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Liquidity as competitive advantage: The role of intangibles

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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.

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.

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

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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 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.

Literature review. 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

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

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 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 competitive advantage through larger market shares or markups.*

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

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. Sections 3.3 and 3.4 introduce our main estimating equations. Finally, Section 3.5 provides further detail on the measurement of the key variables.

3.1. Data

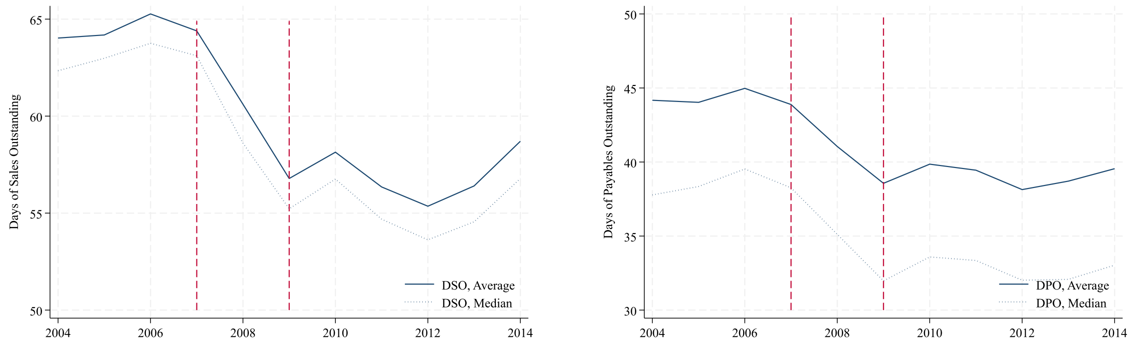
Our empirical analysis draws on a panel of French manufacturing firms from 2004 to 2014, based on Orbis data compiled by Bureau van Dijk.[4] This dataset provides detailed firm-level information on financial statements and production activities, allowing us to construct key variables such as intangible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

[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 et al. (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.



(a) DSO (b) DPO

Fig. 1. Evolution of DSO and DPO, 2004–2014.
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).

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:

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

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.

Fig. 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.

Fig. 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 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:

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

Here, $\text{DSO}_i^{\text{pre}}$ and $\text{DPO}_i^{\text{pre}}$ refer to the days sales outstanding and payable outstanding, respectively, in 2007, the year preceding the reform.[8]

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

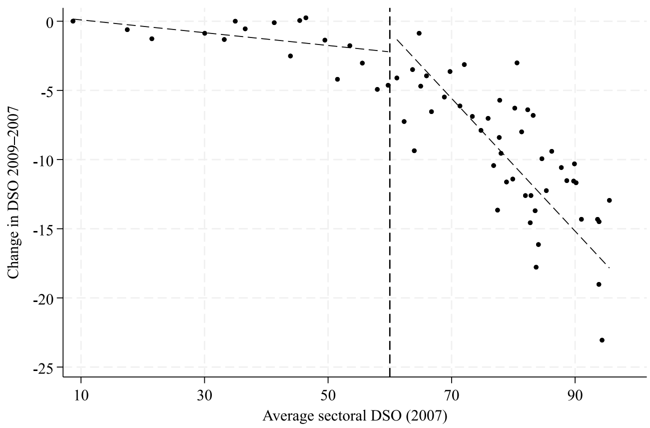


Fig. 2. Impact of the policy on payment days (2007–2009).
Notes: The figure plots the change in average firm-level DSO between 2007 and 2009 as a function of initial 2007 sectoral DSO. Firms are first grouped into percentiles of sectoral DSO in 2007, and each point plots the mean change within a percentile bin against its average initial DSO. The dashed lines report linear fits estimated separately for sectors below and above the 60-day policy cap.

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 Eqs. (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 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 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 Eq. (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 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.

[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".

[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).

Table 1
Summary statistics.

	Nr. Obs.:	Mean	St. Dev.	P25	Median	P75
Intangibles						
Intangible assets	388,492	49,826	95,600	0	8000	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.

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 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 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.

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

[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.

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

Table 2
Liquidity shocks and intangible investment.

Dependent variable:	$\ln(\text{Intan}_{it})$				$\ln(\text{Op Exp}_{it})$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{T}_{1,i} \times \text{Post}_t$	0.027**	0.066***			0.032***	0.037***		
	(0.011)	(0.010)			(0.004)	(0.004)		
$\text{T}_{2,i} \times \text{Post}_t$			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 Eq. (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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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, 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.1 summarizes the distribution of these benchmarks 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 Testable 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 Testable 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 Testable 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. 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 (Eq. (6)) estimated using weighted regressions; unweighted estimates are provided in Appendix Table A.2. 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 $T_{2,it}$ 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 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 Eq. (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:

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

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.3 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.4 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. Figure A.6 plots the coefficient of interest (β from Eq. (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. 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:

$$\ln Y_{it} = \alpha + \beta_1 \cdot \text{Post}_t \times Tj,i + \beta_2 \cdot \text{Post}_t \times Tj,i \times \psi_s + \text{Post}_t \times X_{it}'\gamma + c_i + \delta_t + \varepsilon_{it}, \tag{9}$$

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 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.

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

[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 Eq. (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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4
Markups, intangibles and liquidity.

Dependent variable:	$\ln\ \mu_{it}$			
	(1)	(2)	(3)	(4)
$\ln\ \text{Intan}_{it}$	0.183***		0.042**	
	(0.069)		(0.020)	
$\ln\ \text{Op Exp}_{it}$		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 ($\ln\ \text{Intan}_{it}$) and flow-based ($\ln\ \text{Op Exp}_{it}$) intangible proxies as regressors. Both are instrumented using the liquidity shock from the binary treatment variable, i.e., $\text{Post}_t \times T_{1,i}$. 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.3. From investment to competitive advantage

Table 4 reports estimates from the baseline IV specification in Eq. (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 $\text{Post}_t \times T_{1,i}$.

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.

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 Eq. (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. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.7 replicates the baseline IV specification from Eq. (7) using six alternative markup measures and two additional proxies for competitive advantage, with log intangible capital as the key explanatory variable. 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.2 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]

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.

[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, but it is consistent with the lack of post-reform trends in tangible capital shown in Appendix Figure C.2.

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.

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.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Carlo Altomonte reports financial support was provided by European Commission. If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jinteco.2025.104168.

Data availability

Replication Package for "Liquidity as Competitive Advantage: The Role of Intangibles" (Original data) (Mendeley Data)

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