A2): Intangibles as Direct Inputs As an alternative, we follow the approach in De Loecker et al. (2020), treating intangibles as a direct input in the production function:

$$q_{it} = \beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + \beta_s s_{it} + \omega_{it} + \varepsilon_{it}. \tag{PF2}$$

Here, $s_{it} = \ln(S_{it})$ denotes log intangible investment (deflated).[1] In this setting, intangibles enter the production function analogously to physical inputs, contributing directly to output.

Estimating production functions such as equations (A1) or (A2) using firm-level balance sheet data involves two primary challenges. The first is the well-known *simultaneity bias*, which arises because input choices may be correlated with unobserved productivity shocks (ω_{it}). The second is the *price bias*, which stems from the fact that available data typically record revenues and expenditures rather than physical quantities, introducing potential measurement error in both input and output prices (De Loecker and Goldberg, 2014).

To address simultaneity bias, we adopt a control function approach that specifies a law of motion for productivity and exploits the differential timing of input adjustments. In particular, we assume that material inputs respond contemporaneously to productivity shocks, while capital and labor are predetermined. This structure allows us to identify the output elasticity of materials, which we use to compute firm-level markups.

We address the implications of price bias in greater detail in Section B.1.2.

B.1.1 Control Function Approach

Drawing on the idea that input demand is shaped by productivity and other observable firm characteristics, we can express productivity as an unknown function of observables:

$$\omega_{it} = h_t(d_{it}, s_{it}, k_{it}, \zeta_{it}), \tag{25}$$

[1] Equation (A2) uses a Cobb-Douglas form for clarity, but the actual implementation uses the more flexible Translog specification.

where d_{it} is the chosen control variable, in our case, materials m_{it}.[2] We include intangibles s_{it} in the control function to account for its productivity-enhancing role, as explained above. The term ζ_{it} captures factors such as input or output market conditions that influence input demand independently of productivity, capital, and labor. These shifters allow for firm-level variation in input use, conditional on other state variables.

Estimation proceeds in two stages. In the first stage, we project output on a flexible polynomial of all inputs and intangibles to net out measurement error and unanticipated output shocks:

$$q_{it} = \phi_t(m_{it}, s_{it}, k_{it}, l_{it}, \zeta_{it}) + \varepsilon_{it}. \tag{26}$$

The second stage identifies the output elasticity of the variable input by imposing a law of motion for productivity, assumed to evolve as $z_{it} = g(z_{it-1}) + \xi_{it}$. We then form moment conditions that exploit the idea that while variable input use responds to productivity, dynamic input use does not. This leads to the following orthogonality condition:

$$\mathbb{E}\left(\xi_{it}(\theta_t)\begin{bmatrix} m_{it-1} \\ k_{it} \\ l_{it} \end{bmatrix}\right) = 0, \tag{B.1}$$

where $\xi_{it}(\theta_t)$ represents the innovation to productivity, constructed by projecting the productivity estimate $\omega_{it}(\theta_t)$ onto its lagged value.

For the second specification (A2), we include an additional moment condition to identify the role of overhead inputs, by requiring that the productivity innovation is orthogonal to the overhead input: $\mathbb{E}\left[\xi_{it}(\theta_t)s_{it}\right]$. For implementation, we follow the procedure suggested by

[2]See Ackerberg et al. (2007) for an in-depth discussion.

Woolridge(2009) that forms moments on the joint error term $(\xi_{it}, \varepsilon_{it})$.

B.1.2 Output and Input Price Bias

Production data typically report revenues and expenditures rather than physical quantities. As a result, unobserved input and output prices introduce endogeneity, especially in the presence of product differentiation (De Loecker and Goldberg, 2014; Foster et al., 2008).

To address input price bias, we assume:

A1 *Firms take input prices* P_{it}^X *for* $X = K, M, L$ *as given.*

Under A1, deflated expenditures are valid input proxies as long as cross-firm variation in input prices is limited (De Loecker et al., 2016), an assumption we maintain throughout.

Output price bias is more difficult to address, as output quantities are unobserved and markups vary across firms. This can lead to biased estimates of output elasticities in production function estimation. However, we mitigate this concern in two ways.

First, some of our markup measures rely on cost-share approximations rather than estimated output elasticities. These approaches assume constant returns to scale and compute markups as the ratio of revenue to total costs (with and without intangibles). While the constant returns assumption is strong, this method bypasses the problem of unobserved output prices entirely.

Second, for specifications that assume a Cobb-Douglas production function, markups vary over time primarily through variation in observed revenue shares, which are directly influenced by balance sheet variables.

B.2 Markups

After estimating the output elasticities, we compute firm-level markups following the approach of De Loecker and Warzynski (2012), which builds on the insight of Hall (1988). Under Assumption A1, the first-order condition for cost minimization with respect to materials implies:

$$\mu_{it} = \frac{\theta^m_{it}}{\alpha^m_{it}},$$

where $\theta^m_{it} \equiv \frac{dq_{it}}{dm_{it}}$ is the output elasticity of materials, and $\alpha^m_{it} \equiv \frac{E^m_{it}}{R_{it}}$ is the share of material expenditures in total revenue. Both expenditures on materials E^m_{it} and total revenues R_{it} are directly observed in the data. As standard, we use $\hat{R}_{it} \equiv \exp(r_{it} - \hat{\varepsilon}_{it})$ instead of balance-sheet revenues, which are revenues purged of measurement error using the residual from the first-stage regression, to correct for measurement error (De Loecker and Warzynski, 2012).

B.3 Markup Variations

To assess the robustness of our results, we construct six alternative markup measures based on different production function specifications and estimation strategies:

1. **Baseline:** Markups are computed using output elasticities from the translog production function (A1). Under any translog production function specification in equation (A1), the output elasticity of materials is computed as:

$$\hat{\theta^m_{it}} \equiv \frac{dq_{it}}{dm_{it}} = \hat{\beta}_m + 2\hat{\beta}_{mm} m_{it} + \hat{\beta}_{km} k_{it} + \hat{\beta}_{lm} l_{it}.$$

2. **Cobb–Douglas:** Markups are computed using output elasticities from a Cobb-Douglas version of (A1). In this case, the output elasticity of materials corresponds directly to

the estimated coefficient on m_{it}.

3. **Intangibles as Input:** Markups are computed using output elasticities from (A2).

4. **No Intangibles:** Markups are computed using output elasticities from (A2), but excluding intangibles from both the input bundle and the control function.

5. **Cost Share Approximation:** Lastly, we approximate the output elasticity of materials by its cost share.[3] We first consider the following definition of total cost:

$$\theta_{it}^{M} \approx \frac{E_{it}^{M}}{E_{it}^{M} + E_{it}^{L} + E_{it}^{K}},$$

where E_{it}^{M}, E_{it}^{L}, and E_{it}^{K} denote expenditures on materials, labor, capital, respectively. This yields the implied markup:

$$\mu_{it} = \frac{R_{it}}{E_{it}^{M} + E_{it}^{L} + E_{it}^{K}}.$$

6. **Cost Share Approximation with Intangibles:** We then repeat the same approach, but this time including intangibles along with total costs, yielding the implied markup:

$$\mu_{it} = \frac{R_{it}}{E_{it}^{M} + E_{it}^{L} + E_{it}^{K} + E_{it}^{S}},$$

where E_{it}^{S} denotes expenditures on intangibles. In this case, intangible expenditures enter directly into the denominator.

[3]This requires assuming constant returns to scale. See, e.g., De Loecker et al. (2020); Foster et al. (2008).

C Testing for Pre-Trends

The key identifying assumption in our main DiD empirical strategy is that, absent the policy, the treated and control firms would have followed parallel trends in intangible investment. One crucial concern is that the assignment of firms to treatment and control groups, which is based on a firm's trade credit position before the policy reform, is non-random and may be correlated to a firm's future liquidity and performance, violating the parallel trends assumption. To address this issue, we consider a formal test for pre-trends using an event study framework.

Specifically, we estimate the following regressions:

$$Y_{it} = \sum_{\tau} \beta_{\tau} \cdot \mathbf{1}\{t = \tau\} \cdot \mathrm{T}_i + \sum_{\tau} \gamma_{\tau} \cdot \mathbf{1}\{t = \tau\} \cdot \mathbf{X}_i + \alpha_i + \varepsilon_{it}, \tag{C.1}$$

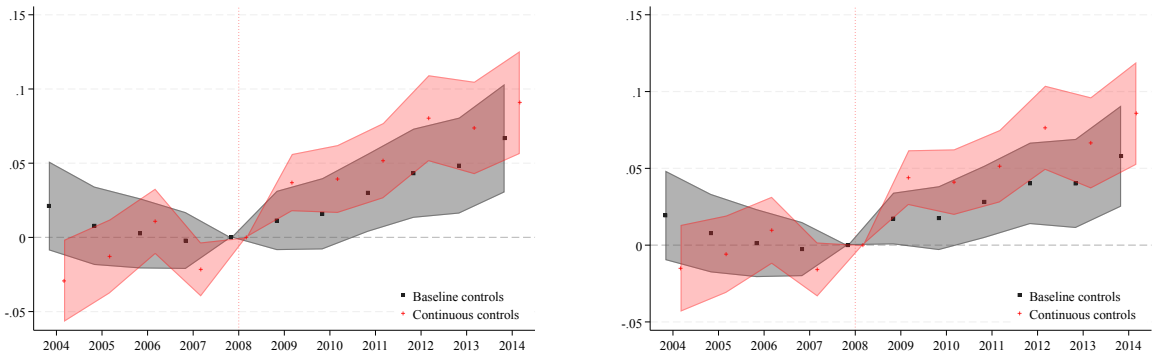
$$Y_{it} = \sum_{\tau} \beta_{\tau} \cdot \mathbf{1}\{t = \tau\} \cdot \mathrm{T}_i + \mathbf{X}_{it} + \alpha_i + \varepsilon_{it}. \tag{C.2}$$

Here, Y_{it} denotes the log of intangible capital for firm i in year t, T_i is a binary treatment indicator, and $\mathbf{1}\{t = \tau\}$ are year dummies. In equation (C.1), the vector $\mathbf{X}_i$ includes baseline firm characteristics (i.e., cash holdings and current liabilities) measured in 2004 and interacted with year dummies. The specification parallels our baseline DiD model in equation (6) but replaces the post-treatment indicator with a full set of year indicators, allowing us to trace dynamic treatment effects and assess the presence of pre-trends.

Equation (C.2) presents an alternative version in which we include time-varying controls $\mathbf{X}_{it}$ instead of baseline characteristics. This mirrors the DiD specification with time-varying covariates reported in the main analysis. In both cases, we estimate weighted and unweighted regressions.

The coefficients β_τ measure the differential evolution of the outcome variable between treated and control firms in each year relative to 2008, which we normalize to zero. If treated and control firms exhibited differential trends prior to the reform, we would expect significant deviations in the β_τ coefficients for years before 2008.

Figure C.1: Event Study: (log) Intangible Capital



(a) Weighted (b) Unweighted

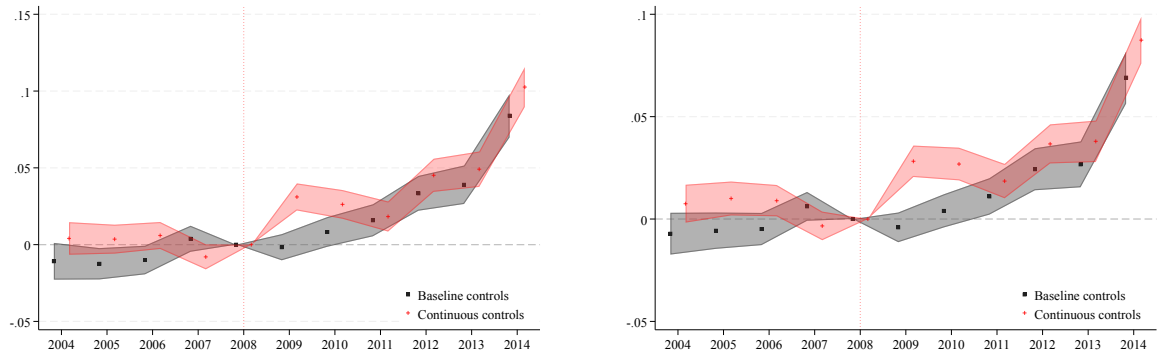
Notes: Each panel plots event study coefficients from DiD regressions estimating the effect of the trade credit reform on log intangible capital, along with 95% confidence interval bands. The baseline year (2008) is normalized to zero. Panel (a) reports weighted regressions; panel (b) reports unweighted regressions. Standard errors are clustered at the firm level.

Figure C.1 presents the estimated β_τ coefficients from the event study analysis, with $Y_{it} = \ln(\text{Intan}_{it})$, our baseline measure of intangible investment. The gray areas and markers are the estimates from the regression with time-invariant controls (equation (C.1)), while the red areas and markers correspond to estimates with time-varying controls (equation (C.2). Panel (a) reports results from weighted regressions, while Panel (b) presents unweighted ones.

Across specifications, the estimated β_τ coefficients in the pre-reform period are generally small and not statistically different from zero, providing no evidence of diverging pre-trends. While a few pre-2008 coefficients in the specification with time-varying controls (in red)

are marginally significant, their magnitude is small and they do not follow any systematic pattern.

Figure C.2: Event Study: (log) Operating Expenses, Demeaned



(a) Weighted (b) Unweighted

Notes: Each panel plots event study coefficients from DiD regressions estimating the effect of the trade credit reform on log demeaned operating expenses, along with 95% confidence interval bands. The baseline year (2008) is normalized to zero. Panel (a) reports weighted regressions; panel (b) reports unweighted regressions. Standard errors are clustered at the firm level.

Figure C.2 reports results using the log of operating expenses as an alternative measure of intangible investment. To account for sector-specific shocks, we redefine the outcome variable as the log of operating expenses demeaned at the four-digit industry-by-year level. This transformation removes common industry-year shocks and helps isolate firm-level deviations in intangible investment, improving the validity of parallel trends assumption. Since treatment is assigned at the firm level, this adjustment preserves the variation relevant for identification while improving precision. The estimated pre-reform coefficients remain statistically insignificant and display no systematic pattern, further supporting the parallel trends assumption. Accordingly, we use the demeaned version of operating expenses as our preferred alternative proxy for intangible investment in our main analyses.

Overall, the evidence suggests that treatment status does not merely reflect pre-existing differences in demand or productivity shocks, supporting the identifying assumption that, conditional on controls, treated and control firms followed parallel trends prior to 2009.

C.1 Robustness in Treatment Definition

Our baseline treatment definition sets $\mathrm{T}_i = 1$ for firms exposed to a positive liquidity shock in 2007, and $\mathrm{T}_i = 0$ otherwise. Two concerns may arise with this baseline approach. First, the truncation of net trade credit exposure at 0 implies that firms with DSO or DPO below the 60-days threshold are effectively treated as unaffected, even if they experienced meaningful shifts in trade credit conditions. This restriction limits variation in treatment and may affect our results. Second, measuring DSO and DPO in a single year (2007) could introduce measurement error. We chose 2007 to capture both persistent and idiosyncratic variation in trade credit exposure, which contributes to the randomness of treatment assignment. However, we acknowledge the potential sensitivity of the results to this choice.

We implement two related robustness checks.

Eliminating Truncation First, we redefine the liquidity shock without truncating the net difference between DSO and DPO at zero:

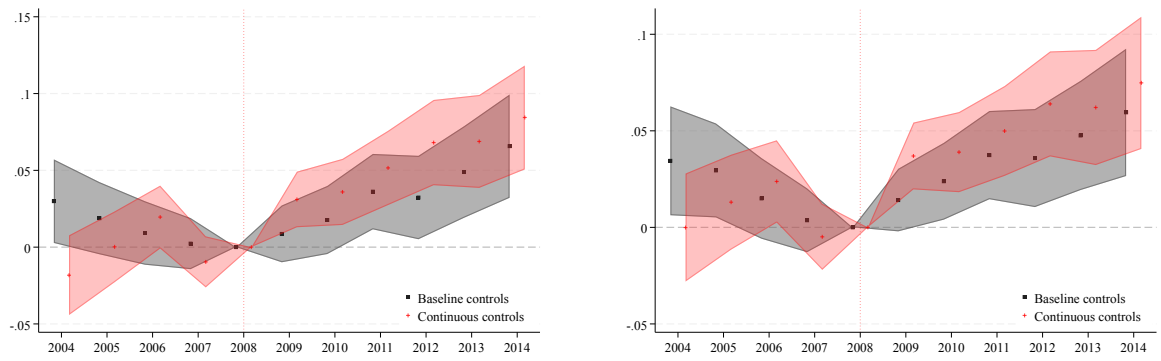
$$\text{Liquidity Shock}_i^{\text{pre}} = \max\left\{(\text{DSO}_i^{\text{pre}} - 60) - (\text{DPO}_i^{\text{pre}} - 60),\ 0\right\} \tag{C.3}$$

$$= \max\left\{\text{DSO}_i^{\text{pre}} - \text{DPO}_i^{\text{pre}},\ 0\right\}, \tag{C.4}$$

where pre = 2007. This alternative measure introduces greater variation in exposure by capturing both tightening and loosening of trade credit terms. It also eliminates sensitivity

to the 60-day threshold, which cancels out by construction. While the 60-day benchmark reflects the policy cap introduced by the reform, applying it rigidly may introduce measurement error in our context, as we proxy for credit exposure rather than observe contractual terms directly.

Figure C.3: Event Study: No Truncation



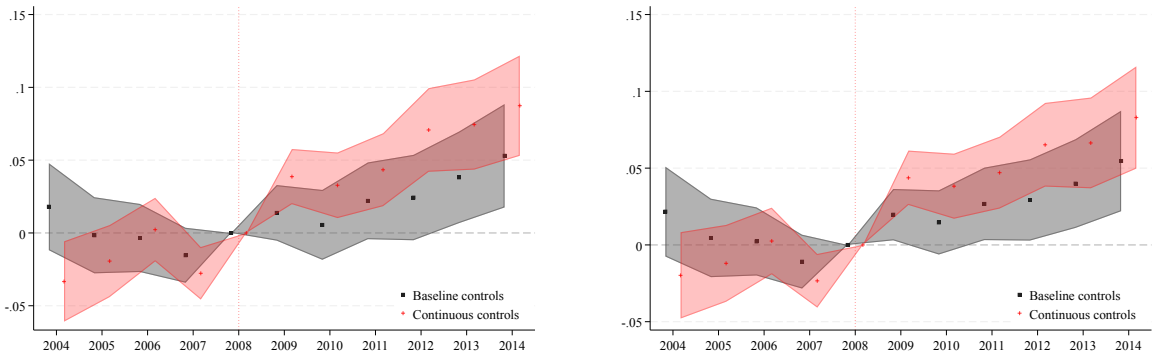
(a) Weighted (b) Unweighted

Notes: Each panel plots event study coefficients from DiD regressions estimating the effect of the trade credit reform on log intangible capital, along with 95% confidence interval bands. Liquidity shock is measured as in equation (3). The baseline year (2008) is normalized to zero. Panel (a) reports weighted regressions; panel (b) reports unweighted regressions. Standard errors are clustered at the firm level.

Figure C.3 displays the corresponding event study estimates. Panel (a) reports results using the baseline outcome (log intangible capital), while panel (b) uses the alternative outcome (log, demeaned operating expenses). We focus on weighted regressions; unweighted estimates yield nearly identical patterns. In both cases, the event study results are broadly consistent with our baseline results. Estimates based on operating expenses (panel b) are less precise, with larger standard errors and some significant coefficients in the pre-reform period under time-varying controls, although we still find no evidence of systematic trends. Overall, the patterns remain very much in line with our baseline results.

Multi-year average (2004–2007). To address concerns about measurement error from relying on a single year (2007), we compute the liquidity shock using the average DSO and DPO over the 2004–2007 period. The shock is defined as in equation (3), with pre $\in [2004, 2007]$.

Figure C.4: Event Study: Multi-Year Average



(a) Weighted (b) Unweighted

Notes: Each panel plots event study coefficients from DiD regressions estimating the effect of the trade credit reform on log intangible capital, along with 95% confidence interval bands. Liquidity shock is measured as in equation (**??**), but based on a multi-year average of pre-reform DSO and DPO. The baseline year (2008) is normalized to zero. Panel (a) reports weighted regressions; panel (b) reports unweighted regressions. Standard errors are clustered at the firm level.

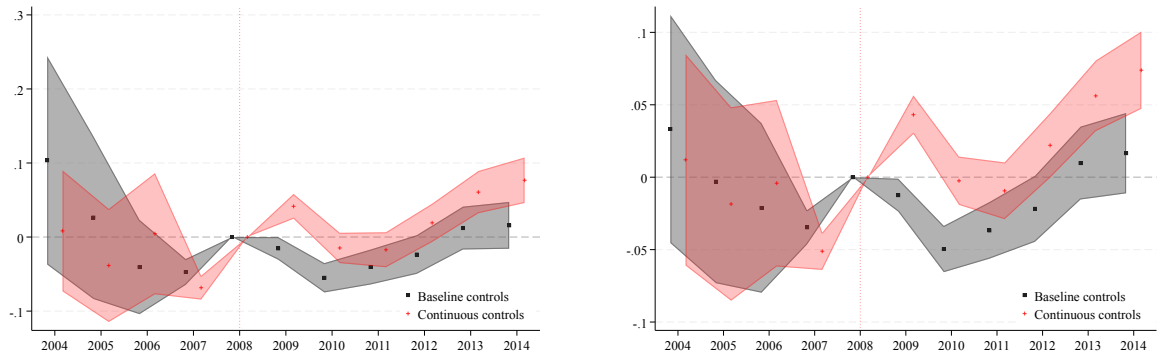
Figure C.4 shows the results. As in the previous robustness check, the event study patterns remain broadly consistent with our baseline and show no evidence of diverging pre-trends. Estimates are somewhat less precise than those from the baseline, and a few pre-reform coefficients are marginally significant. This is consistent with our interpretation that relying on 2007 values introduces more idiosyncratic variation and enhances quasi-random exposure. Overall, the results support the robustness of our identification strategy.

C.2 Tangible Capital

As an additional falsification test of our empirical strategy, and to further support the exclusion restriction, we replicate the event study using log tangible capital as the outcome variable. A central claim of the paper is that, unlike intangible assets, tangible investment is less sensitive to short-term liquidity shocks and should therefore exhibit little response to the 2009 reform.

Figure C.5 confirms this prediction: we find no evidence of differential trends in tangible investment between treated and control firms, either before or after the reform. This result reinforces our interpretation that the policy primarily affects intangible investment, highlighting the greater sensitivity of intangibles to trade credit conditions.

Figure C.5: Event Study: Log Tangible Capital



(a) Weighted

(b) Unweighted

Notes: Each panel plots event study coefficients from DiD regressions estimating the effect of the trade credit reform on log tangible capital, along with 95% confidence interval bands. Liquidity shock is measured as in equation (**??**). The baseline year (2008) is normalized to zero. Panel (a) reports weighted regressions; panel (b) reports unweighted regressions. Standard errors are clustered at the firm level.