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Funding liquidity without banks: evidence from a shock to the cost of very short-term debt

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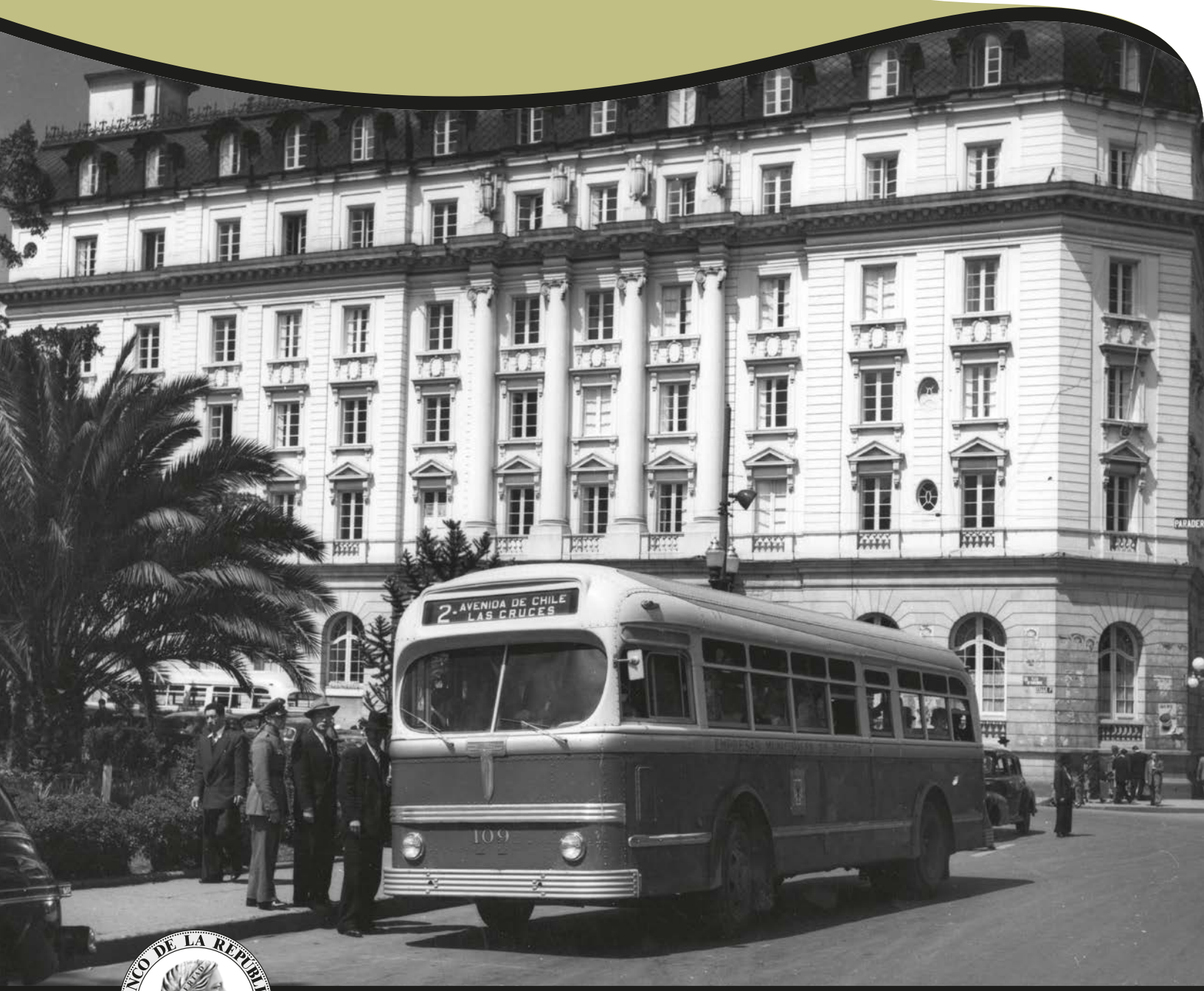

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PROVISIÓN DE LIQUIDEZ SIN BANCOS: EVIDENCIA DE UN CHOQUE A LOS CRÉDITOS DE MUY CORTO PLAZO

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RESUMEN

En 2011, Colombia incluyó los desembolsos a terceros dentro de la base gravable a los movimientos financieros (o 4x1000), incrementando así los costos de los créditos de muy corto plazo con relación a los de muy largo plazo. Las firmas respondieron con una disminución en los créditos de corto plazo para solventar problemas de liquidez y con un aumento en el uso del efectivo y las cuentas por pagar. En industrias en donde las cuentas por pagar son más comunes se encuentra una sustitución por las mismas con poco efecto en el efectivo o inversión. Caso contrario a lo observado en industrias con menos uso de las cuentas por pagar. Es así como las cuentas por pagar ofrecen una fuente de liquidez sustituta frente a choques a la provisión de liquidez de los bancos.

Clasificación JEL: H81, F38, D22, D25
Palabras Clave: crédito de corto plazo, cuentas por pagar, préstamos bancarios, liquidez, diferencias en diferencias.

FUNDING LIQUIDITY WITHOUT BANKS:

EVIDENCE FROM A SHOCK TO THE COST OF VERY SHORT-TERM DEBT[†]

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ABSTRACT

In 2011, Colombia instituted a tax on repayment of bank loans, thereby increasing the cost of short-term bank credit more than long-term credit. Firms responded by cutting their short-term loans for liquidity management purposes and increasing their use of cash and trade credit. In industries where trade credit is more accessible (based on U.S. *Compustat* firms), we find substitution into accounts payable and little effect on cash and investment. Where trade credit is less available, firms increase cash and cut investment. Thus, trade credit offers a substitute source of liquidity that can insulate some firms from bank liquidity shocks.

JEL Code: H81, F38, D22, D25
Keywords: short-term credit, trade Credit, bank loans, liquidity, difference in differences.

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I. INTRODUCTION

How do firms respond when bank liquidity dries up? In most economies, banks predominate as suppliers of liquidity and payments mechanisms between counter-parties. Demand deposits and credit lines (or, equivalently, very short-term bank loans) are fundamental payments products supplied almost uniquely by banks (Kasyhap, Rajan and Stein, 2002; Gatev and Strahan, 2009). Many firms use credit lines both to make payments and to smooth out liquidity needs over time, drawing funds when payments need to be made (i.e., making payroll or paying for supplies) and re-paying those funds when payments are received (i.e., after realizing sales receipts). Demand deposits and credit lines differ mainly in that the latter include a credit aspect (they are not pre-funded by the customer), whereas the former do not. Firms with limited access to lines of credit use deposits (out of their buffer stock of cash) to make payments, and they use trade credit with suppliers and customers to smooth payments over time.[1]

This paper offers a clean and well-identified setting to estimate how firms respond to an exogenous increase in the relative price of very short-term bank debt. We show that firms substitute into cash and trade credit as alternative sources of liquidity. In industries where trade credit is less available, the increase in cash is large; these firms cut both long-term and short-term investment. In contrast, in industries with better access to trade credit, firms increase accounts payable, but neither their cash balances nor their investment change after the shock. Together, these results have two implications. First, cash is a costly substitute for bank liquidity facilities.

[1] Banks also supply term loans as a source of credit to firms, usually structuring them with a short-maturity as a key contracting tool to help solve information and monitoring problems. Outside of real estate and other projects collateralizable by hard assets, most bank loans are short term, thus requiring borrowers to roll them over frequently. This discipline in turn provides banks the option to restrict or deny credit, thereby improving borrower *ex ante* incentives. Diamond (1991) offers the seminal theoretical treatment of debt maturity and argues that small and information-intensive firms are often only able to borrow short-term. Stohs and Mauer (1996) provide empirical support for this prediction.

Holding cash can be costly, both because cash yields low, after-tax returns and because cash exacerbates agency problems (Jensen (1986); Yun, (2009)). Moreover, when external funds are costlier than internal, firms needing to raise their stock of cash (because short-term bank credit has become expensive) must draw funds away from investment.[2] Second, for some industries trade credit offers liquidity services that can substitute for bank-provided liquidity without distorting real decisions (investment).

The empirical setting is Colombia, whose unique institutional features we exploit to identify how an increase in the cost of – or, equivalently, a decrease in the supply of – bank liquidity affects firms. First, in Colombia bank liquidity is more available for 'preferential clients,' all of whom have annual sales above a specific sales cutoff set by banks, than it is for smaller firms. These preferential clients have access to Treasury Facilities – very short-term extensions of credit from banks – and use these to manage their liquidity needs but not to finance real investment (due to their very short maturity).[3] Second, in 2011 Colombia initiated a new law that taxed *each* re-payment on any bank loan. The change made short-maturity Treasury Facilities (and other very short-term debt instruments) prohibitively expensive, but had little effect on the cost of long-term bank loans (due to the very low tax rate of 0.4%). We therefore focus our

[2] Sufi (2009) argues that many firms, in fact, must self-insure against liquidity shocks because their access to credit lines is limited by low cash flow, a key contractual control mechanism that banks use to monitor their borrowers. Credit lines expose banks to substantial credit risk as well as liquidity risk because firms may want to draw funds when cash flow is low due to poor fundamentals. Cash-flow based covenants help lenders alleviate this risk, but also imply that lines do not provide firms insurance against low cash flow in bad states. In fact, Lins et al (2010) survey CFOs from the largest firms across 29 countries and conclude that credit lines are a more important source of liquidity than cash among their sampled firms, but also that firms will simultaneously hold cash buffers to self-insure against bad states. Thus, credit lines are often used to allow firms to take advantage of future uncertain investment opportunities.

[3] Smaller firms also have access to bank credit lines for liquidity management, but these come with much more restricted access and tighter covenants. Small firms can also access short-term debt (say a 30-day loan), but supply is more constrained and conditions (rates, collateral, covenant) less favorable. Operationally, firms with sales below the sales threshold would bank out of a branch office; in contrast, firms with higher sales are assigned to a loan officer/account manager who gives this customer preferential treatment.

empirical analysis on large firms with access to Treasury Facilities, and construct a difference-in-differences identification strategy to exploit variation in this access, as heavy users of Treasury Facilities are most affected by the tax change. Hence, firms with access to Treasury Facilities (measured with pre-period use of short-term bank debt issuance with original maturity less than 60 days) constitute our treatment group; firms without operate as the control sample.

We report three core results. First, there is a strong 'first stage' relationship between the use of short-term bank credit and exposure to the tax shock. In other words, the use of short-term bank borrowing for liquidity falls sharply after the 2011 tax shock for firms exposed to the tax. Second, the decline leads to a substitution into both trade credit and cash. Both net accounts payable and cash increase after 2011, with the increases being larger for firms exposed to the tax shock. Third, the decline in bank-supplied liquidity leads to drops in both long-term and short-term investment. Capital expenditures and a proxy for investment in research and development (R&D) both decline for treated firms. In addition, short-term inventory investment in raw materials and unfinished goods also declines. Across industries, however, those with high access to trade credit substitute into accounts payable but experience no investment declines. Moreover, the substitution to trade credit seems complete, as there is *no* change in cash for firms in these sectors. Firms in industries with greater frictions in their ability to borrow from suppliers, however, experience no change in accounts payable but a large increase in cash and large declines in investment (both in long-term and short-term assets). Thus, in these segments the decline in bank liquidity spills over to real decisions. Declines in long-term investment, we surmise, occur because firms need to divert funds in order to increase their buffer stock of cash to manage liquidity. The increase displaces investment. Whether or not this decline is permanent is difficult to determine, given our short time-window of just four years following treatment. The effect on

inventories, however, is more direct because firms use short-term bank debt (along with trade credit) as a key source of funds for investments in working capital.[4]

As we describe in Section II, the tax regime in Colombia distorts the relative after-tax prices of tools firms use to manage liquidity – bank credit facilities, trade credit, and cash – in both the pre-2011 and post-2011 periods. Our paper thus compares two equilibria. In the first (pre-2011), many firms minimize taxes by using relatively tax-advantaged very short-term bank debt, rather than cash or trade credit, to smooth payments. In the second (after 2011), due to the tax change, the relative price of using very short-term bank debt rises. This increase leads to a sharp drop in the use of short-term bank debt, and in turn induces an increase in the use of liquidity substitutes: cash and trade credit. The increased demand for trade credit induces a higher price because domestic trade credit suppliers sometimes use bank credit to finance their accounts receivable (which is thus also indirectly affected by the tax shock).[5] Hence, only firms with good access to trade credit as a source of borrowed funds increase its use, while other firms instead increase their use of cash. Increasing the cash buffer leads to investment distortions, but increased use of trade credit (accounts payable) does not.[6]

In contrast to the extant literature, our shock focuses specifically on the liquidity role of banks. Most existing studies test how shocks that affect banks' cost of funds (e.g. monetary policy), or bank solvency (e.g. bank failures, as in Peek and Rosengren, 2000), or the supply to banks of local deposits (e.g. Becker, 2007; Gilje et al, 2016) propagate to non-financial firms. These kinds of shocks affect *both* the liquidity/payments role of banks *and* the credit-production role, thus

[4] For completeness, we also check for effects on investment in acquisitions but find no changes.

[5] That is, the supply of trade credit is upward sloping.

[6] We thank an anonymous referee for helping us frame our tax shock this way.

making it hard to unpack which aspect of banking (credit or liquidity) is affecting which outcomes for firms. We know of no existing study that can isolate a system-wide shock to the payments/liquidity role alone. As we explain in detail below, the tax shock in our setting changes the relative prices of short-term bank credit v. other payments mechanisms, but it has little effect on the cost of longer-term bank credit. Thus, it does little to the credit role of the banking sector. We find that trade credit can substitute for the liquidity-provision role of banks in some industries, consistent with Fisman and Love (2003), who show that poor financial development constrains growth less in industries that can substitute toward trade credit. Our setting differs because we have an exogenous shock to bank liquidity supply within a well-developed (bank-based) financial system.[7]

Our results suggest that investment can be distorted by shocks to bank-supplied liquidity. As we have argued, most of the literature considers cases where drawing a bright line between credit and liquidity is difficult. The 2007-2008 Financial Crisis illustrates on a grand scale how difficult such a separation can be, as solvency shocks metastasized when amplified by illiquidity, leading to a massive reduction in both bank credit and liquidity, followed by an economic disaster (e.g., Brunnermeier, 2009; Ivashina and Scharfstein, 2010). Much of the banking and macroeconomics literature has focused on the importance of banks as suppliers of both liquidity and credit to firms, but without a way to separate the two roles.[8] To take a specific example of how hard that separation can be, consider Merrouche and Nier (2010), who study a shock that

[7] Most existing studies of banks as liquidity suppliers focus on why banks combine deposits with credit lines (e.g. Kashyap, et al, 2002; Gatev and Strahan, 2006; Gatev, Schuerman and Strahan, 2009) and how lines are used, but do not explore substitution into trade credit, as we do. See also Jimenez, Lopez and Suarez, 2009, Ivashina and Scharfstein, 2010, Campello, et al, 2013. For a survey on the use of bank credit line by non-financial firms to manage liquidity, see Demiroglu and James (2011).

[8] In fact, Rajan (1998) describes how those two roles jointly evolved in medieval European banking, whereby money changers began using their 'float' from customers needing payments services to provide credit to other customers. This history illustrates the tight link between the liquidity and credit roles of banks.

improves bank *payments systems*. The paper finds that following payments innovations (in several Eastern European countries), funds flowed into banks, which in turn used those funds to expand *credit*. Our empirical setting is again different because it allows us to focus on the liquidity role alone, with little feedback to the credit role of banks.[9] We find that many firms can attain this liquidity via firm-to-firm trade credit without constraining their investment decisions.

We also contribute to the literature on trade credit by cleanly tracing out how firms respond to a well-identified increase in trade credit demand originating from a shock to their banks. Early studies, such as Petersen and Rajan (1997), rely on reduced form links from firm characteristics to accounts payable and receivable, making it hard to sort out demand v. supply side effects. More recent papers, in contrast, attempt to identify one side or the other. Amberg et al. (2016), for example, use the failure of Panaxia – a Swedish cash-in-transit firm - as a plausibly exogenous shock to liquidity demand for firms exposed to the failure. They find that this shock leads to increases in the use of trade credit and declines in firms' cash balances. Using shocks to payment terms in the French trucking industry, Barrot (2016) finds that firms substitute into cash when trade credit supply declines. Breza and Lieberman (2016), using a similar restriction on trade credit terms among Chilean retailers, also find a decline in trade credit accompanied by an increase in vertical integration.

A number of existing studies emphasize the relative substitutability of banks v. trade creditors. Both Wilner (2000) and Cunat (2007) argue that trade creditors have advantages over banks in enforcement and in renegotiation. Burkart and Ellingsen (2004) argue that trade creditors have an advantage over banks because they directly supply goods rather than cash, which unlike

[9] In fact, there is no trend break in aggregate deposits in the Colombian banking system after 2011, as we show in Figure 4.

goods is uniquely vulnerable to diversion (i.e. theft). But none of these studies trace out how shocks to banks affect trade credit, as we do. Similar to our study, Garcia-Appendini and Montoriol-Garriga (2013) find indirect evidence of substitution, in that firms with relatively high cash extend more trade credit and constrained firms use more trade credit during the financial crisis. The interpretation is complicated, however, because high-cash firms also were better able to sustain investment after the crisis (Duchin, Ozbas and Sensoy, 2010), and because the crisis affected both bank liquidity and credit supply as well as investment demand. Our study differs in that we examine a specific decline in bank-supplied liquidity from the tax shock, as opposed to a broad credit shock like the crisis, with limited spillovers to potential confounds such as changing investment opportunities.

In the next section, we describe the specifics of our institutional setting. Section III outlines the research design and identification, the data, and the results. We conclude briefly in Section IV.

II. COLOMBIAN BAD TAXES, THE 2011 CHANGE, AND TAX AVOIDANCE

For several reasons, Colombia offers an interesting setting to assess the importance of bank loans for firm liquidity management. First, the 2011 tax change that we describe below offers a clean shock to the relative cost of using bank credit for liquidity purposes. Second, Colombia is a bank-centric economy with limited alternatives for liquidity in other parts of the financial system, such as what the commercial paper market would offer to large, public firms in the U.S. Third, we have access to detailed financial statements data on a large, representative sample of firms in Colombia, both before and after the tax shock, which allows us to observe within-firm changes in cash management, financial policies and investment.

The 2011 tax shock emerged out of the bank account debit tax (BAD), adopted in Colombia in 1999 amid a weak economy, an unhealthy financial system, and low fiscal revenues.[10] Under the original legislation, all debits from bank accounts generate a tax liability. That is, BAD taxes affected withdrawals from bank accounts, including check clearances, electronic transfers, ATM cash withdrawals, loan payments (but, until 2011, firms could evade the tax on loan *re-payments*) and most other transactions that involve debiting money from a bank account.[11]

The tax rate started at 0.2% per transaction and increased to 0.3% in 2001. The rate increased again in 2004, to its current level of 0.4%. Over time, firms and households found ways to avoid the tax. For example, payments made outside the banking system do not pay the BAD tax. Figure 1 shows the general effect of the advent of tax arbitrage by plotting BAD tax receipts (relative to GDP) over time. As economic agents learn how to avoid the tax, collections gradually decline. In fact, Arbeláez et al. (2002) document a significant increase in the use of currency relative to bank deposits following the introduction of the tax, which is the easiest way to avoid it. They also show a decrease in the number of checks cleared after the tax, and checks would often circulate for many payments and with multiple endorsements before finally clearing (and thus being taxed). Restrepo (2016) shows that this dis-intermediation in the Colombian economy after 1999 reduced bank credit production, which in turn harmed financially dependent firms. Thus, as

[10] Other countries have implemented similar taxes on bank transactions (e.g. Australia, Argentina, Brazil, Ecuador, Hungary, Peru and Venezuela). In the U.S., a two-cent tax on bank checks was imposed during the Spanish-American war at the end of the nineteenth century, and then from June 1932 through December 1934. Lastrapes and Selgin (1997) document how the Hoover administration adopted the tax in the early 1930s when "faced with a dramatic collapse in income tax revenues."

[11] Prior to 2011 firms could use funds held in investment accounts (e.g. a money market fund) to dispense the loan principal payment to the bank without incurring the tax. The 2011 law change eliminated the ability to use this mechanism to avoid the tax on loan payments.

with most of the current banking literature, the original BAD tax affected *both* the liquidity and credit roles of banks.

Prior to 2011, large firms could – and did – use Treasury Facilities (essentially very short-term bank loans) to make payments without incurring the BAD tax. Payors would borrow funds and deposit those funds directly into the recipient's bank account, thereby avoiding a bank account withdrawal and thus any tax. When the loan was due, firms would dispense the loan payment using an investment account, also avoiding paying the tax on the repayment. The 2011 law stopped this practice.[12] As can be seen in Figure 1, the change worked, as BAD tax receipts jumped by about 1/3 after the law change (from 0.59% to 0.82% of GDP in 2010 and 2011 respectively) without the need to increase the statutory tax rate, which has remained at 0.4% since 2004.

The easiest way to understand the effect of the tax change is with an example. Consider a firm purchasing $10 million in supplies. Payment options include: 1) an immediate cash disbursement from funds withdrawn from the firm's checking account; 2) the use of trade credit (with, say, up to 30 days to pay); or, 3) the use of funds borrowed from a Treasury Facility. Under the tax regime prior to 2011, the firm would pay a tax of $40,000 (0.4% times $10 million) using either cash or trade credit because either mechanism would require a debit of $10 million from the firm's checking account. The difference between the two comes down to timing: with cash, payment would be immediate; with trade credit, a firm could make the payment any time over the subsequent 30 days. The second option – trade credit – would allow the firm to time its payment to coincide with its sales receipts, thus lowering the optimal balance in its checking account.[13] Note that the tax burden depends on the flow of payments through the checking account, not the

[12] Law 1430 of December 29th, 2010.

[13] Trade credit has costs as well, which we are leaving out for illustrative purposes.

level (or buffer stock) held in the account itself. The firm, however, could avoid the tax entirely before 2011 by borrowing the $10 million from a Treasury Facility and having the payment deposited directly into the supplier's bank. Firms with access to these facilities would choose them to avoid the transaction tax.

In 2011, the Colombian Government eliminated the third option to use funds borrowed from a bank, such as a Treasury Facility, to avoid the BAD tax. This change occurred both because banks could no longer put borrowed funds directly into a third-party's account, and because borrowers would incur the tax when those funds were later re-paid to the bank. Thus, after 2011 borrowing $10 million from the bank for a payment would incur *two* taxes because these funds first would go into the borrower's checking account (and hence incur the BAD tax when the supplier gets paid), and a second time when the loan comes due. Given the change, firms are now better off either paying suppliers immediately from cash (i.e. from their checking account), or using trade credit. Both would incur a one-time tax payment because the firm would have to debit the same $10 million from its account to pay the supplier eventually, thereby incurring the same $40,000 tax (ignoring the small effect of time-value of funds). However, trade credit would have the advantage of allowing the firm to time its payments to coincide with incoming sales receipts. Thus, we would expect that firm demand for trade credit would *increase* after 2011 to improve its ability to smooth payments and conserve on its holdings of a cash buffer stock (i.e. accounts payable would rise). Market clearing dictates that the increase in trade credit demand generates and equivalent increase in trade credit supplied, so accounts receivable also should increase.

As we will show, the tax law change in 2011 encouraged firms to switch away from bank loans for payments and to increase the use of trade credit (or, if not available, cash). However, as we have argued, the change had little effect on the cost of long-term credit from banks. The tax

change also had no effect on the other side of the banking system's balance sheet (hence there is no reason to expect the supply of longer-term loans to change); nor did it have a large effect on the cost of using bank debt to finance investment directly.

Figure 2 illustrates this latter point. We graph the increase in borrowing cost on an annualized basis as a function of a loan's maturity. Since borrowers must pay the 0.4% tax for every loan re-payment, it affects the 'all-in' cost of debt most for very short maturity loans. For instance, consider the annualized cost for an 8% (APR) loan, roughly the average rate for a bank loan in Colombia. At a maturity of five days, the all-in cost annualizes to 37.25% (= [(1 + 8.0% × 5/365) × (1 + 0.4%) – 1] × (365/5)); in contrast, the 'all-in' cost for a 1-year loan comes to just 8.4% (= [(1 + 8.0% × 365/365) * (1 + 0.4%) –1] × (365/365)). By the time maturity gets beyond 60 days, the effect of the tax is small (Figure 2). At maturity of one year, the tax falls to just 0.4%, a five percent increase in the average interest rate on a bank loan in Colombia; for longer loans the effect is even smaller. And, the tax clearly had a major impact on the use of bank liquidity. Aggregate issuance of bank loans for liquidity management to preferential customers – Treasury Facilities – falls by about 80% at exactly the time that the tax goes into effect (see Figure 3).

We have argued that the 2011 change is about liquidity, not credit. This is clear from Figure 4, where we graph Deposits/GDP and Credit/GDP for the Colombian banking system all the way back to 1990. The figure shows disintermediation in the years just following the introduction of the BAD tax (1999), and then recovery due to the advent of tax avoidance strategies mentioned earlier. What matters for us: there is no change in the trends of either deposits or credit right around the 2011 tax change. Both are increasing in line with the overall expansion of the economy. This matters because it supports our claim that the 2011 tax affected *liquidity* – that is,

very short-term loans and credit lines - but had no noticeable impact on long-term *credit* production.[14]

III. Research Design, Data and Results

Research Design & Identification

As explained above, we exploit two unique features of the Colombian financial system to construct our difference-in-differences models. First, we focus only on firms with sales above the threshold of COP 15,000 million in the year prior to the shock, as these firms have predominant access to Treasury Facilities. Smaller firms do sometimes use bank credit lines to make payments, but these are more limited in the amount committed and come with tighter contractual restrictions (e.g., covenants) compared to the Treasury Facilities.[15] Second, we measure exposure to the tax change using an indicator equal to one for firms in the top quintile of the distribution of average pre-2011 usage of very short-term bank credit (loans issued with original maturity under 60 days).[16] Figure 5 reports this distribution. Short-term debt usage is nearly zero for about 40% of the large firms; hence they are affected very little by the 2011 tax change. Among firms with some

[14] As Figure 4 shows, the 2008 Global Financial Crisis had a relatively small impact on Colombia's banking system. Regarding banking structure, in our sample period there were 10 distinct foreign banks in Colombia (e.g Banco Santander, Scotia Bank, Citibank, HSBC, BBVA), and 13 local banks. Measured by the amount of outstanding loans in the balance sheet, foreign banks had approximately a 20% market share in 2010. They operate for the most part as independent banks, but naturally they have ties to their corresponding parents, and as such may be more sensitive to external shocks.

[15] Different banks have used different sales thresholds to classify firms into 'small and medium enterprises' and 'corporate' clients. For the top five banks in Colombia: *Bancolombia*, *Banco de Bogota*, *Davivienda*, *Banco de Occidente* and *Banco Polular*, the threshold in 2010 ranged between COP 15,000 million for both *Bancolombia* and *Banco de Occidente*, to COP 20,000 for *Banco de Bogota* and *Davivienda*. Since firms above COP 15,000 could access the portfolio of 'corporate' financial products from at least one bank, we use the lower bound of the range as our threshold. Together the top five banks in Colombia account for over 60% of total assets in the banking system. The annual sales threshold of COP 15,000 million is approximately equivalent to $5 million.

[16] In an earlier version of the paper, we also included firms with sales below the cutoff and used variation in sales as a measure of heterogeneous exposure to the tax treatment. These results are similar to those reported here, which we prefer in the interest of clarity and simplicity of the research design.

very short-term loans, the amount increases sharply in the upper-most quintile. We thus use these firms as our treatment sample, inferring access to Treasury Facilities based on their high level of very short-term debt issuance.[17]

We compare the treated firms with two sets of control firms, generating two ways to build difference-in-differences regressions. In the first, we include all of the firms in the bottom two quintiles from Figure 5; these firms act as natural controls because they are roughly matched on sales size yet have nearly zero very short-term bank debt. In the second, we construct a matched sample based on a propensity score estimated from a Probit regression to explain treatment status as a function of pre-period firm characteristics. The Probit sample contains all of the treated firms and all of the control firms from the first approach (i.e., from all firms in the bottom two quintiles and the top quintile of Figure 5). For each treated observation, we select the nearest neighbor using the estimated propensity score, with one-to-one matching without replacement. This alternative control group, by construction, looks more similar along observables to firms in the control group prior to the tax change.[18]

With the two control samples, we estimate 'first-stage' equations to explain how the use of very short-term bank debt responds to the tax change, as follows:

$$STD_{i,t} = \alpha_i + \gamma_{j,t} + \beta Treatment_i \times Post\text{-}2011_t + Time\text{-}Varying\ Controls_{i.t} + \varepsilon_{i,t} \quad (1)$$

[17] The treatment group excludes a small number of firms (87) that are categorized in the top quintile of pre-period very short-term debt issuance but that in the year prior to the shock (2010) did not issue *any* very short-term debt. We are not able to observe exactly the use of Treasury Facilities in our firm-level data; nor are we able to observe flows into and out of credit lines. Hence, we use issuance of very short-term bank loans as a proxy for firms with preferential access to bank liquidity. We report our baseline results, which are similar but somewhat attenuated, constructing treatment status with a broader set of firms (those in the top two quintiles) in the Internet Appendix (Table IA.4).

[18] Appendix Table IA.2 reports the Probit model used to construct propensity scores (Panel A), as well as pairwise comparisons of the covariates used in the matching procedure, before and after matching (Panel B).

where $STD_{i,t}$ equals issuance of loans with original maturity under 60 days during the year, divided by total assets at the beginning of the year. Our panel includes four years before and four years after the tax change (2007-2014). The coefficient β represents the difference-in-differences estimator. As noted, $Treatment_i$ equals one for firms in the top quintile of the distribution of bank debt from Figure 5, and zero otherwise. The coefficient β measures the change in *STD* for treated firms relative to the control sample(s).

We include both firm (α_i) and industry-time ($\gamma_{j,t}$) fixed effects in all models. These capture unobserved, time-invariant heterogeneity across firms as well as industry-specific temporal shocks affecting all firms in the same industry. For instance, they absorb industry-specific business cycle effects, as well as any differential trends across industries. Since our cross-sectional measure of exposure to the tax is time-invariant, its direct effect is captured by the firm fixed effects. By using pre-2011 levels to define *Treatment*, we avoid a potential feedback from the tax change to firm outcomes. The industry-time effects absorb the direct effect of $Post_t$.

After establishing a causal effect of the tax change on the use of very short-term bank debt, we then test how other financial and operating variables respond to treatment, as follows:

$$Y_{i,t} = \alpha_i + \gamma_{j,t} + \beta Treatment_i \times Post\text{-}2011_t + Time\text{-}Varying\ Controls_{i.t} + \varepsilon_{i,t} \qquad (2)$$

where $Y_{i,t}$ represents the outcomes related to both other financial policies as well as all components of investment. Financial policies include the following: Bank Debt / Assets; (Bank Debt < 1 year) / Assets; (Bank Debt >= 1 year) / Assets; Cash / Assets; Accounts Payable / Assets; Accounts Receivable / Assets; Net Trade Credit / Assets = (Payables-Receivables) / Assets. For investment, we include Capital Expenditures$_t$ / Assets$_{t-1}$; Research & Development and other long-term investment / Assets$_{t-1}$; ΔFinished Goods Inventory / Assets$_{t-1}$ and ΔRaw Materials and Supplies /

$Assets_{t-1}$; and, (Purchase of Long-term Assets + ΔIntangible Assets) / $Assets_{t-1}$. For completeness, we report all dimensions of investment, including long-term investment in both tangible and intangible assets, short-term investment in inventories, as well as acquisitions of other real assets.[19]

Each difference-in-differences model has two specifications: one with just firm and industry-time fixed effects, and the other with the following time-varying control variables: 1) Asset size = Log of firm assets; 2) Equity = Log of shareholders' equity; 3) Age = Log of firm age; 4) Asset tangibility = Property, plant and equipment / Assets); 5) Asset turnover = Sales / Assets.[20] We measure each covariate as of the prior year to ensure that they are predetermined relative to the outcome. The industry-time effects are defined at the 3-digit ISIC level. This level of granularity generates 130 distinct industry classifications in our sample. To build standard errors, we cluster by firm.

Data

Our firm-level sample comes from financial statements data collected by Colombia's Corporations Superintendence (*Superintendencia de Sociedades*), a government agency in charge of overseeing privately held corporations.[21] We also obtain loan issuance for each firm from Colombia's central bank (*Banco de la República*). The sample includes annual firm-level data

[19] Under local GAAP (in effect during our sample period) R&D is capitalized, so that this form of investment is included in an account on the assets side of the balance sheet. It includes R&D, business startup costs, expenses incurred improving rented properties, and other long-term deferred charges. We observe this line item but not its individual components, and use its change as a proxy for investment in R&D and other long-term assets.

[20] Colombia instituted a tax on book-value of equity at different times during our sample (starting in 2007 at a tax rate of 1.2%, and in 2011 at a base tax rate of 1.2% that increased with the book-value of equity). Hence, we interact the log of book equity with an indicator equal to one for these years. In unreported results we try alternative, more complex specifications (e.g. interacting with the tax rate for various levels of book value of equity) and obtain similar results.

[21] Privately held companies represent the vast majority of firms in Colombia. In 2010, for instance, there were only 92 listed firms in Colombia's stock exchange (*Bolsa de Valores de Colombia*). In contrast, we have data for approximately two thousand private firms in that same year. Public firms are not part of our dataset. Financial statements data are publicly available in https://www.supersociedades.gov.co/.

between 2007 and 2014. Focusing on this period allows us to examine firm behavior for four years before (2007-2010) and four years after (2011-2014) the tax-law change. Our sample is composed of firms that had sales above COP 15 million the year before the shock. We drop utilities (ISIC codes 40 and 41), financial firms (ISIC codes 65 to 67), and firm-year observations with asset growth exceeding 100%, and we require firms to have at least one observation both before and after the shock. The final sample contains 13,886 firm-year observations corresponding to 2,006 distinct firms.[22]

Table 1 reports summary statistics. Panel A contains statistics for all of the data in our regression sample; Panel B divides the sample based on exposure to treatment, comparing means from the pre-treatment years. The top section of each panel contains firm characteristics (i.e., the time-varying control variables in Equations 1 & 2), the middle section contains financing outcomes, and the bottom contains investment outcomes. In Panel B, we denote cases where means differ significantly between the treated and control samples with '*', using standard t-tests (Appendix Table IA.1 further provides details on the sample composition by industry segments at the ISIC section-level).

Comparing treated v. the first (non-matched) control sample, we observe some statistically significant differences in control variables, such as asset size and asset turnover. In addition, capital expenditures is significantly higher for the control firms. After matching, however, these differences are reduced. When we compare the treated group with the propensity-score matched sample, none of the differences are statistically significant. Financial outcomes, in contrast, vary much more between treated and control firms, which is expected because we are dividing the

[22] The data are free from survivorship bias (records are not deleted). If a firm no longer meets the conditions set by law requiring submission of financial statements, its data time series ends but its historical data remain.

sample based on issuance of very short-term debt, which substitutes for both cash and for trade credit (recall section II). Hence, the levels of cash and accounts payable are both lower during the pre-period for the treated group compared to control, since treated firms with access to Treasury Facilities use them rather than cash and/or accounts payable to smooth payments. Long-term bank debt is also higher for the treated firms, reflecting their better access to credit in general. These differences in levels are expected, but it is worth re-emphasizing that our identification does not rely on comparison across these two sets of firms. Rather, we rely on the differential impact of the 2011 tax change across firms *within* firm.

Results

Result 1: The Effect of the 2011 Tax Change on very Short-Term Debt (STD)

In order to establish that the 2011 tax-law change had causal effects on both financial and real outcomes, we first establish parallel trends in the evolution of *STD*. We construct a time-series plot to capture how treatment status affects *STD* from year to year. The figure allows us to check for both parallel trends in the pre-period, and to illustrate the dynamics of the effects after the tax change. In particular, we first estimate the following regression:

$$STD_{i,t} = \alpha_i + \gamma_t + \beta_t Treatment_i \times Year_t + Time\text{-}Varying\ Controls_{i.t} + \varepsilon_{i,t} \qquad (3)$$

Figure 6 reports the plot of γ_t (which represents the average for control firms) and $\gamma_t + \beta_t$ (which represents the average for treated firms). We omit the $Year_{2010}$ and its interaction, so 2010 acts as the reference year. For these graphs, we include all firms in the bottom two quintiles for the control sample. Figure 6 shows a sharp decline in *STD* in 2011, and also that this drop persists over the subsequent three years (this is the sample equivalent of the aggregate effect shown in Figure 3).

Moreover, there is no evidence of any differential trend between the two groups prior to treatment, consistent with the identification requirements for difference-in-differences estimators.[23]

Table 2 reports the difference-in-differences regression that corresponds to Figure 6. We report four regressions, two specifications × two control samples: with and without time-varying controls variables (columns 1 & 2) and with and without matching (columns 4 & 5). In addition, we also include two more columns to test formally the parallel trends assumption (columns 3 and 6). These latter two specifications introduce interactions between the year indicators during the pre-period with *Treatment*. Consistent with the figure, *STD* declines sharply for treated firms after the tax law changes but not before. In all six models, the point estimate is large, both statistically and economically. For comparison, treated firms average 0.198 for *STD* in the pre-treatment period (Table 1B). Hence, their issuance of very short-term bank debt declines by about two-thirds (=0.122/0.198, based on the difference-in-differences coefficient in column 1). And, we find no evidence of any pre-period significance of the year indicators times *Treatment*, meaning that trends are parallel during the four years prior to the tax change (consistent with Figure 6).

Result 2: Financial Outcomes

Tables 3 and 4 link the tax shock to other firm financial policies. Table 3 reports levels of bank debt and cash, and Table 4 reports trade credit. We interpret these models as reduced form relationships in which the treatment effect reflects changes in firm financial policies induced by the decline in usage of *STD*. Table 3, Panel A reports results for overall bank debt and cash; Panel B then disaggregates the bank debt by maturity (over v. under one-year in residual maturity). Table

[23] We report evidence for the absence of pre-trends for the other financial and real outcomes as well in Appendix Figure IA.1.

4 reports net payables in Panel A, and gross payables and gross receivables in Panel B. We report each set of models with the same four specifications as in Table 2.

Four dimensions of financial policy change when firms cut *STD* for liquidity management. First, firms borrow less from banks overall. In the bank-debt regressions, the coefficients suggest that treated firms reduce debt by about 1.1% of assets, or about 4% of the pre-treatment average (Table 3A, columns 1-4).

Second, treated firms hold more cash after the tax change (columns 5-8). Cash rises by about 0.8% of assets, or about 20% of the pre-treatment mean. The change in the stock of cash occurs, we surmise, because firms are more likely to use direct cash disbursement to make payments after the tax change as a substitute for payments from Treasury Facilities or lines of credit (although we cannot directly measure these flows of payments). Greater direct payment flows requires firms to hold a high buffer stock of cash. Economic and statistical significance is similar for the two control samples.

Third, Panel B of Table 3 shows that the declines in bank debt are mitigated by partial substitution into longer-term debt. Short-term debt (maturity < 1-year) declines by about 2.3% of assets, while long-term debt increases by 1.0% to 1.6% of assets. Hence, the decline in overall bank borrowing is partially offset by an extension of maturity.[24] Firms substitute into long-term debt because it carries a lower tax burden, but the substitution is incomplete. These results support our identification assumption: the tax shock has a very large negative effect only at the short-end of the maturity distribution, which induces firms to lengthen debt maturity. The incomplete

[24] In Appendix Table IA.3, we show that issuance of loans with original maturities up to 180 days declines with treatment, while issuance of longer-maturity loans increases. As expected, the (negative) effect is greatest at the shortest end of the maturity spectrum (<30 days), and then increases monotonically thereafter.

substitution suggests that long-term and short-term bank debt serve different roles. Short-term bank loans exist to help firms manage payments and liquidity; long-term debt exists to help them finance long-term investment projects.

Fourth, the tax change has a substantial effect on the use of trade credit. Net accounts payable increases sharply for treated firms, by 1.9% to 2.6% of assets, depending on the specification (Table 4, Panel A). This change is large relative to the pre-treatment average level of net payables (-8.7% of assets). The net change is driven more by a decline in accounts receivable for treated firms, as the increase in accounts payables is not statistically significant. (As we show below, this baseline model masks a key difference in how trade credit responds to treatment as a function of access to trade credit.)

Result 3: Real Effects

Table 5 reports how investment is affected by changes in the tax cost of using bank liquidity. We report the same set of four models for each outcome. Panel A considers long-term investment: Capital Expenditure and investments in R&D and other long-term assets; Panel B considers short-term investment in raw materials and supplies in inventory, and in finished goods inventory; and, Panel C considers acquisitions.

We find significant declines in both long-term and short-term investment, although not across all dimensions. Capital expenditures decline by 0.5% to 0.7% of assets, or about 15% of the pre-treatment investment level. Investment in raw materials in inventory also declines significantly, by about 0.3% of assets. The other dimensions of investment do not change significantly.

Result 4: The role of Trade Credit

We have shown that net accounts payable rises sharply with the tax change, and we also find declines in both long-term and short-term investment. But access to trade credit may vary substantially across firms. Better access to accounts payable in particular ought to help firms minimize the need to increase their cash buffer, and it also ought to allow them to continue to finance inventories. Hence, we ask: do firms less able to substitute trade credit for bank credit experience larger real effects when the tax law changes? In answering this question, it is crucial to focus separately on accounts payable (which is the borrowing side of trade credit) with accounts receivable (the lending side) because frictions ought to constraint firms only on the borrowing side.

To see why frictions might matter, imagine a firm that can wait just 30 days to make its payments (without penalty), but waits 45 days to receive cash from customers after a sale. A firm like this needs a bridge to span the 15-day timing mismatch. The bridge could be built either by holding a cash buffer stock or by borrowing on a credit line or Treasury Facility. Clearly if the option to access bank credit over very short horizons like 15 days becomes more expensive, this firm would need to increase its cash, which in turn may displace investment, either because internal funds get diverted away from investment, or because holding a higher buffer of cash leads to permanently lower investment. Firms in the opposite situation, those with greater access to trade credit, would be less dependent on banks (or cash) for liquidity because of their ability to substitute into trade credit.

Given this intuition, we follow Murfin and Njoroge (2015) and construct a measure of trade credit access to firms that reflects the timing of payments flows in and out of the firm. Specifically, we build an indicator, *High TC Access*, equal to one for firms in industries in which the average days to make payments (= accounts payable / (cost of goods sold + change in inventory)) exceeds the average days to receive payments (= accounts receivable/sales). Firms that can wait longer to

make payments (relative to when they receive them) are in a better position to substitute trade credit for bank-supplied short-term loans.

Much of the theory of trade credit relies on the idea that suppliers have an informational advantage over other providers of liquidity (i.e., banks), or that suppliers are best able to redeploy collateral. Schwartz (1974) provides an early explanation of trade credit as a key component of a firm's pricing policy, in part motivated by differential access to capital markets. Smith (1987) provides a broad theory of inter-firm credit that includes product quality, market power and sunk costs. She proposes that delayed payment facilitates allow buyers to verify product quality before paying. Smith also models trade credit as a screening contract where terms can be designed to provide credit quality information, as the response to credit terms may help identify low creditworthiness buyers. Burkhart and Ellingsen (2004) argue that a key advantage for trade credit suppliers is their certainty that the transaction with the buyer has occurred. Overall, such explanations suggest that industry characteristics will determine the availability of trade credit. In fact, Ng, Smith and Smith (1999) use data compiled from a survey to credit managers for over 2,000 *Compustat* firms and show that there is significant variation in trade credit policies *across* industries, while there is little variation *within* industries.

Petersen and Rajan (1997) provide comprehensive empirical evidence correlating accounts payable and accounts receivable to firm characteristics using small US firms. They show that the use of accounts payable is more than 70% lower for firms whose inventories are finished goods, compared to firms with no finished goods in inventory. Fisman and Love (2004) report that they find similar correlations based on comparisons across countries and industries. In contrast, Giannetti et al. (2011) find more trade credit supplied by firms selling differentiated products. The difficulty with these reduced-form correlations, however, is that they reflect both supply and

demand-side factors. On the trade credit supply side, raw materials are easier to repossess and put into alternative uses. On the demand side, however, trade credit users may have a greater demand to delay payments on differentiated goods in order to certify their quality (Smith, 1987).

Our setting allows us to identify a shock that both raises trade credit demand and reduces its supply. This follows because, as we have shown, the tax on short-term bank credit induces treated firms to substitute toward alternative means of payment (i.e., net trade credit increases) but also increases the cost of firms to supply trade credit to other firms (gross accounts receivable declines). Together these changes imply that the equilibrium price of trade credit ought to be higher after the tax change. Figure 7 shows precisely this: in aggregate, firms exploit early-payment discounts less frequently after the tax change.[25]

The cross-sectional implication of these changes is that firms with better availability to borrow with trade credit ought to substitute relatively more into accounts payable in response to the negative tax shock, compared to firms with less availability. To test this idea, we measure trade credit availability in a setting that has not been distorted either by the BAD taxes or by the tax change on bank loans from 2011; that is, we construct our indictor based on the median *Compustat* firm in the United States by industry, defined at the 3-digit ISIC level.[26] Murfin and Njoroge (2015) document substantial heterogeneity in this measure by firm size, but our purpose is to avoid variation in access due to firm-specific characteristics and to capture the overall availability of trade credit based on exogenous industry characteristics such as the nature of inventories (raw materials v. finished goods) or technology differences.

[25] To construct this figure, we collect data on a separate, albeit much smaller, sample of 156 firms that issue public securities and are thus required to report additional information, including early payment discounts on sales.

[26] Similar to Rajan and Zingales (1998) and Fisman and Love (2003), we interpret the US data on trade credit as representative of the inherent use of trade credit by firms in a given industry. We use *Compustat* data from 1990 to 2006 to construct this measure.

We focus in this analysis on the reduced form regressions (i.e., Equation (2)), with an additional interaction term. These modified reduced forms, with trade credit interactions, are as follows:

$$Y_{i,t} = \alpha_i + \gamma_{j,t} + \beta_1 Treatment_i \times Post\text{-}2011_t \times \left[Hi\text{-}TC\ Access_j = 0\right] + \beta_2 Treatment_i \quad (4)$$
$$\times Post\text{-}2011_t \times \left[Hi\text{-}TC\ Access_j = 1\right] + Time\text{-}Varying\ Controls_{i.t} + \varepsilon_{i,t}$$

where $Y_{i,t}$ equal either the financial or real (investment) outcomes from above. *High TC Access*$_j$ equals an indicator set to one for a firm in 3-digit ISIC industry j in which the time to make payments exceeds that of the time to receive payments.[27] The coefficient β_1 measures the effect of exposure to the tax treatment for industries where trade credit leaves a financing gap (e.g., payments come in after 45 days but are due in just 30), whereas β_2 allows us to test how firms with better access respond to treatment.

Tables 6 and 7 report financial outcomes for the extended model. Bank debt declines with exposure to the tax shock, consistent with Table 3 (Table 6A, columns 1-4), but without any differential effect depending on a firm's access to trade credit. When we split bank debt by maturity, we find declines in short-term debt with similar magnitudes, irrespective of *Hi-TC Access* (Table 6B, columns 1-4). This supports our identification assumption, which is that the tax shock raises the relative price of very short-term bank debt for all treated firms similarly. But the induced effects on the *other* financial outcomes differ substantially based on access to trade credit. Firms without *Hi-TC Access* substitute into long-term debt, while those with *Hi-TC Access* do not (Table

[27] One could think of *High TC Access* as an 'instrument' for actual trade credit access for Colombian firms, using US *Compustat* firms. In fact, this 'instrument' is strongly correlated with the same variable constructed from Colombian data ($\rho = 0.58$).

6B, columns 5-8). Similarly, firms without *Hi-TC Access* substitute into cash, and again those with *Hi-TC Access* do not (Table 6A, columns 5-8).

Use of trade credit explains the differential effects of the tax on cash and long-term debt (Table 7). *Only* firms with *High TC Access* increase their use of accounts payable (both net and gross); for firms without *Hi-TC Access,* accounts receivable falls (recall, the cost of supplying trade credit has increased for everyone), but payables do not increase significantly (in fact, the point estimate is negative). For firms with *Hi-TC Access*, gross accounts payable rises substantially, by 1.7% to 2.4% of assets (Table 7B, columns 1-4). Thus, some firms increase their payables – that is, they borrow more from suppliers – when short-term bank credit becomes expensive; this allows them to manage liquidity without holding more cash. Others – those where trade credit frictions are substantial – instead must increase their buffer stock of cash (because it is more expensive to borrow from suppliers than before).

Table 8 suggests that the increase in cash for firms without *High TC Access* has negative real effects. Investment behavior differs sharply across industries. Firms without *High TC Access* cut long-term investment, while firms with *High TC Access* do not. The magnitude of the drop in Capital Expenditures rises to 1% to 1.4% of assets, or about twice the amount observed in the simple specifications. Investment in R&D also falls significantly only for firms without *High TC Access* (Table 8A). Investment rates fall, presumably, because raising the buffer stock of cash to replace short-term bank loans diverts internal funds away from investment (and external funds are costly).[28] In contrast, firms with *High TC Access* are able to increase their use of accounts payable, thereby limiting the need to raise cash and avoiding the investment distortions that would

[28] Increasing the cash buffer following the trade shock requires financing, which we presume displaces investment. Once the cash stock reaches a new higher level, however, investment ought to return to its old level. We are not able to test this effectively because not enough time has passed since the tax law change.

otherwise occur. Declines in short-term investments – raw materials in inventory – are somewhat larger for firms without *High TC Access*, but this difference is small and not statistically significant. As in the simpler models, we find no significant change in acquisitions for either type of firm following the shock.

IV. CONCLUSIONS

Bank credit lines and very short-term loans offer liquidity to firms and limit the need to hold cash. Our results suggest that some firms can avoid holding larger buffers of cash when short-term bank credit becomes more expensive by substituting into trade credit. Identifying this substitution has been challenging in the literature, but we can exploit a tax shock in Colombia that penalizes very short-term bank loans. We show that substitution away from banks and into trade credit is important, as it allows firms to avoid the real investment reductions that we find in industries where access to trade credit is constrained. Trade credit is thus an important alternate source of liquidity that can insulate firms from bank liquidity shocks. Unlike much of the existing literature, we can analyze a shock that affects very short-term bank credit used for liquidity without feeding back into the long-term credit role of banks.

Our paper also highlights a pervasive theme in the empirical literature, which is the fundamental tradeoff between tight identification vs. generality, sometimes called 'external validity'. We certainly recognize limitations from concern over external validity, as our tax shock happens in an unusual setting that has already adapted to a broad bank transaction tax. Yet the key finding – that trade credit can substitute for bank liquidity and thereby avoid real distortions – is consistent with other broader but less well-identified settings. For example, Garcia-Appendini and Montoriol-Garriga (2013) show that trade credit supplied by cash-rich firms increases around the 2008 Financial Crisis. They argue that the increase reflects substitution motivated by the declining

supply of liquidity from banks. Our study extends theirs because the tax shock in Colombia is very precise – it affects *only* very short-term bank loans, while leaving other potentially confounding variables like investment demand or changes in the supply of long-term bank loans unaffected (recall Figure 4). Our study thus helps mitigate identification concerns with their study, while their study helps mitigate external validity concerns with ours.

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Figure 1. BAD Tax Rate and Annual Tax Revenue Collected

This figure presents the evolution of the BAD tax rate (left y-axis) and the fiscal revenues from the BAD tax scaled by GPD (right y-axis). Fiscal revenues data are from Colombia's revenue agency (*Departamento de Impuestos y Aduanas Nacionales, DIAN*).

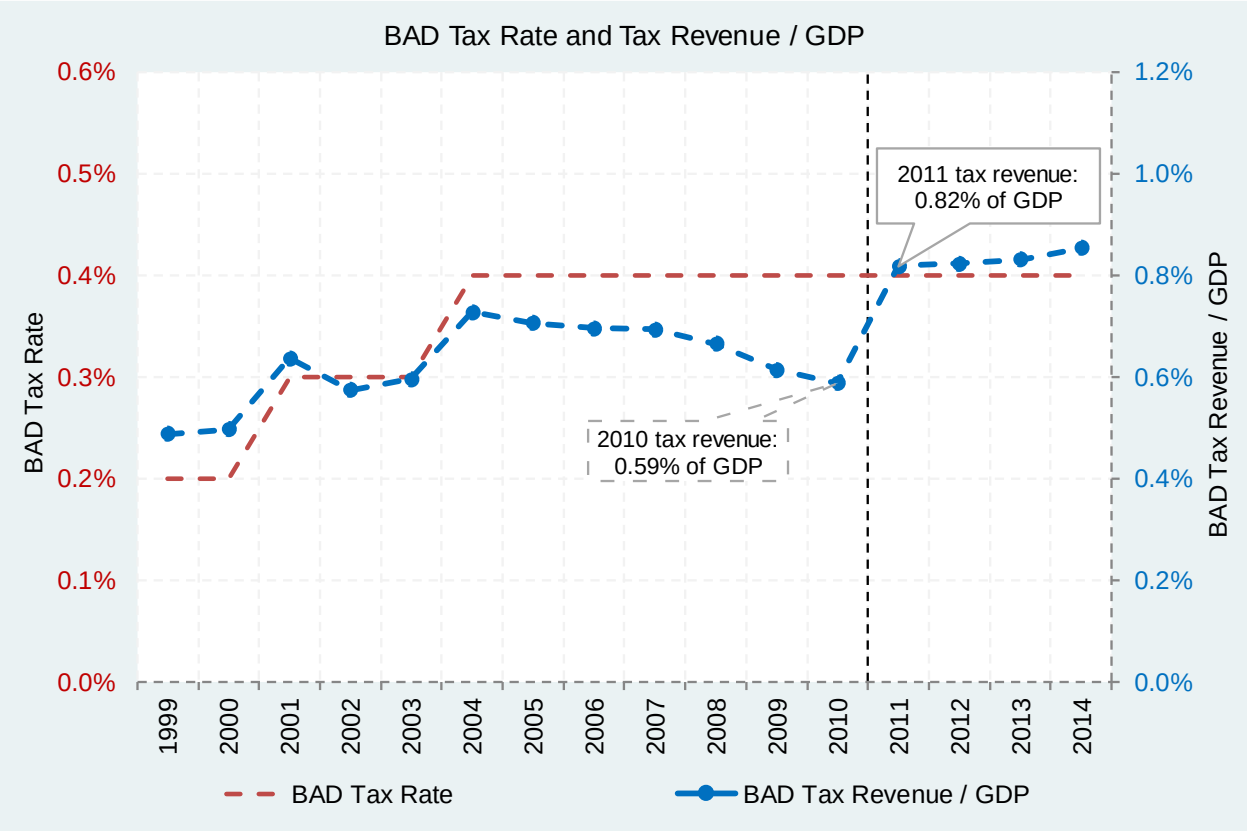


Figure 2. Effect of 2011 Tax Law Change on the 'All-In' Term Structure of Bank Debt

This figure illustrates the increase in the cost of debt on an annualized basis stemming from the 2011 tax law change, as a function of a loan's maturity. For simplicity, the red dashed line is set constant at 8.0%, reflecting a flat term structure of interest rates. The blue solid line depicts the 'all-in' cost of debt at after the cost of the tax is included in the loan's cash flow. The tax law change adds a cost of 0.4% to every loan, irrespective of its maturity; thus the 'all-in' annualized cost for shorter term loans increases significantly more than for longer term debt. For example, the 'all-in' cost for a 5-day loan is equal to: [(1+8.0%*5/365) * (1+0.4%)-1] * (365/5) = 37.2%, while the 'all-in' cost for a 1-year loan is equal to: [(1+8.0%*365/365) * (1+0.4%)-1] * (365/365) = 8.4%

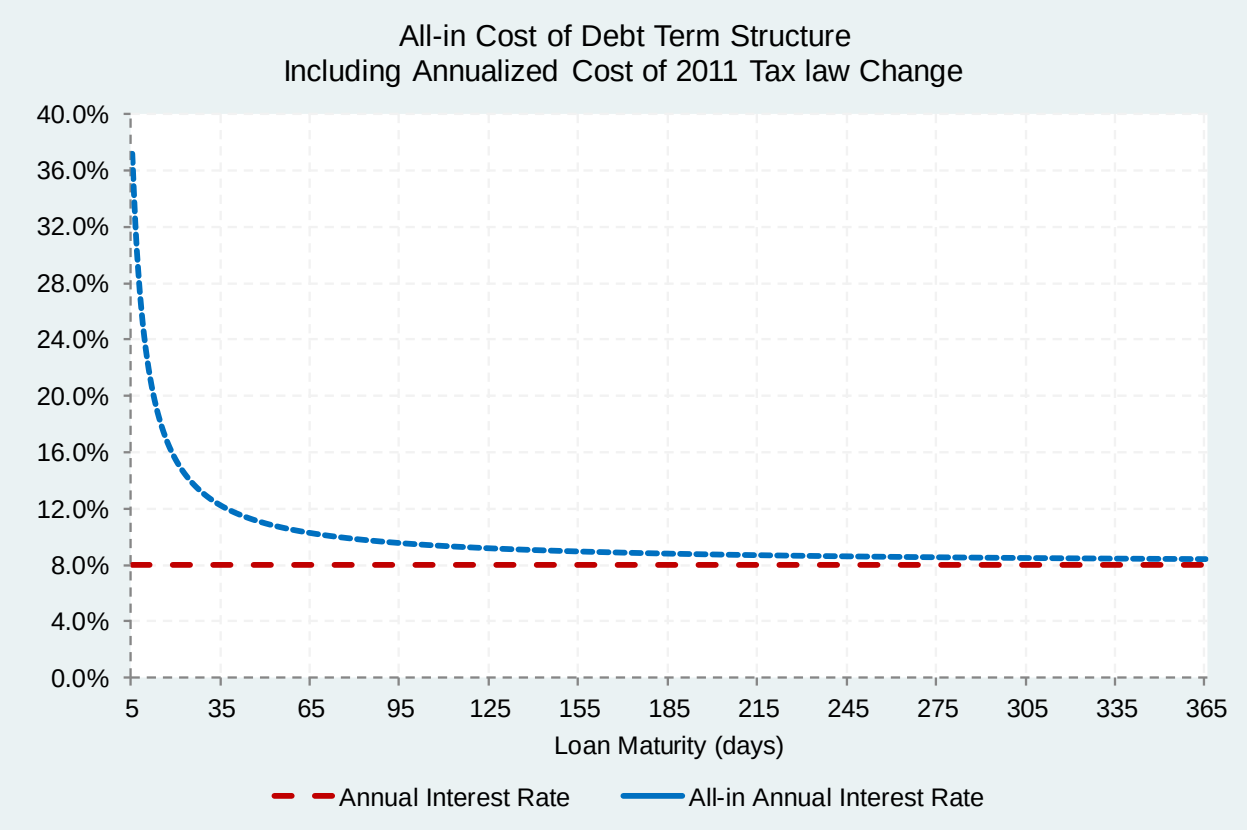


Figure 3. Aggregate Monthly Issuance of Treasury Facilities around 2011 Tax Law Change

This figure plots the aggregate monthly issuance of Treasury Facilities around the 2011 tax law change. All numbers are in COP millions. Aggregate data are from Colombia's central bank (*Banco de la República)* and include all Treasury Facility loans issued in Colombia.

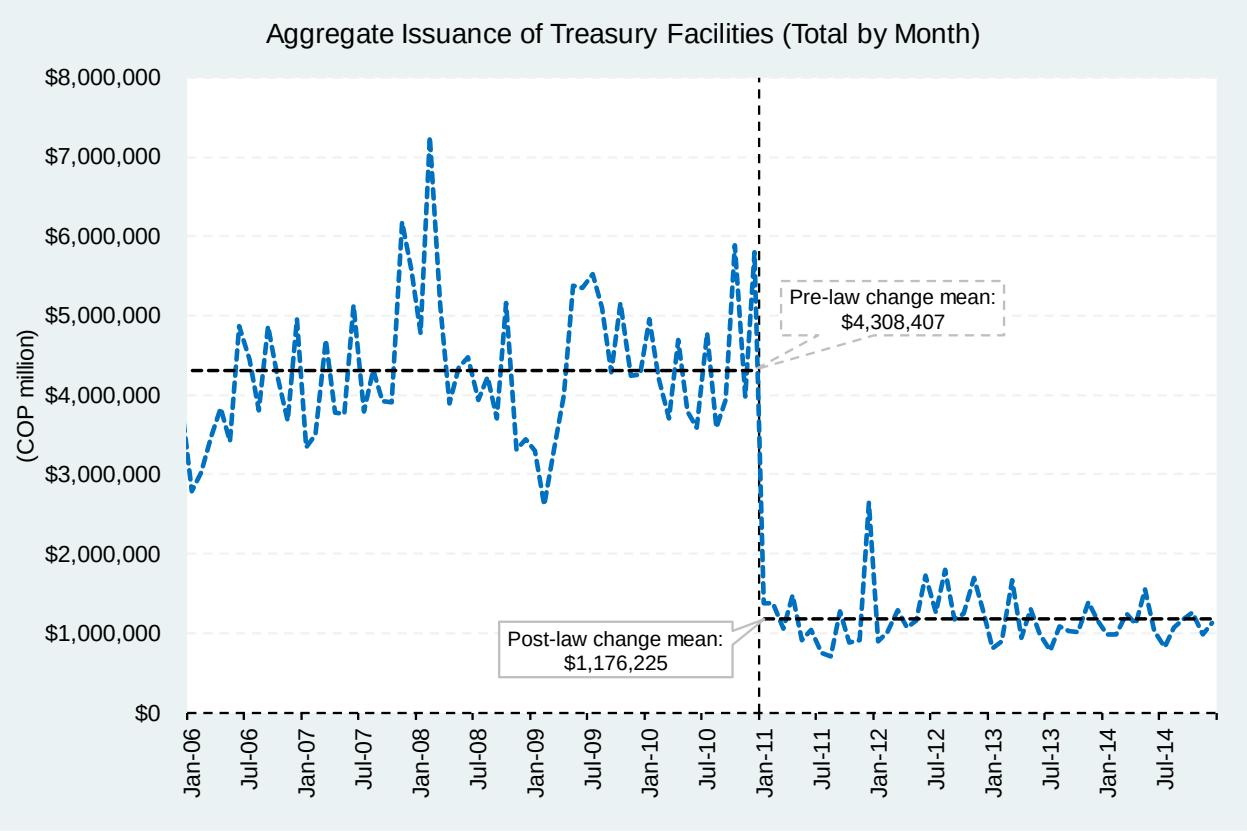


Figure 4. Credit and Deposits over Time

This figure plots total credit and total deposits in Colombia as a percentage of GDP from 1990 to 2015. Total deposits include checking, savings and time deposits. Credit is total credit provided by financial institutions. Data are from Colombia's central bank (*Banco de la República)*

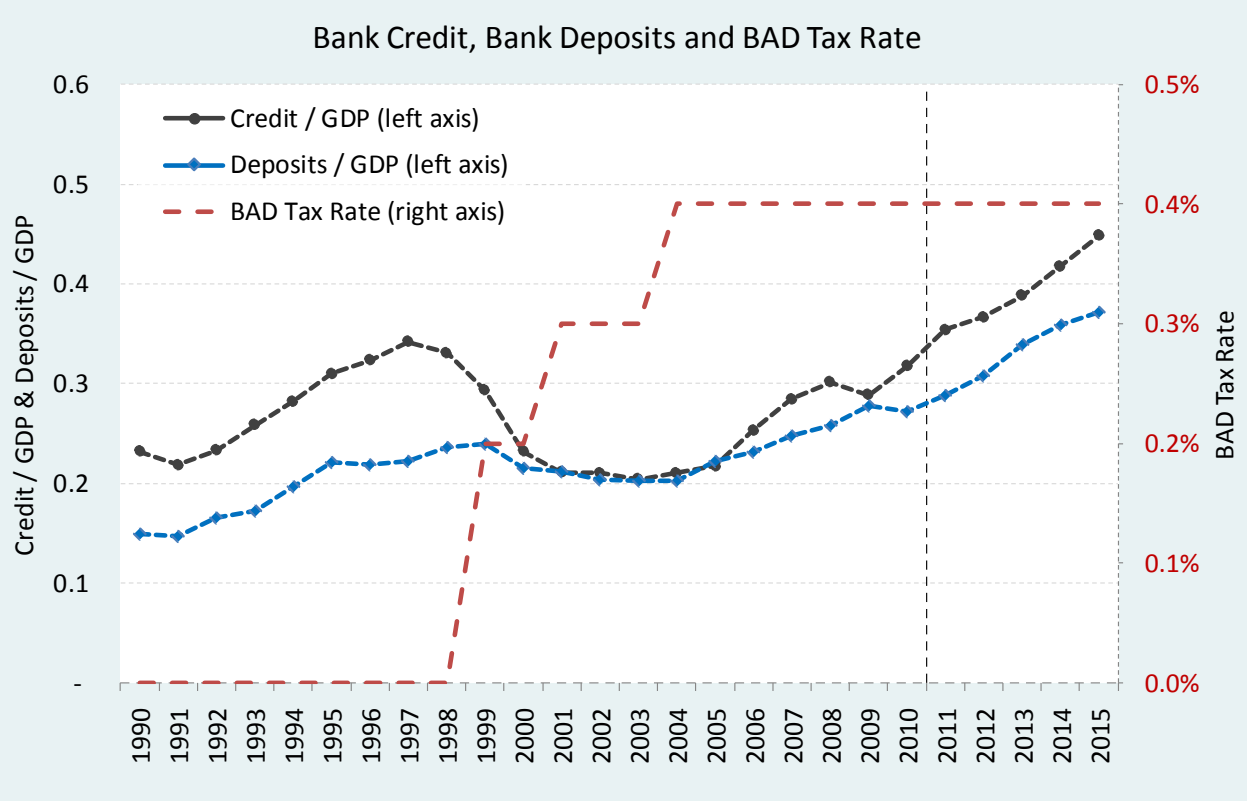


Figure 5. Quintiles of Issuance of Short-Term Debt (0 to 60 days) in the Pre-2011 Period

This figure presents the average issuance of short-term debt (0 to 60 days) scaled by beginning of the year assets for the five quintiles constructed based on the total issuance of short term debt over total beginning of the year assets in the pre-shock period (2007 – 2010).

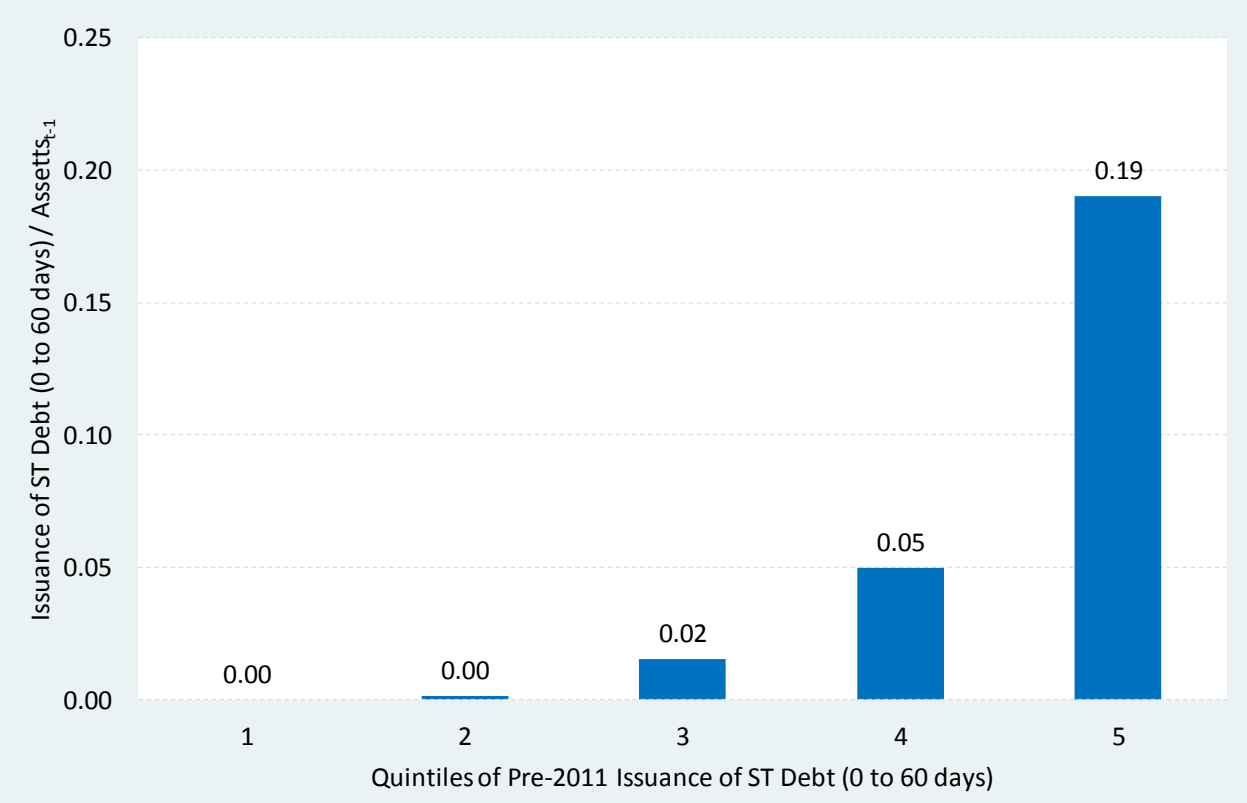


Figure 6. Trends around shock for Treatment and Control Firms: Issuance of Short Term Debt (0 to 60 days)

This figure plots the time coefficients from a regression of the issuance of short-term debt (0 to 60 days) on year indicator variables for both treatment and control firms (pre-match). The dependent variable is scaled by beginning of the year assets. The regression estimated is Equation 3, which also includes firm fixed effects and time-varying control variables. We omit the $Year_{2010}$ and its interaction with *Treatment*, (i.e. 2010 acts as the reference year).

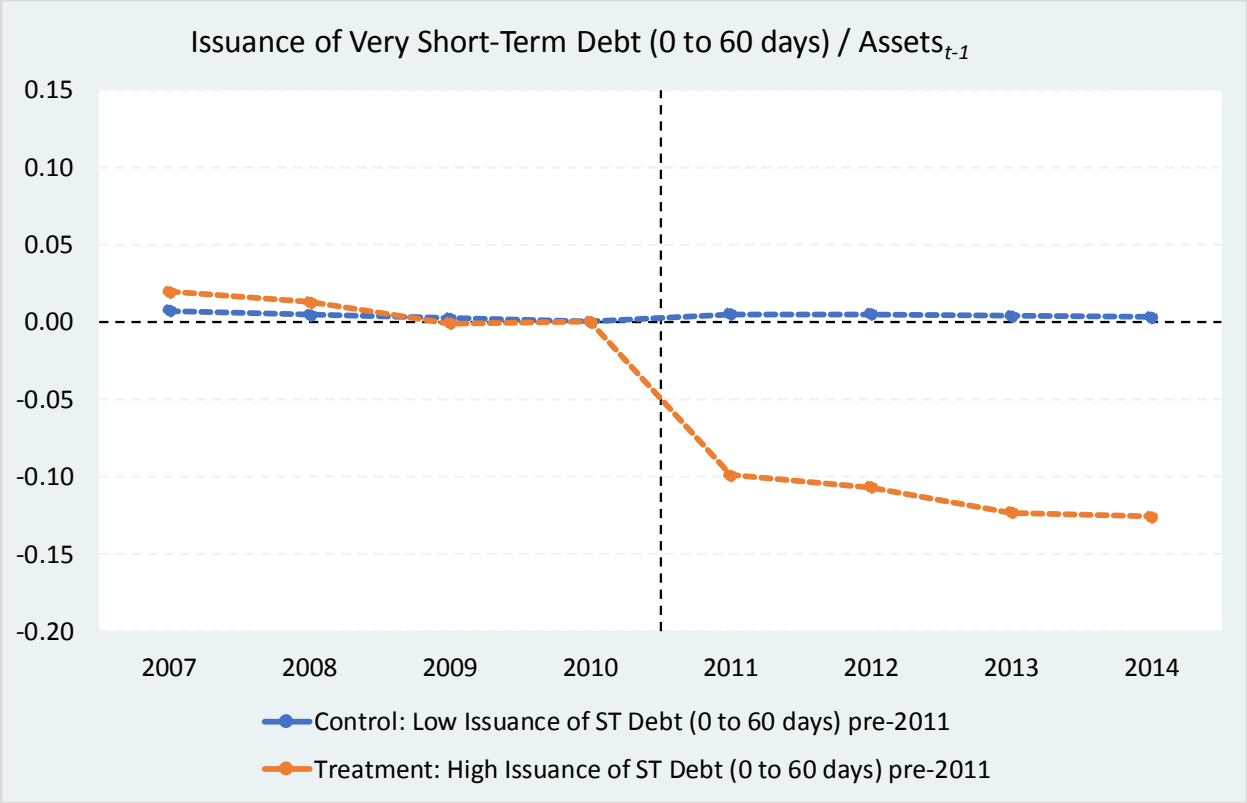


Figure 7. Early Payment Discounts on Sales / Sales around 2011 Tax Law Change

This figure plots sales discounts over total sales between 2005 and 2014 for a separate sample of 156 Colombian firms that issue public securities and are thus required to report additional financial statements items not reported by our larger sample of private firms, including the COP amount of early payment discounts provided on sales. The graph plots the average sales discount over total sales per year (we account for differences across firms by estimating the average per year in a regression that includes firm fixed effects).

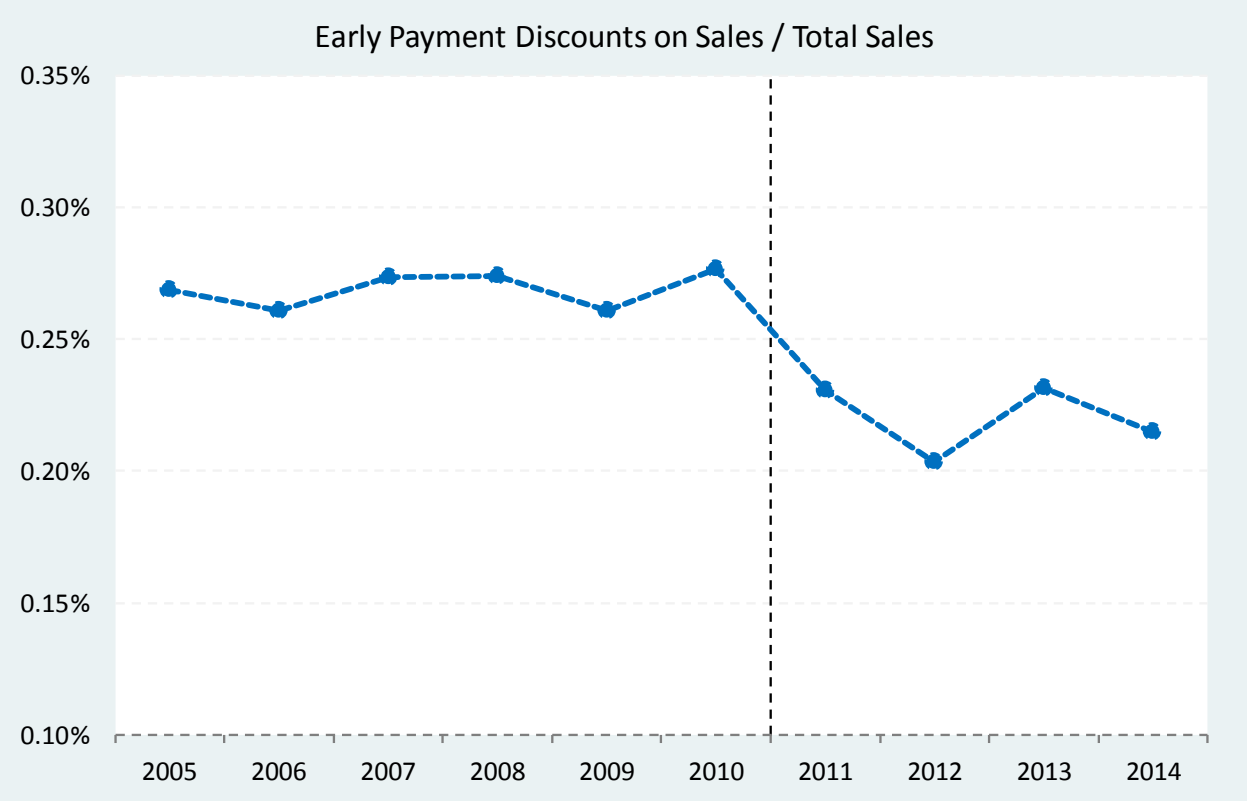


Table 1. Summary Statistics

Panel A reports summary statistics for the full sample period (2007 – 2014). Panel B reports pre-shock (2007 – 2010) averages for treatment and control firms (pre-match and post-match samples). The top section of each panel contains firm characteristics (i.e., the time-varying control variables in Equations 1 – 3), the middle section contains financing outcomes, and the bottom contains investment outcomes. Flow variables are scaled by beginning of year assets and winsorized at the 1% level. In Panel B, we denote cases where means differ between the treated and control samples with '*', using standard t-tests. Data are from Colombia's Corporations Superintendence (*Superintendencia de Sociedades*).

Panel A. Sample

	Mean	Median	Std. Deviation
Control Variables			
Ln(Assets)	17.2	17.0	1.3
Ln(Shareholders' Equity)	16.3	16.2	1.5
Firm Age	24.3	21.0	15.4
Asset Tangibility	15.7%	11.1%	15.1%
Asset Turnover	2.01	1.50	1.66
Asset Growth	12.5%	9.2%	23.1%
Financial Variables			
ST Debt Issuance (0 to 60 days) / $Assets_{t-1}$	4.4%	0.0%	10.5%
Bank Debt / Assets	17.2%	11.6%	17.9%
ST Bank Debt (1 year or less) / Assets	11.5%	5.2%	14.9%
LT Bank Debt (more than 1 year) / Assets	5.6%	0.0%	10.5%
Cash / Assets	6.7%	3.5%	8.9%
Accounts Payable / Assets	21.5%	17.1%	16.7%
Accounts Receivable / Assets	23.4%	20.4%	18.2%
Net Accounts Payable / Assets	-1.9%	-1.5%	21.0%
Investment			
Capex / $Assets_{t-1}$	4.4%	1.5%	7.6%
Change in Raw Inventories / $Assets_{t-1}$	0.3%	0.0%	4.9%
Change in Finished Inventories / $Assets_{t-1}$	0.1%	0.0%	3.5%
Change in R&D & Other LT Inv. / $Assets_{t-1}$	0.2%	0.0%	3.2%
Cash Flow used for Acquisitions / $Assets_{t-1}$	1.2%	0.0%	4.1%
Number of Firm-Years		13,886	
Number of Distinct Firms		2,006	

Panel B. Pre-2011 Sample Means for Control and Treatment Firms

	Control Firms: Low Issuance of ST Debt (0 to 60 days) pre-2011		Treatment Firms: High Issuance of ST Debt (0 to 60 days) pre-2011
	Pre-Match Sample	Post-Match Sample	
Control Variables			
Ln(Assets)	17.0**	17.1	17.1
Ln(Shareholders' Equity)	16.1	16.1	16.0
Firm Age	22.8*	25.3	24.1
Asset Tangibility	17.0%***	15.0%	14.3%
Asset Turnover	1.95***	2.38	2.55
Asset Growth	13.8%	14.9%	14.4%
Financial Variables			
ST Debt Issuance (0 to 60 days) / $Assets_{t-1}$	0.08%***	0.08%***	19.8%
Bank Debt / Assets	11.1%***	11.6%***	30.3%
ST Bank Debt (1 year or less) / Assets	6.6%***	6.8%***	23.8%
LT Bank Debt (more than 1 year) / Assets	4.5%***	4.8%***	6.5%
Cash / Assets	7.9%***	8.5%***	4.2%
Accounts Payable / Assets	22.6%***	25.5%***	20.8%
Accounts Receivable / Assets	22.0%***	22.1%***	29.5%
Net Accounts Payable / Assets	0.55%***	3.45%***	-8.7%
Investment			
Capex / $Assets_{t-1}$	5.2%*	5.0%	4.7%
Change in Raw Inventories / $Assets_{t-1}$	0.29%	0.32%	0.38%
Change in Finished Inventories / $Assets_{t-1}$	0.12%	0.13%	0.18%
Change in R&D & Other LT Inv. / $Assets_{t-1}$	0.14%	0.17%	0.16%
Cash Flow used for Acquisitions / $Assets_{t-1}$	1.1%*	1.1%	1.4%
Number of Firm-Years in Pre-2011 Period	4,804	1,950	1,950
Number of Distinct Firms	1,409	531	531

***, **, and * indicates a significant difference from the mean of the treatment sample, at the 1%, 5% and 10% level, respectively.

Table 2. Effect of 2011 Tax Law Change on the Issuance of Short Term Debt (0 to 60 days)

This table estimates difference-in-differences regressions for the issuance of short term debt (0 to 60 days) scaled by beginning of the year assets. The main coefficient of interest is on the interaction term *Treatment* × *Post 2011*. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control sample. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). We report four base regressions: with and without time-varying control variables for both the pre-match sample (columns 1& 2) and the post-match sample (columns 4 & 5). In addition, we include columns 3 and 6 to test the parallel trends assumption. These latter two specifications introduce interactions between the year indicators during the pre-period with *Treatment*. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

	Issuance of ST Debt (0 to 60 days) / $Assets_{t-1}$					
	Pre-Match Sample			Post-Match Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Post 2011	-	-	-	-	-	-
Treatment	-	-	-	-	-	-
Treatment × Post 2011	-0.122***	-0.121***	-0.125***	-0.119***	-0.118***	-0.123***
	(0.005)	(0.005)	(0.009)	(0.006)	(0.006)	(0.010)
$Ln(Firm\ Age)_t$		0.014*	0.014*		0.029*	0.029*
		(0.007)	(0.007)		(0.016)	(0.016)
$Ln(Assets)_{t-1}$		0.015***	0.015***		0.025***	0.025***
		(0.004)	(0.004)		(0.006)	(0.006)
$Asset\ tangibility_{t-1}$		0.012	0.012		0.006	0.006
		(0.012)	(0.012)		(0.025)	(0.025)
$Asset\ turnover_{t-1}$		0.008***	0.008***		0.012***	0.012***
		(0.002)	(0.002)		(0.003)	(0.003)
$Ln(BV\ Equity)_{t-1}$		-0.007**	-0.007**		-0.014***	-0.014***
		(0.003)	(0.003)		(0.005)	(0.005)
$Ln(BV\ Equity)_{t-1}$ × Dummy Equity Tax		0.001	0.001		0.001	0.001
		(0.001)	(0.001)		(0.002)	(0.002)
Treatment × Year = 2008			-0.002			-0.004
			(0.008)			(0.009)
Treatment × Year = 2009			-0.011			-0.014
			(0.010)			(0.011)
Treatment × Year = 2010			-0.003			0.000
			(0.010)			(0.011)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,886	13,713	13,713	7,645	7,613	7,613
r^2 (within FE)	0.136	0.140	0.141	0.097	0.106	0.106

Table 3. Effect of Shock to Very Short-Term Debt on Leverage and Cash Holdings

This table estimates difference-in-differences regressions for bank debt and cash holdings. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). We report four regressions for each outcome: with and without time-varying control variables for both the pre-match sample and the post-match sample. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Leverage and Cash Holdings

	Bank Debt / Assets				Cash Holdings / Assets			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011	-0.011*	-0.013**	-0.009	-0.010	0.007**	0.007***	0.008**	0.009**
	(0.006)	(0.006)	(0.007)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	13,886	13,713	7,645	7,613	13,886	13,713	7,645	7,613
r^2 (within FE)	0.001	0.055	0.001	0.047	0.001	0.019	0.002	0.015

Panel B. Short-term and Long-Term Leverage

	Short-Term Bank Debt (< 1 year) / Assets				Long-Term Bank Debt (>= 1 year) / Assets			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011	-0.022***	-0.023***	-0.024***	-0.025***	0.011***	0.010***	0.016***	0.015***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.004)	(0.005)	(0.005)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	13,886	13,713	7,645	7,613	13,886	13,713	7,645	7,613
r^2 (within FE)	0.004	0.018	0.005	0.018	0.002	0.031	0.003	0.030

Table 4. Effect of Shock to Very Short-Term Debt on Trade Credit

This table estimates difference-in-differences regressions for net accounts payable (= accounts payable - accounts receivable), accounts payable and accounts receivable. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). We report four regressions for each outcome: with and without time-varying control variables for both the pre-match sample and the post-match sample. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Net Accounts Payable

	Net Accounts Payable / Assets			
	Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)
Treatment × Post 2011	0.022***	0.019***	0.026***	0.022***
	(0.007)	(0.007)	(0.009)	(0.009)
Industry × Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
Observations	13,886	13,713	7,645	7,613
r^2 (within FE)	0.002	0.015	0.003	0.015

Panel B. Accounts Payable and Accounts Receivable

	Accounts Payable / Assets				Accounts Receivable / Assets			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011	0.007	0.004	0.007	0.004	-0.015***	-0.015***	-0.019***	-0.019***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)	(0.006)	(0.006)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	13,886	13,713	7,645	7,613	13,886	13,713	7,645	7,613
r^2 (within FE)	0.0003	0.034	0.000	0.039	0.002	0.018	0.004	0.023

Table 5. Effect of Shock to Very Short-Term Debt on Investment, R&D and Inventories

This table estimates difference-in-differences regressions for capital expenditures, investments in R&D and other long-term assets, changes in inventories, and cash flow used in acquisitions. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). We report four regressions for each outcome: with and without time-varying control variables for both the pre-match sample and the post-match sample. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Capex and R&D & Change in other LT Investments

	Capex / Assets$_{t-1}$				R&D & Change in Other LT Investments / Assets$_{t-1}$			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011	-0.005*	-0.005*	-0.007**	-0.007**	-0.001	-0.001	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	13,886	13,713	7,645	7,613	13,873	13,713	7,640	7,613
r^2 (within FE)	0.0004	0.030	0.001	0.028	0.0001	0.002	0.000	0.002

Panel B. Change in Inventories

	Change in Raw Inventories / Assets$_{t-1}$				Change in Finished Inventories / Assets$_{t-1}$			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011	-0.003***	-0.003***	-0.003**	-0.003**	0.000	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	13,873	13,713	7,640	7,613	13,873	13,713	7,640	7,613
r^2 (within FE)	0.0005	0.006	0.000	0.007	0.000	0.002	0.000	0.003

Panel C. Cash flow used in Acquisitions

	Cash Flow used in Acquisitions / Assets$_{t-1}$			
	Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)
Treatment × Post 2011	-0.001	-0.001	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
Industry × Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
Observations	13,886	13,713	7,645	7,613
r^2 (within FE)	0.000	0.001	0.000	0.001

Table 6. Differential Effect on Industries with High TC Access: Leverage and Cash

This table estimates difference-in-differences regressions for bank debt and cash holdings, focusing on an additional interaction term that captures an industry's inherent access to trade credit. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). *High TC Access* equals one for firms in 3-digit ISIC industries in which the time to make payments exceeds that of the time to receive payments, based on U.S. Compustat firms. We report four regressions for each outcome variable: with and without time-varying control variables for both the pre-match sample and the post-match sample. At the bottom of each column we report a t-test evaluating whether the two coefficients, *Treatment × Post 2011 × Hi TC Access = 0* and *Treatment × Post 2011 × Hi TC Access = 1* are equal. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Leverage and Cash Holdings

	Bank Debt / Assets				Cash Holdings / Assets			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011 × Hi TC Access = 0	-0.010	-0.012*	-0.005	-0.007	0.009**	0.009**	0.012**	0.013**
	(0.008)	(0.007)	(0.010)	(0.010)	(0.004)	(0.004)	(0.005)	(0.005)
Treatment × Post 2011 × Hi TC Access = 1	-0.015*	-0.016*	-0.017	-0.017*	0.005	0.005	0.003	0.004
	(0.009)	(0.008)	(0.011)	(0.010)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	13,412	13,243	7,459	7,428	13,412	13,243	7,459	7,428
r^2 (within FE)	0.001	0.054	0.001	0.046	0.001	0.018	0.002	0.015
				Test for difference in coefficients				
Difference in coefficients	-0.005	-0.004	-0.012	-0.010	-0.004	-0.005	-0.009	-0.010
	(0.012)	(0.011)	(0.015)	(0.014)	(0.006)	(0.005)	(0.007)	(0.007)

Panel B. Short-term and Long-Term Leverage

	Short-Term Bank Debt (< 1 year) / Assets				Long-Term Bank Debt (>= 1 year) / Assets			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011 × Hi TC Access = 0	-0.026***	-0.026***	-0.029***	-0.030***	0.016***	0.014***	0.025***	0.022***
	(0.007)	(0.007)	(0.009)	(0.009)	(0.005)	(0.005)	(0.007)	(0.006)
Treatment × Post 2011 × Hi TC Access = 1	-0.022***	-0.023***	-0.023**	-0.024***	0.007	0.007	0.006	0.007
	(0.008)	(0.008)	(0.009)	(0.009)	(0.006)	(0.006)	(0.007)	(0.007)
Observations	13,412	13,243	7,459	7,428	13,412	13,243	7,459	7,428
r^2 (within FE)	0.005	0.018	0.006	0.018	0.002	0.032	0.005	0.031
	Test for difference in coefficients							
Difference in coefficients	0.005	0.003	0.006	0.006	-0.009	-0.007	-0.018*	-0.016*
	(0.011)	(0.011)	(0.013)	(0.013)	(0.008)	(0.008)	(0.010)	(0.009)

Table 7. Differential Effect on Industries with High TC Access: Trade Credit

This table estimates difference-in-differences regressions for net accounts payable (= accounts payable - accounts receivable), accounts payable and accounts receivable. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). *High TC Access* equals one for firms in 3-digit ISIC industries in which the time to make payments exceeds that of the time to receive payments, based on U.S. Compustat firms. We report four regressions for each outcome variable: with and without time-varying control variables for both the pre-match sample and the post-match sample. At the bottom of each column we report a t-test evaluating whether the two coefficients, *Treatment × Post 2011 × Hi TC Access = 0* and *Treatment × Post 2011 × Hi TC Access = 1* are equal. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Net Accounts Payable

	Net Accounts Payable / Assets			
	Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)
Treatment × Post 2011 × Hi TC Access = 0	0.016*	0.013	0.022*	0.019
	(0.009)	(0.009)	(0.012)	(0.012)
Treatment × Post 2011 × Hi TC Access = 1	0.029***	0.027***	0.031**	0.028**
	(0.010)	(0.010)	(0.013)	(0.013)
Observations	13,412	13,243	7,459	7,428
r^2 (within FE)	0.002	0.015	0.004	0.015
	Test for difference in coefficients			
Difference in coefficients	0.013	0.014	0.008	0.009
	(0.014)	(0.014)	(0.018)	(0.017)

Panel B. Accounts Payable and Accounts Receivable

	Accounts Payable / Assets				Accounts Receivable / Assets			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011 × Hi TC Access = 0	-0.004	-0.006	-0.010	-0.011	-0.020***	-0.019***	-0.032***	-0.030***
	(0.008)	(0.007)	(0.010)	(0.010)	(0.006)	(0.006)	(0.008)	(0.008)
Treatment × Post 2011 × Hi TC Access = 1	0.020**	0.017**	0.024**	0.020**	-0.010	-0.010	-0.006	-0.007
	(0.008)	(0.008)	(0.011)	(0.010)	(0.008)	(0.008)	(0.010)	(0.009)
Observations	13,412	13,243	7,459	7,428	13,412	13,243	7,459	7,428
r^2 (within FE)	0.001	0.034	0.003	0.040	0.003	0.018	0.006	0.025
	Test for difference in coefficients							
Difference in coefficients	0.024**	0.023**	0.034**	0.031**	0.011	0.009	0.025**	0.022*
	(0.011)	(0.010)	(0.015)	(0.014)	(0.010)	(0.010)	(0.013)	(0.012)

Table 8. Differential Effect on Industries with High TC Access: Investment and Inventories

This table estimates difference-in-differences regressions for capital expenditures, investments in R&D and other long-term assets, changes in inventories, and cash flow used in acquisitions. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). *High TC Access* equals one for firms in 3-digit ISIC industries in which the time to make payments exceeds that of the time to receive payments, based on U.S. Compustat firms. We report four regressions for each outcome variable: with and without time-varying control variables for both the pre-match sample and the post-match sample. At the bottom of each column we report a t-test evaluating whether the two coefficients, *Treatment × Post 2011 × Hi TC Access = 0* and *Treatment × Post 2011 × Hi TC Access = 1* are equal. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Capex and R&D & Change in other LT Investments

	Capex / Assets$_{t-1}$				R&D & Change in Other LT Investments / Assets$_{t-1}$			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011 × Hi TC Access = 0	-0.011***	-0.010**	-0.014***	-0.013***	-0.002***	-0.002***	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × Post 2011 × Hi TC Access = 1	0.002	0.002	0.001	0.000	0.001	0.001	0.002	0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	13,412	13,243	7,459	7,428	13,399	13,243	7,454	7,428
r^2 (within FE)	0.001	0.030	0.002	0.029	0.001	0.002	0.001	0.003
	Test for difference in coefficients							
Difference in coefficients	0.013**	0.012**	0.016**	0.013*	0.004***	0.004***	0.004**	0.004**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.001)	(0.001)	(0.002)	(0.002)

Panel B. Change in Inventories

	Change in Raw Inventories / Assets$_{t-1}$				Change in Finished Inventories / Assets$_{t-1}$			
	Pre-Match Sample		Post-Match Sample		Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post 2011 × Hi TC Access = 0	-0.004**	-0.004***	-0.005***	-0.005***	0.000	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × Post 2011 × Hi TC Access = 1	-0.004*	-0.003*	-0.001	-0.001	0.000	0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	13,399	13,243	7,454	7,428	13,399	13,243	7,454	7,428
r^2 (within FE)	0.001	0.006	0.001	0.007	0.000	0.002	0.000	0.003
				Test for difference in coefficients				
Difference in coefficients	0.000	0.001	0.004	0.003	0.000	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)

Panel B. Cash Flow used for Acquisitions

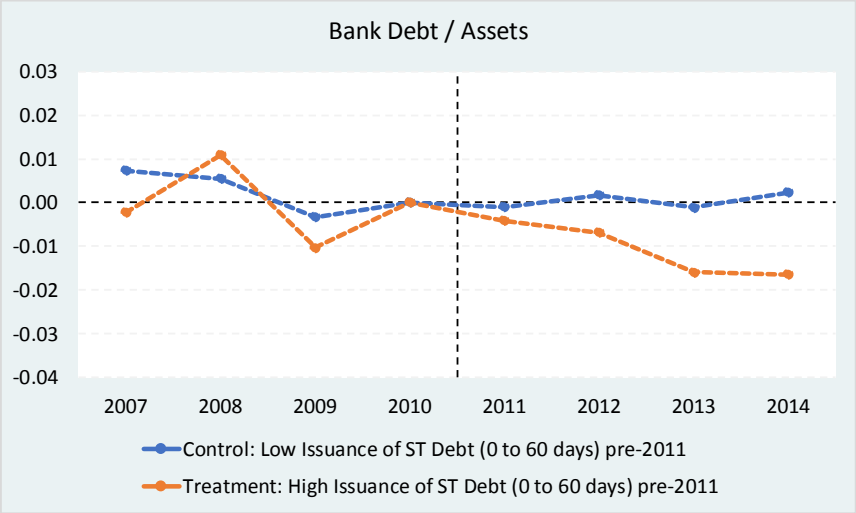
	Cash Flow used in Acquisitions / Assets$_{t-1}$			
	Pre-Match Sample		Post-Match Sample	
	(1)	(2)	(3)	(4)
Treatment × Post 2011 × Hi TC Access = 0	-0.001	-0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.003)	(0.003)
Treatment × Post 2011 × Hi TC Access = 1	-0.001	-0.001	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	13,412	13,243	7,459	7,428
r^2 (within FE)	0.000	0.001	0.000	0.001
	Test for difference in coefficients			
Difference in coefficients	0.000	0.001	0.001	0.000
	(0.004)	(0.004)	(0.005)	(0.005)

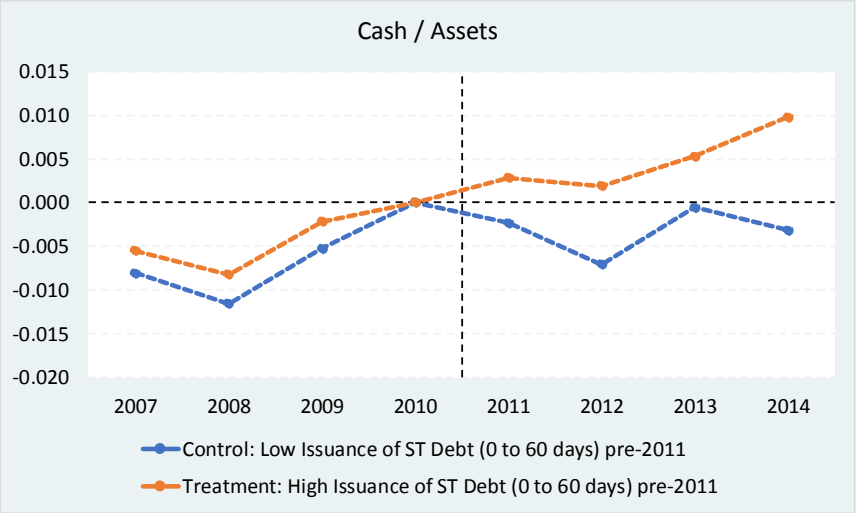
Internet Appendix

Figure IA.1. Trends around shock for Treatment and Control Firms

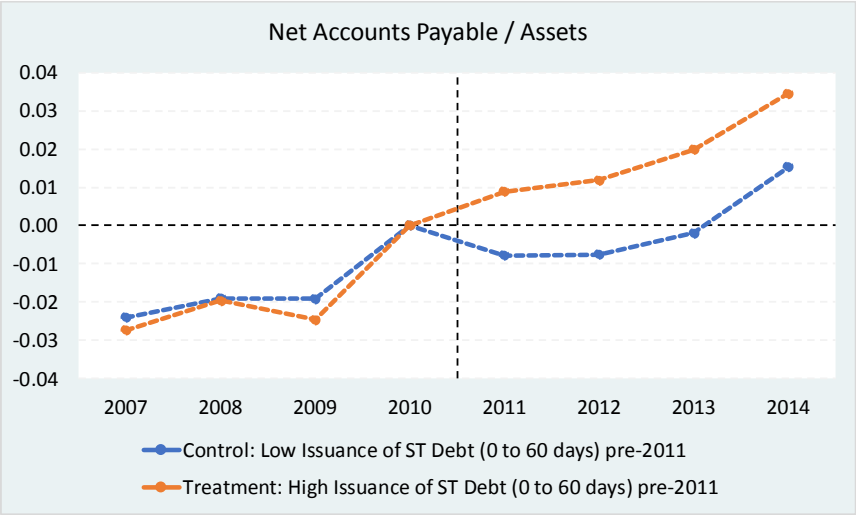
This figure plots time coefficients from regressions of various outcomes on year indicator variables for treatment and control firms (pre-match). Panel A reports estimates for regressions on bank debt and cash. Panel B reports estimates for net accounts payable and Capex. Panel C reports estimates for the change in raw inventories and in finished products inventories. Regressions include firm fixed effects and time-varying control variables. We omit the $Year_{2010}$ and its interaction with *Treatment*, (i.e. 2010 acts as the reference year).

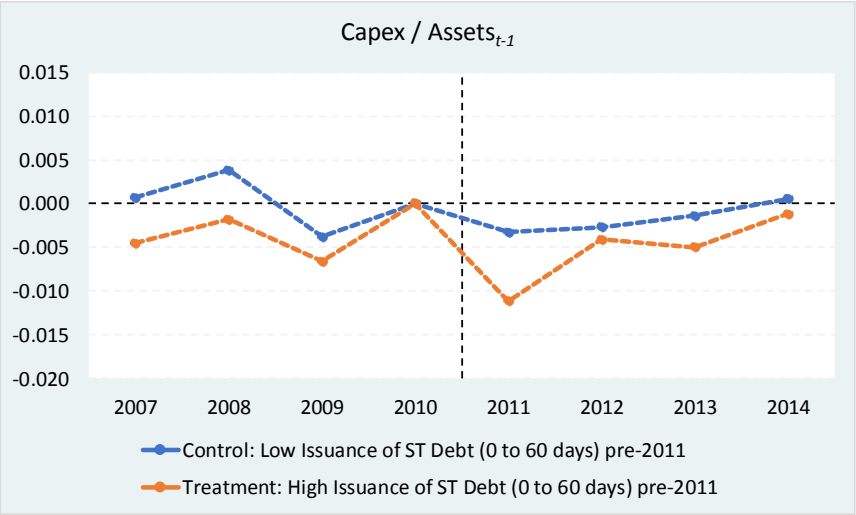
Panel A. Leverage and Cash Holdings



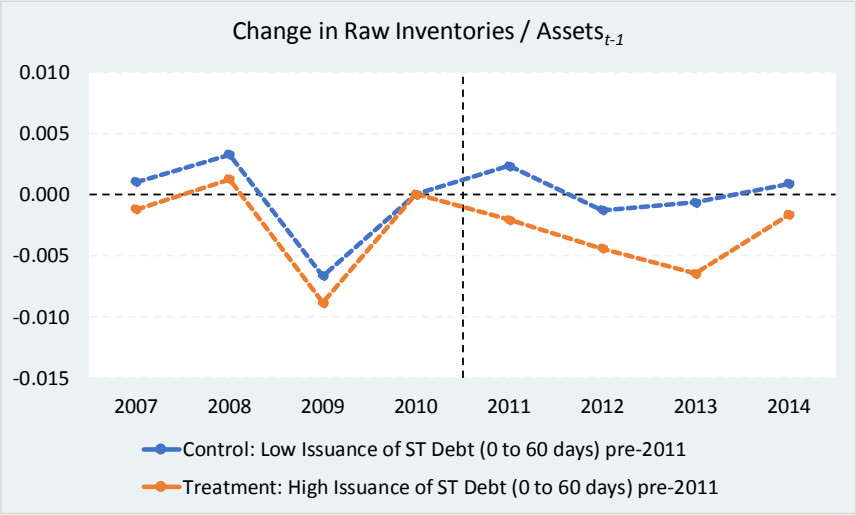


Panel B. Net Accounts Payable and Capex





Panel C. Change in Inventories



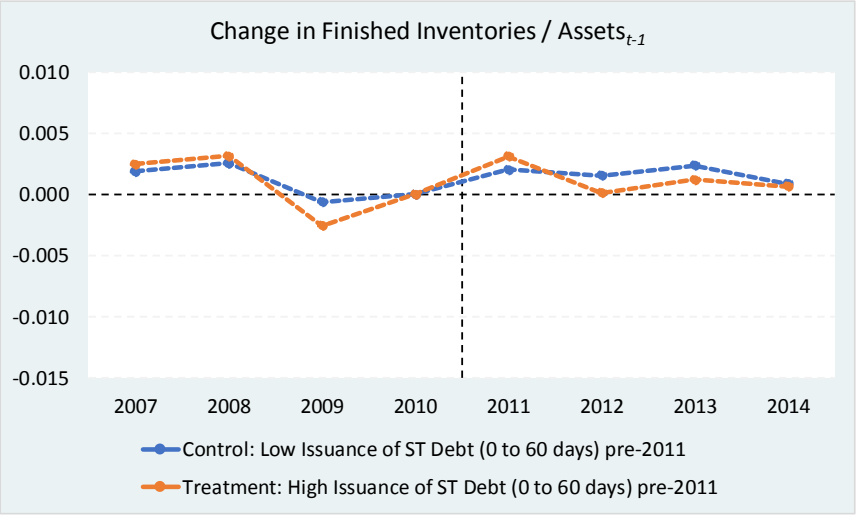


Table IA.1. Sample description: Number of firms and observations by industry

This table reports the number of distinct firms and the number of observations by industry at the ISIC section level for the full sample period (2007 – 2014).

ISIC Section	Number of Observations	Number of Firms
A - Agriculture, hunting and forestry	440	59
B - Fishing	19	3
C - Mining and quarrying	582	97
D - Manufacturing	3,860	524
F - Construction	1,264	200
G - Wholesale and retail trade	5,457	779
H - Hotels and restaurants	200	29
I - Transport, storage and communications	448	71
K - Real estate, renting and business activities	1,288	195
M - Education	34	5
N - Health and social work	39	8
O - Other community, social and personal service activities	255	36
Total	13,886	2,006

Table IA2. Propensity Score Matching Diagnostics

Panel A reports parameter estimates from the probit model used in estimating the propensity scores to obtain the sample of control and treatment firms. The dependent variable equals one for firms in the top quintile of pre-shock issuance of short-term debt (0 to 60 days), and zero for firms in the bottom two quintiles. We implement a one-to-one, nearest neighbor matching procedure without replacement. The matching of treatment and control firms is performed the year before the shock (2010). We require control firms to have at least one observation before and after the shock. Panel B presents pairwise comparisons of the variables used to perform the matching (for both the pre and post-match samples).

Panel A. Probit Estimation

	Dependent Variable: *I.*[High Access to Short-Term Debt (0 to 60 days) Pre-2011]
	(1)
Constant	-6.175***
	(0.584)
$Ln(Assets)_{t-1}$	0.638***
	(0.060)
$Ln(BV\ Equity)_t$	-0.396***
	(0.051)
$Asset\ tangibility_t$	-0.323
	(0.220)
$Ln(Firm\ Age)_t$	0.225***
	(0.046)
$Asset\ growth_t$	0.249*
	(0.130)
$Asset\ turnover_t$	0.190***
	(0.023)
Observations	1,876
Pseudo r^2	0.080

Panel B. Pairwise Comparisons of Covariates used in Matching Procedure

	Pre-Match Sample (Pre-2011 Period Means)			Post-Match Sample (Pre-2011 Period Means)		
	Control (Pre-Match)	Treatment	*t*-test Diff. in means (p-values)	Control (Post-Match)	Treatment	*t*-test Diff. in means (p-values)
Ln(Assets)	17.14	17.12	0.017	17.14	17.12	0.860
Ln(BV Equity)	16.13	16.02	0.142	16.13	16.02	0.319
Asset tangibility	15.0%	14.3%	0.000	15.0%	14.3%	0.387
Firm Age	25.30	24.08	0.098	25.30	24.08	0.230
Asset growth	14.9%	14.4%	0.484	14.9%	14.4%	0.581
Asset Turnover	2.38	2.55	0.000	2.38	2.55	0.142
No. of Obs. in Pre-2011 Period	1,805	1,950		1,805	1,950	
No. of Unique Firms	512	532		532	532	

Table IA.3. Effect of Shock to Very Short-Term Debt on the Issuance of Short Term Debt for Different Maturities

This table estimates difference-in-differences regressions for the issuance of short term debt for different maturities scaled by beginning of the year assets. The main coefficient of interest is on the interaction term *Treatment × Post 2011*. The indicator variable *Treatment* is equal to one for firms in the top quintile of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control sample. *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). We report two regressions for each maturity: with t time-varying control variables for both the pre-match and the post-match sample. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

	0 to 30 days		31 to 60 days		61 to 90 days		91 to 180 days		181 to 270 days		271 to 365 days	
	Pre-Match Sample	Post-Match Sample	Pre-Match Sample	Post-Match Sample	Pre-Match Sample	Post-Match Sample	Pre-Match Sample	Post-Match Sample	Pre-Match Sample	Post-Match Sample	Pre-Match Sample	Post-Match Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment × Post 2011	-0.069***	-0.067***	-0.046***	-0.044***	-0.026***	-0.023***	-0.023***	-0.026***	0.012**	0.015***	0.026***	0.025***
	-0.004	-0.004	-0.003	-0.003	-0.004	-0.004	-0.007	-0.007	-0.005	-0.005	-0.005	-0.005
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,479	7,435	13,479	7,435	13,479	7,435	13,479	7,435	13,479	7,435	13,479	7,435
r2 (within FE)	0.105	0.082	0.072	0.055	0.022	0.021	0.022	0.027	0.015	0.019	0.018	0.019

Table IA.4. Robustness Test: Main DID Results using Broader Definition of Treatment (Pre-Match Sample)

This table reports results for the regressions in Tables 3 to 5 using a broader definition of *Treatment*. In these regressions the indicator variable *Treatment* is now equal to one for firms in the top two quintiles (i.e. quintiles 4 and 5) of pre-period use of short-term bank debt (0 to 60 days); firms in the bottom two quintiles operate as the control group (the definition of control group is unchanged). *Post 2011* takes a value of one in the years after the shock (2011 – 2014) and zero in the pre-period (2007 – 2010). For brevity we report one regression for each outcome: with time-varying control variables for the pre-match sample. All regressions include firm fixed effects and industry-year fixed effects. Standard errors (in parentheses) are clustered by firm. ***, **, and * indicates significance at the 1%, 5% and 10% level, respectively.

Panel A. Debt and Cash Outcomes

	Issuance of ST Debt (0 to 60 days) / $Assets_{t-1}$	Bank Debt / Assets	Cash Holdings / Assets	Short-Term Bank Debt (< 1 year) / Assets	Long-Term Bank Debt (>= 1 year) / Assets
	(1)	(2)	(3)	(4)	(5)
Treatment × Post 2011	-0.074***	-0.005	0.005**	-0.015***	0.010***
	(-0.004)	(-0.004)	(-0.002)	(-0.004)	(-0.003)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Observations	16,683	16,683	16,683	16,683	16,683
r2 (within FE)	0.073	0.057	0.016	0.015	0.035

Panel B. Trade Credit Outcomes

	Net Accounts Payable / Assets	Accounts Payable / Assets	Accounts Receivable / Assets
	(1)	(2)	(3)
Treatment × Post 2011	0.011**	0.002	-0.009**
	(-0.005)	(-0.004)	(-0.004)
Industry × Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Observations	16,683	16,683	16,683
r2 (within FE)	0.014	0.039	0.018

Panel C. Investment Outcomes

	Capex / Assets$_{t-1}$	Change in Deferred Asset Charges / Assets$_{t-1}$	Change in Raw Inventories / Assets$_{t-1}$	Change in Finished Inventories / Assets$_{t-1}$	Cash Flow used for Acquisitions / Assets$_{t-1}$
	(1)	(2)	(3)	(4)	(5)
Treatment × Post 2011	-0.005**	0.000	-0.002**	0.000	0.000
	(-0.002)	(-0.001)	(-0.001)	(-0.001)	(-0.001)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Observations	16,683	16,683	16,683	16,683	16,683
r2 (within FE)	0.032	0.001	0.006	0.002	0.001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