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Collateral damaged? Priority structure, credit supply, and firm performance

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ABSTRACT

A unique legal reform in 2004 in Sweden redistributed collateral rights from banks holding floating liens to unsecured creditors without changing the value of assets on firms' balance sheets. Using a country-wide panel of all incorporated firms, we document that a zero-sum redistribution of collateral rights and the resulting reduction in collateral capacity towards banks contracts the amount and maturity of corporate debt and leads firms to slow investment and forego growth. Altering their allocation of assets, firms reduce particularly those assets with a low collateralizable value for banks and also hoard more cash. However, the reform has no impact on corporate capital intensity or efficiency, suggesting that under these newly binding credit constraints firms simply shrink their operations.

1. Introduction

A firm's debt capacity depends on how much lenders can expect to recover if the firm defaults. When a firm is liquidated, the slice of the liquidation proceeds that a given creditor receives depends on two margins. The first is the total value of the liquidated assets, or “the size

of the pie”. Corroborating this balance sheet channel, Gan (2007) and Chaney et al. (2012) for example show that shocks to real estate values affect corporate debt and investment. The second margin is the share of the liquidated assets that the lender is entitled to, or “the share of the pie”. The latter, less studied, determinant of collateral capacity is the focus of our paper.

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We use a unique legal reform and data covering the universe of incorporated firms in Sweden to investigate how an exogenous transfer of priority rights from banks to other creditors affects corporate financing, investment, as well as firm performance and growth. Although the reform does not change the quantity or the value of firm assets, the redistribution of control rights should not be neutral for the firm, because debt obtained from non-bank creditors such as employees, tax authorities and trade creditors is inelastic with respect to expected recovery rates.¹

The legal reform in question reduces the liquidation payoff to floating lien holders in favor of unsecured claimants. Floating liens allow firms to pledge their movable property (mainly inventories, receivables, and equipment). Before 2004, floating liens were special priority claims in Sweden that enabled banks to seize a firm's pledged movable property prior to bankruptcy or liquidation, before other creditors were paid, and without the intervention of a court.

A change in the law on January 1, 2004 introduced two important changes that affect the distribution of payoffs among creditors. First, the special priority rights of all floating liens were abolished, implying that banks have to wait for a court-appointed liquidator to obtain their share of the liquidation proceeds. Second, the share of liquidation proceeds a bank is entitled to is now capped at 55% of the eligible movable assets. Since the law had no direct impact on the underlying asset pool, the legal reform constituted a zero-sum transfer of control rights that lowered the liquidation payoff to banks while increasing the liquidation payoff to other creditors such as suppliers, employees, and the tax authorities.²

Our empirical setting is uniquely suited to advancing our understanding of the micro-foundations of the collateral channel. While most prior studies exploit variation in the market value of assets (“the size of the pie”), we exploit variation in the fraction of the firm's assets that goes to lenders when the firm is liquidated (“the share of the pie”).³ Studying this second margin is not only important as a complement to previous studies, but it also offers two distinctive features.

First, our setting provides exogenous variation in firms' collateral capacity that is unrelated to the value of their assets. To see why this matters, suppose we observe a reduction in house prices. The debt capacity of the affected real estate owners should decrease, as creditors have less valuable assets to seize in the case of liquidation. But at the

¹ On the one hand, it may be unsustainable for a firm to raise additional financing from employees by retaining their salaries or from tax authorities by delaying the payment of taxes due. On the other hand, trade credit is normally uncollateralized and there is no evidence that suppliers adjust their prices or maturities based on the presence of collateral. Consequently, it will be difficult for a firm to replace bank credit with financing from these other stakeholders. And even if it were possible, the debt owed to employees, tax authorities, and suppliers typically has a shorter maturity, making it less suitable for financing investment.

² Cerqueiro et al. (2016) analyze this legal reform using *loan-level data from one bank*. They find that this bank responds to the fall in the value of its collateral by reducing both the credit it extends and its monitoring activities.

³ From the perspective of an individual lender, liquidation costs could possibly also be interpreted as affecting the “share of the pie”. Theories in which informational frictions and collateral play a role often assume that the value that creditors obtain from liquidating a firm is some fraction of the value of the firm's assets. The legal reform in our study also reduces this fraction and hence resembles in some way an increase in liquidation costs for an individual lender. Another important determinant of liquidation costs is how quickly creditors can seize the assets upon the default of a debtor. This alternative channel has been analyzed for example in Vig (2013), who studies the effect on corporate debt structure of the 2002 Indian bankruptcy reform, which aimed at speeding up the process of debt recovery in India. He finds that the strengthening of creditor rights reduced collateralized credit because borrowers anticipated a greater liquidation bias in bankruptcy. In Vig (2013) liquidation costs are essentially a deadweight cost, while in our legal reform the reduction in liquidation value for banks is associated with a redistribution of liquidation proceeds to other creditors.

same time real estate owners also have lower net worth, which can by itself reduce the supply of credit. While variation in asset prices makes it hard to disentangle these two supply effects, our legal reform provides exogenous variation in the distribution of collateral while keeping the size of the borrowers' balance sheets constant.

Second, our setting enables us to more effectively disentangle credit supply from credit demand. While the previous example illustrates how real estate wealth affects the supply of credit, a decrease in house prices can also lead to borrowers *demanding* less credit due to the negative wealth effect (Mian and Sufi (2014)). Our legal reform “shuts down” this balance sheet channel. We are thus uniquely able to focus exclusively on how the supply of credit adjusts to a redistribution of collateral rights and affects firm financing, asset structure and the real economy in normal times.

We combine this quasi-experimental setting with comprehensive data for Sweden over the 2000–06 period. We obtain accounting and collateral information from annual reports that all incorporated firms must file with the Swedish Companies Registration Office. We additionally obtain the firm's date of registration, as well as investment and industry affiliation data from Statistics Sweden.

We study the effects of the change in the law by comparing a treated group of firms that pledged floating liens before 2004 with a control group of firms that did not. To ensure our control group provides a good counterfactual, we select for each treated firm a set of control firms with the same age and economic activity at the 5-digit industry code level, thereby ensuring that our results are not driven by life-cycle patterns or by industry-specific shocks, such as shifts in investment opportunities.

We obtain the following five main findings.

First, following the change in the law, firms reduce their collateral to debt ratio by 5% on average. This reduction is caused by the 2004 law change (and by the consequent decline in floating lien use), since the use of fixed liens is unchanged.

Second, following the reduction in collateral capacity, firms hold less debt and with shorter maturity. Treated firms reduce their leverage ratio by almost 1.5% and their fraction of long-term debt to total debt by 11%. We also find a reduction in credit line limits that is compensated for with an increase in short-term borrowing.

Third, we find that the legal reform affects investment decisions and asset structure. Treated firms reduce their investment rate by 7%, affecting investment in both machinery and equipment as well as buildings and land. While fixed tangible assets (as a share of assets) drops by 3%, inventories, which can be pledged via floating liens, drop by 4%. In contrast, cash holdings increase by 7%. Our findings thus indicate that merely redistributing collateral rights can distort asset structure, consistent with the notion in Gilje et al. (2018) that *future* collateral requirements in debt renegotiations can be a source of investment distortions that enhance collateral but sacrifice higher returns.

Fourth, the law has no effect on production technology, since capital, labor, and sales all drop roughly at the same rate. Instead, treated firms scale down the level of their operations. However, firm profitability and sales growth decline, indicating that access to credit had previously helped firms to achieve an efficient scale of operations.

Finally, we find the effect of the law on firm bankruptcy to be negligible.

Earlier work in this area has focused on large-scale legal reforms undertaken around the world. For example Aretz et al. (2016) study how easing the actual pledging of hard assets as collateral affects larger businesses. They find that firms with hard assets increase leverage, reduce borrowing costs and extend loan maturities. Our study is also related to a growing literature that studies the impact of changes in the contracting environment. Campello and Larrain (2016) analyze a legal reform that permitted movable assets to be pledged as collateral and find this broadened access to credit, resulting in higher employment and capital stocks. While Campello and Larrain (2016) illustrate why

debtors' ability to pledge "movable" assets as collateral is important, our study highlights the importance of lender *seniority* over such movable assets and that the allocation of collateral rights affects economic development. Calomiris et al. (2017) use cross-country data to show that the enforcement of movable collateral rights explains average loan-to-value ratios and aggregate sectorial activity.⁴

We complement this literature by analyzing a change in the law that modified (along the intensive margin) a well-defined legal system in a developed economy, and by analyzing its effects on the universe of registered firms. In particular, we study both the resulting impact on the asset and liability structure of the firm and a much wider range of firm outcomes in order to provide more conclusive evidence on the effects of the distribution of collateral rights.

The remainder of this paper is organized as follows. Section 2 describes the change in the law. Section 3 details the data and variables. Section 4 explains the empirical methodology. Section 5 presents our results. Section 6 presents a number of robustness tests. Section 7 concludes.

2. Institutional background

Below we describe floating liens in the context of the Swedish law, as well as the legal reform that we analyze in the paper. We provide more details about the institutional background in Appendix 2.

2.1. Floating liens

A *floating lien* (or *floating charge*) allows a business to pledge its movable property, which includes inventories, accounts receivable, equipment, machinery, and intangible assets.⁵ While the floating lien provides a security interest on these classes of assets, it is not attached to any particular asset. Consequently, the pool of assets underlying a floating lien can change over time. For instance, suppose that a company pledges a floating lien to a creditor and assume that the only asset the firm possesses is equipment. The actual items of this property can change over time due to the purchase and disposal of equipment. The borrower is allowed to use, collect, or dispose of its movable property, and the floating lien automatically extends to any movable property that is acquired by the company while the debt is outstanding. The floating lien does not provide the creditor legal rights to the firm's existing assets until some "crystallizing event" occurs—for instance, the debtor defaults on the loan or files for bankruptcy. The floating lien then fixes itself (or "crystallizes") to the existing assets covered by the lien, which will be protected by a Bankruptcy Trustee, and the creditor obtains a prioritized claim on the liquidation value of these assets.

2.2. The legal reform

Before 2004, floating liens had *special priority* rights that could be activated prior to bankruptcy. In particular, the lien could be activated in the event that any other creditor seized the firm's property. Floating liens were therefore senior to: (1) *general priority claims*, which included costs incurred by bankruptcy or reconstruction procedures, taxes, and most of the wage claims of employees (a limited part had special priority rights); and (2) *ordinary claims*. The enforcement of both general priority claims and ordinary claims requires a court order declaring

the debtor's bankruptcy.

On January 1, 2004, the law that regulates floating liens (or, "the law") was changed. The new law introduced two important changes. First, the special priority rights of floating liens were downgraded and floating liens became general priority claims, implying that under the new regime lien holders can seize the debtor's assets only in the case of bankruptcy. The new law thereby reduced the liquidation payoff of lien holders, since the assets covered by the floating lien now also had to contribute to the costs of bankruptcy or reorganization procedures, and to taxes. Second, the new law reduced the share of total eligible assets that could be covered in a floating lien from 100% to 55% of a debtor's total eligible assets that remain after senior creditors have been paid. At the same time, the new law expanded the categories of assets that could be pledged in the floating lien to take in all asset types, thus including cash, bank deposits, financial assets, and real estate. Overall, the general assessment was that the assets available under the floating lien scheme shrank while increasing the assets available in liquidation to unsecured creditors.⁶

All elements of the policy change were "mandatory" in the sense that the parties involved in loan contracting could not avoid the consequences of the law by agreeing on a new contract that would give floating-lien holders the priority they would have obtained automatically prior to the reform.

2.3. Transition period

The law became effective on January 1, 2004. Floating liens granted after this date were immediately governed by the new rules, while all floating liens granted before that date automatically converted by January 1, 2005. Between January 1, 2004 and January 1, 2005 creditors had the opportunity to reevaluate their exposures and to call in debtors to renegotiate terms, which could involve a request for additional collateral. If the lender and the borrower agreed on new contract terms, the existing floating lien would be converted to the new regime. If the lender and the borrower did not reach an agreement, the lender had the right to demand full repayment and terminate the lending agreement. The presence of a brief, fixed-term transition period—during which parties could renegotiate or terminate their existing contracts—ensured that old-regime contracts by default converted into new-regime contracts 12 months after the introduction of the new law. Our results should therefore not be driven by the short-term costs of adjustment of contracts, costs that delay the transition to a new equilibrium.

2.4. Objectives and consequences of the law

The change in the law had two explicit objectives. The first was to avoid inefficient liquidation and allow viable companies to enter a reorganization process. The second was to give incentives to creditors to screen and monitor their borrowers rather than rely excessively on collateral. The fact that the law change led to many businesses experiencing a decline in pledgeable assets had unintended consequences however. Using loan-level data from a large Swedish bank, Cerqueiro et al. (2016) find that the bank responded to the 2004 law change by increasing interest rates, tightening credit limits, and reducing its monitoring activities. These results indicate that the 2004 law made it more difficult for many companies to borrow against this type of

⁴ See also von Lilienfeld-Toal et al. (2012), Vig (2013), and Rodano et al. (2016).

⁵ Gennaioli and Rossi (2013) show that in the presence of strong creditor rights, the optimal contractual resolution of financial distress involves the use of a floating lien. Franks and Sussman (2005) document that the floating charge in the UK works well as the basis of the foreclosure of small and medium-sized companies, while Djankov et al. (2008) generalize this result to a broad set of countries.

⁶ Strömberg and Thorburn (1996) analyze a sample of Swedish firms that filed for bankruptcy between 1987 and 1991 and report that the median recovery rate for floating lien holders (banks) is 83%, while the median recovery rate for suppliers and employees was 0%, and tax authorities received 12.5%. Bergström, Eisenberg and Sundgren (2004) document a similar redistributive reform in Finland on the distribution of claims in bankruptcy. They find that average payments to unsecured creditors increased after the reform while payments to secured creditors decreased.

Table 1

Definition of variables.

Data sources: official annual reports, filed at Swedish Company Registration Office. Variable definitions provided by credit bureau UC.

Variable	Definition
Total collateral / Debt	Sum of pledged fixed liens and floating liens as a % of total liabilities
Fixed liens / Debt	Pledged fixed liens as a % of total liabilities
Debt / Assets	Total liabilities as a % of total assets
Long-term debt / Debt	Liabilities with maturity ≥ 1 year as a % of total liabilities
Long-term loans / Debt	Loans from financial institutions with maturity ≥ 1 year as a % of total liabilities
Short-term loans / Debt	Loans from financial institutions with maturity < 1 year as a % of total liabilities
Lines of credit limit / Debt	Lines of credit limit as a % of total liabilities
Total net investment / Assets	Total net investment as a % of total assets
Investment in machinery and equipment / Assets	Net investment in machinery and equipment as a % of total assets
Investment in land and buildings / Assets	Net investment in land and buildings as a % of total assets
Tangible assets / Assets	Tangible assets as a % of total assets
Inventories / Assets	Inventories as a % of total assets
Liquid assets / Assets	Cash and equivalents as a % of total assets
(Machinery + Equipment) / Employees	Sum of machinery and equipment (in mSEK) per employee
Sales / (Machinery + Equipment)	Total revenue as a % of the sum of machinery and equipment
Employment growth	Year-to-year change in the log of total employees
Asset growth	Year-to-year change in the log of total assets (mSEK)
Firm filed for bankruptcy	Indicates whether the firm filed for bankruptcy in the current year

collateral.⁷ We now use the quasi-experimental setting this legal change provides to study the effects of the 2004 law on corporate financing structure and investment decisions.

3. Data and variables

3.1. Data sources

We obtain our data from three sources. The main source is the Swedish credit bureau UC, which holds annual accounting information for all incorporated firms registered in Sweden. All firms registered in Sweden—including dormant companies and companies in liquidation—have to file a yearly report with the Swedish Companies Registration Office. Our sample comprises almost 200,000 firms, which we observe over the period 2000–06. The total number of firm-year observations is around 1.3 million.

We extract from this database the common balance sheet and income statement items, such as assets, liabilities, and earnings, as well as off-balance sheet information about nominally pledged collateral (floating liens and fixed liens). From this dataset we also extract information on credit lines, including commitment values and amounts drawn, numbers of employees, and information on whether the firm has filed for bankruptcy.

The second source of data is SCB (Statistics Sweden), from which we obtain investment data and industry codes. For each firm we obtain total net investment, net investment in machinery and equipment, and net investment in land and buildings. These investment flows are net in the sense that they are adjusted for depreciation. Industry classification is according to the 5-digit Swedish Standard Industrial Classification (SNI) codes, which is very similar to US industry classification systems.

Our third data source is the SCRO, from which we obtain the firm's date of registration, which we use to determine the firm's age.

3.2. Variables and descriptive statistics

Table 1 lists the variables used in our empirical analysis, and Table 2 displays some descriptive statistics for the year 2003. Total

collateral is the total value of the nominal collateral claims of creditors on the firm's assets, which includes both fixed liens and floating liens.⁸ Fixed liens are defined as the nominal value of claims on the firm's real property, as opposed to floating liens, which denote the nominal value of claims on the firm's movable property. In our main specifications we analyze these collateral variables as a proportion of the firm's debt, but in robustness tests we show that our results hold when we analyze our variables in log levels.

We compute several measures related to debt financing. We use the debt-to-assets ratio as our main measure of financial leverage. To analyze debt maturity, we compute the ratio of long-term debt (with maturity of at least 1 year) to total debt. We also consider in additional tests alternative debt measures that include only bank debt (or loans). The lines of credit limit variable is the maximum amount of credit a business can obtain under its lines of credit. We scale all bank debt variables by the firm's total debt.

We employ three measures of investment, which we analyze as a proportion of the firm's assets. Total net investment equals capital

⁸ To better understand our collateral value definition, consider the following numerical example. A firm simultaneously registers two floating liens with nominal values of \$100 and \$50, respectively (see Appendix 2 for details of the registration process). The total value of registered floating liens for this firm is thus \$150. The firm receives the two corresponding certificates (of \$100 and \$50, respectively). To avoid mixing up the loan and the collateral numbers, assume the firm obtains a \$110 loan and gives the \$100 certificate as collateral to the bank. Until the loan is fully repaid, the firm must report at the end of each fiscal year the total value of floating lien certificates pledged, which equals—in this case—\$100. This is irrespective of the actual assets that are available to back up the \$100 floating lien. If the firm would enter bankruptcy, and the collateral is sold with proceeds of V , then the lender has a senior claim equal to $\min(V, \$100)$. That is, if the firm holds collateralizable movable assets in surplus of the floating liens it pledged, the lien holders will receive the full nominal value of the lien as a senior claim payment. However, if the liquidation value V of the collateralizable movable assets is smaller than the pledged lien, then the lien holder receives a senior claim payment of V and obtains a junior claim of $(100 - V)$. In the present paper we use the \$100 as the definition of the nominal value of floating liens (other types of collateral are similarly defined). We note that the receiving bank is likely to make an independent assessment of the actual value that the floating lien represents, for example by inspecting the assets currently owned by the firm. The lien could for example be valued at \$75. This assessed value is the measure of collateral value used in Cerqueiro, Ongena and Roszbach (2016). In Appendix 4 we provide a detailed description of different collateral measures.

⁷ The 2004 law was virtually reversed in 2009. While few confounding events occurred around 2004, the near reversal in 2009 cannot be confidently analyzed as it took place amid the financial crisis and vigorous national policy reactions to that crisis.

Table 2
Summary statistics.

Statistics are for the year 2003. The number of firms in the sample is 193,594. P25 and P75 refer to 25th and 75th percentiles, respectively. mSEK is millions of Swedish Kroner (8.5 SEK = 1 USD).

Variable	Mean	Standard deviation	P25	Median	P75
<i>Collateral</i>					
Total collateral / Debt	0.370	0.416	0.000	0.136	0.793
Fixed liens / Debt	0.159	0.323	0.000	0.000	0.000
<i>Debt and debt structure</i>					
Debt / Assets	0.588	0.272	0.436	0.653	0.825
Long-term debt / Debt	0.260	0.327	0.000	0.068	0.518
Long-term loans / Debt	0.149	0.257	0.000	0.000	0.242
Short-term loans / Debt	0.020	0.070	0.000	0.000	0.000
Lines of credit limit / Debt	0.320	0.276	0.084	0.218	0.417
<i>Investment</i>					
Total net investment / Assets	0.014	0.030	0.000	0.004	0.017
Investment in machinery and equipment / Assets	0.012	0.025	0.000	0.003	0.014
Investment in land and buildings / Assets	0.003	0.014	0.000	0.000	0.000
<i>Asset structure</i>					
Tangible assets / Assets	0.219	0.279	0.014	0.092	0.356
Inventories / Assets	0.117	0.210	0.000	0.000	0.157
Liquid assets / Assets	0.241	0.277	0.015	0.126	0.384
<i>Capital intensity and productivity</i>					
(Machinery + Equipment) / Employees	0.697	2.565	0.000	0.000	0.000
Sales / (Machinery + Equipment)	2.799	1.698	1.674	2.880	3.944
<i>Firm growth and size</i>					
Employment growth	-0.007	0.185	0.000	0.000	0.000
Asset growth	0.013	0.660	-0.108	0.005	0.157
Number of employees	10.598	127.539	2.000	3.000	6.000
Assets (log mSEK)	14.273	1.976	0.505	1.410	4.348
<i>Bankruptcy</i>					
Firm filed for bankruptcy	0.002	0.048	0.000	0.000	0.000

expenditures after accounting for the depreciation of existing assets. We separately consider investments in movable property (machinery and equipment) and investments in real property (land and buildings). This distinction is important because the change in the law we are investigating reduces the value of collateral for holders of floating liens (without changing the nominal value of the liens or the value of the firm's assets) and thus the collateralizable value of movable property (relative to real property).

We decompose the asset structure of the firm into three parts: tangible assets (which include machinery, equipment, and land and buildings), inventories, and liquid assets (which include cash and equivalents). We scale these variables by total assets.

We employ proxies for technology and the efficiency of the firm following Cronqvist et al. (2009). We measure the capital intensity of a firm as the combined value of machinery and equipment per employee. We compute firm operating efficiency as sales divided by the combined value of machinery and equipment.

We measure firm growth in terms of both employees and assets and calculate growth as the first difference of the natural logarithm of the variable of interest (either employees or assets). We truncate the growth variables at the 1st and 99th percentiles to remove the influence of outliers. We also provide descriptive statistics for two in-levels variables: the number of employees and firm assets (in logs).

Finally, we employ an indicator of whether the firm filed for bankruptcy during the book year.

4. Methodology

Our identification strategy exploits the 2004 change in law (effective on January 1st) that reduces the value of floating liens in Sweden.

We examine the effects of that change using the following difference-in-differences regression:

$$y_{it} = \alpha_i + \lambda_t + \beta(Treated_i \times Post - law_t) + u_{it},$$

where i indexes firms, t indexes time, y_{it} is the dependent variable, α_i are firm fixed effects, λ_t are time fixed effects, $Treated_i$ is a dummy variable indicating firms that pledged floating liens before the change in the law, $Post-law_t$ is a dummy variable indicating the period from 2004 to 2006, and u_{it} is an error term. The difference-in-differences estimate β measures the differential effect of the change in the law across firms that had pledged and firms that had not pledged floating liens before 2004. We cluster standard errors at the firm level.

We now turn to our main identifying assumptions. Both treated and control firms experience a reduction in their *debt capacity* as a result of the legal reform, as the reform shrinks the contracting space by reducing firms' ability to pledge their movable property to banks. We assume that firms with outstanding floating liens should be more affected by the law because they were already relying on that collateral to raise external financing. The reform we analyze is essentially forcing these treated firms to scale down relative to the control firms, and the goal of our paper is to measure by how much.

A second assumption is the absence of confounding events surrounding the legal change. At a macro level, Sweden experienced consistent economic growth during our sample period (2000–2006): GDP grew on average by 3.2%, gross investment grew by 4.2% per year, and the main economic sectors experienced steady increases in revenue (see Appendix Fig. 1.2).

It is also important to rule out the possibility that other legislation of relevance for corporate lending was passed or enacted around the same time as the collateral reform we study. We carried out an exhaustive search of legislation that was discussed or enacted between 1998 and 2006 by the Swedish parliament. In particular, we searched all proposals that were dealt with in either the Finance Committee or the Committee on Civil Affairs, using the following keywords: "credit", "loan", "priority rights", "bankruptcy", "collateral", "lien", and "liquidation". None of these proposals or related reports discussed legal changes that might affect differentially our treated and control groups. In Appendix 3 we explain our search methodology and list the most relevant proposals found.

The third assumption is that the treated and control firms would have behaved similarly in the absence of the legal change in question. We select for each treated firm a set of control firms that were established both in the same year and in the same industry (at the 5-digit industry code level). By matching firms on age, we ensure that we control for life-cycle effects, including potential differences in size, growth, and financing structure. Matching firms on industry implies that the treated and control firms should have similar exposures to shifts in demand, productivity shocks, regulatory changes, external shocks, and other aggregate shocks.⁹

Table 3 shows that treated and control firms are substantially different in terms of balance sheet structure and size, even after being matched on industry and age. In particular, the treated firms are larger, more leveraged, hold more long-term debt, and obtain higher credit

⁹ Our sample contains 1303 different industries and we are able to retain 70% of them after the matching procedure. To understand why our high level of industry disaggregation matters, suppose that in a given winter hospitals have low patient numbers due to abnormally good weather conditions. Firms that offer catering services for hospitals (code 55.522) are thus hit by a negative demand shock that could affect their financing and investment decisions. In a difference-in-differences setting, this demand shock might be confounded with the effects of the legal reform and bias our estimates if, for instance, we were to compare these firms with catering firms for schools (code 55.523). Our matching procedure filters out such confounding factors because we compare the effect of the legal reform across two groups of firms that provide catering services for hospitals. Appendix Fig. 1.1 provides further details.

Table 3

Sample means for control and treated firms.

Sample averages are for the period 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Differences in means are assessed with the *t*-test. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variable	Control	Treated	Difference
<i>Collateral</i>			
Total collateral / Debt	0.112	0.609	0.497***
Fixed liens / Debt	0.112	0.179	0.067***
<i>Debt and debt structure</i>			
Debt / Assets	0.534	0.701	0.167***
Long-term debt / Debt	0.208	0.309	0.101***
Long-term loans / Debt	0.075	0.221	0.146***
Short-term loans / Debt	0.009	0.027	0.017***
Lines of credit limit / Debt	0.224	0.316	0.092***
<i>Investment</i>			
Total net investment / Assets	0.016	0.020	0.004***
Investment in machinery and equipment / Assets	0.014	0.017	0.003***
Investment in land and buildings / Assets	0.003	0.003	0.001***
<i>Asset structure</i>			
Tangible assets / Assets	0.186	0.266	0.079***
Inventories / Assets	0.091	0.175	0.083***
Liquid assets / Assets	0.308	0.167	-0.141***
<i>Capital intensity and productivity</i>			
(Machinery + Equipment) / Employees (mSEK)	0.781	0.862	0.081***
Sales / (Machinery + Equipment)	2.736	2.861	0.125***
<i>Firm growth and size</i>			
Employment growth	0.004	0.022	0.009***
Asset growth	0.059	0.080	0.014***
Number of employees	8.50	9.37	0.875**
Assets (log mSEK)	13.94	14.48	0.543***
<i>Bankruptcy</i>			
Firm filed for bankruptcy	0.003	0.003	< 0.001*
Number of firms	80,431	88,055	

lines than the control firms. However, we note that these differences are a direct consequence of our definition of treatment and that they would also emerge even if floating liens were randomly assigned (which we are not claiming to be the case). A firm pledges a floating lien in order to obtain additional loans that can be used to finance additional assets, such as tools and inventories, and to hire more employees. Consequently, the treated firm will be necessarily larger and more leveraged than a control firm even if they were identical *ex ante*.

Since it is not plausible to assume that the effects of our change in the law are the same for firms that did or did not use floating liens beforehand, we rely only on the parallel trends assumption and interpret the estimates obtained from our baseline regressions as average treatment effects on the treated (ATT).

We address the plausibility of the parallel trends assumption by exploiting the time-series variation in our data to investigate the dynamic behavior of the treated and control firms before and after the legal reform. We use these plots to see whether pre-trends are parallel and to assess whether the adjustment of the outcome variables is economically sensible. Moreover, we run augmented specifications of the above regression model that explicitly control for potentially different linear trends between treated and control firms.

In order to address potential concerns that our results might be driven by unobserved differences between the treated and control firms, in a robustness section we show that our results hold when we: (i) define as treated firms those with floating liens already pledged in 2000 (instead of 2000–2003), (ii) match firms on size, profitability, investment rate, and leverage, and (iii) analyze heterogeneous treatment effects by exploiting cross-sectional differences among the treated firms in a triple differences setting. We also discuss that differences in “style”, as in Bertrand and Schoar (2003), explain more than 90% of the variation in floating lien pledge.

5. Results

5.1. Collateral use

In Table 4 we estimate difference-in-differences regressions to quantify the reduction in collateral use resulting from the change in the law.¹⁰ The treatment group contains firms with pledged floating liens before the law was changed, while the control group contains firms with no floating liens pledged before the law change. We present, for both dependent variables, results from three specifications. The first specification uses the entire sample of firms. The second specification uses the matched sample, in which a treated firm is compared with one or more control firms with the same age and 5-digit industry code (e.g., two firms that provide catering services for hospitals). The third specification uses the same matched sample and controls for differences in trends between the treated and the control firms. All three specifications include firm and year fixed effects. The last row of Table 4 displays the predicted percentage change in the dependent variables implied by the difference-in-differences estimates. We compute the predicted percentage change as the *Treated* × *Post-law* coefficient divided by the sample mean of the dependent variable for the treated subsample.

Table 4 presents estimates of the effect of the 2004 law change on both total collateral (which includes floating liens and fixed liens) and fixed liens. We scale both variables by firm debt, but results are similar if we instead scale by assets. The results in columns 1–3 show that the drop in collateral use experienced by treated firms is statistically significant and economically meaningful. For example, the point estimate in column 2 indicates that the treated group reduces, on average, its collateral pledges by 5% following the change in the law. We note that this number underestimates the actual drop in collateral value for lenders, since our collateral measure is the nominal amount pledged and does not account for the loss in seniority faced by floating lien holders.¹¹

In Columns 4–6 we analyze the effect of the 2004 law change on fixed liens, which should not be affected directly. The first two specifications show an increase in the use of fixed liens by treated firms after the law change. This increase in fixed liens may reflect an attempt to compensate for the loss in the value of floating liens by pledging more valuable types of collateral. We note, however, that the increase in the use of fixed liens is no longer significant when we control for differential trends (which we do in column 6). The fact that we do not see a drop in the use of fixed liens confirms that the observed reduction in collateral pledges is mostly driven by floating liens.

5.2. Debt and debt structure

We next investigate how the reduction in collateral capacity affects financial leverage and debt maturity. We provide, in Fig. 1, a graphical snapshot of our main results. The figure plots separately for the treated and the control firms the time series of *Debt/Assets* (top panel) and of *Long-term debt/Debt* (bottom panel).¹² Both graphs show that the two

¹⁰ In Appendix Fig. 5.1 we depict the use of floating liens in Sweden and show that floating liens are economically important as more than 40% of all firms with some collateral outstanding pledge floating liens. After the change in the law floating lien usage falls both across and within firms.

¹¹ Using loan-level data from a large Swedish bank containing timely assessments of these collateral values, Cerqueiro, Ongena and Roszbach (2016) find an average decrease in the collateral coverage ratio of 13%.

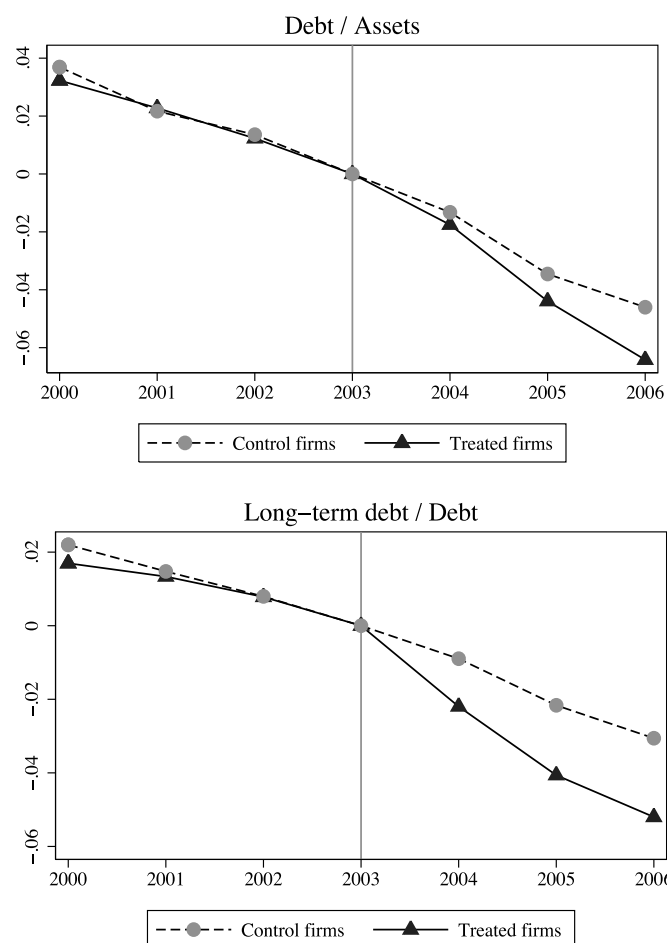
¹² We create these graphs by running separate regressions for the treated and the control firms on a set of year dummies (using 2003 as the omitted year) and controlling for firm fixed effects. The year-by-year point estimates obtained thus denote within-firm changes in the dependent variable relative to 2003. This approach removes differences in levels between the treated and control groups and facilitates the comparison of their dynamic behavior before and after the change in the law.

Table 4

Total collateral and fixed liens.

Total collateral includes floating liens and fixed liens. Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. *Predicted% change* is the $Treated \times Post\text{-}law$ coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Total collateral / Debt			Fixed liens / Debt		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated \times Post-law	-0.0220*** [0.00105]	-0.0295*** [0.00137]	-0.0343*** [0.00141]	0.0163*** [0.000861]	0.0133*** [0.00116]	0.00101 [0.00120]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Matched samples	-	Yes	Yes	-	Yes	Yes
Treated \times Trend	-	-	Yes	-	-	Yes
Number of firms	191,385	167,261	167,261	191,385	167,261	167,261
Number of observations	1,303,505	1,144,023	1,144,023	1,303,494	1,144,016	1,144,016
R-squared	0.003	0.004	0.004	0.002	0.003	0.003
<i>Predicted% change</i>	-3.62	-4.84	-5.64	9.12	7.45	0.56

**Fig. 1.** Effect of the 2004 Law on firm debt and on debt maturity.

We run separate panel regressions for the treated and control firms of the variables shown on a set of year dummies, controlling for firm fixed effects. The figures plot the coefficients obtained for the year dummies (2003 is the omitted year). The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Sample period is from 2000 to 2006.

groups behave identically prior to the 2004 law change, confirming that our control firms provide a good counterfactual. Following the change in the law, the treated firms become less leveraged and borrow more

short term relative to the control firms.

In Table 5 we present the corresponding difference-in-differences estimates. Columns 1–3 focus on the leverage ratio. The coefficient in column 2 indicates that treated firms experience a reduction in their leverage ratio of 1.3% relative to control firms. This estimate remains statistically significant, but it becomes quantitatively smaller when we allow for different linear trends (column 3). The estimates in columns 4–6 indicate larger economic effects for debt maturity. For instance, the estimate in column 5 indicates that the reduction in long-term debt is about 11 percent.

Before the change in the law floating liens were widely used to secure bank credit. For this reason, we also examine the effect of the law change on various types of bank financing: long-term bank loans, short-term bank loans, and limits on lines of credit. The results, which we report in Appendix Table 5.1, show that as a result of the change in the law banks reduce long-term loans and line-of-credit commitments to treated firms. We also find that treated firms compensate for part of the reduction in long-term borrowing and liquidity through a significant increase in short-term borrowing.

The results so far offer a clear pattern. Firms can no longer use floating liens to promise priority to financiers with regard to their moveable property. As a consequence, these firms suffer a reduction in their debt capacity and are forced to borrow more short term to counterbalance the drop both in long-term funding and in access to lines of credit. The observed reduction in long-term debt complements the findings of previous studies by Giannetti (2003), Benmelech et al. (2005), Qian and Strahan (2007), Benmelech and Bergman (2009), and Vig (2013), who find that the strengthening of creditor rights is correlated with longer debt maturity. Next, we investigate whether these changes in corporate debt affect firms' investment decisions and performance.

5.3. Investment and asset structure

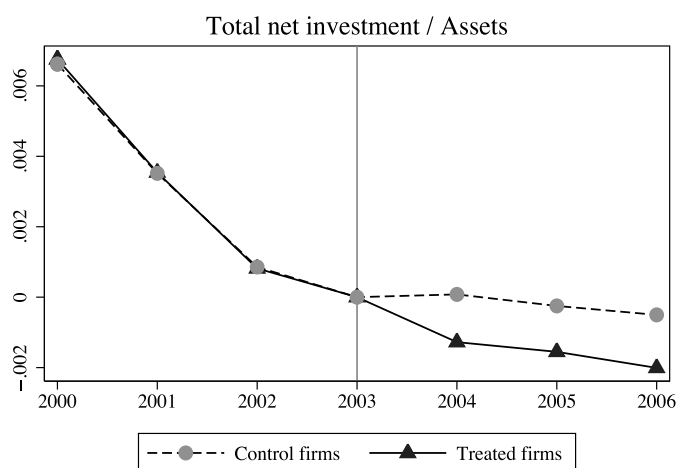
In this section, we investigate the effects of the law change on firm investment and asset structure. We first consider investment. Fig. 2 plots separately for the treated and the control firms the time series of *Total net investment/Assets*. The figure shows that prior to the legal change the two groups displayed identical investment behavior. Following the change in the law, treated firms reduce their investment by more than control firms do, clearly showing that the legal change forced firms that were using floating liens to cut back on investment. The fact that we compare firms that have the same age and that operate in the same industry makes it unlikely that these results are reflecting differential shocks to investment opportunities. Columns 1–3 of Table 6

Table 5

Leverage and debt maturity.

Long-term debt is debt due in one year or more. Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. *Predicted % change* is the $Treated \times Post\text{-}law$ coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Debt / Assets			Long-term debt / Debt		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated \times Post-law	-0.00928*** [0.000809]	-0.00940*** [0.00108]	-0.00528*** [0.00112]	-0.0345*** [0.000929]	-0.0350*** [0.00124]	-0.0151*** [0.00138]
Firm fixed effects	Yes	Yes	Yes			
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Matched samples	-	Yes	Yes	-	Yes	Yes
Treated \times Trend	-	-	Yes	-	-	Yes
Number of firms	190,790	165,992	165,992	191,453	167,315	167,315
Number of observations	1,273,170	1,108,215	1,108,215	1,310,462	1,149,965	1,149,965
R-squared	0.047	0.053	0.053	0.014	0.013	0.014
<i>Predicted% change</i>	-1.32	-1.34	-0.75	-11.17	-11.34	-4.89

**Fig. 2.** Effect of the 2004 Law on firm investment.

We run separate panel regressions for the treated and control firms of the variable shown on a set of year dummies, controlling for firm fixed effects. The figures plot the coefficients obtained for the year dummies (2003 is the omitted year). The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Sample period is from 2000 to 2006.

Table 6

Firm investment.

Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. *Predicted % change* is the $Treated \times Post\text{-}law$ coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Net investment / Assets			Investment in machinery and equipment / Assets			Investment in land and buildings / Assets		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated \times Post-law	-0.0006*** [0.0001]	-0.0014*** [0.0002]	-0.0012*** [0.0003]	-0.0006*** [0.0001]	-0.0010*** [0.0001]	-0.0011*** [0.0002]	0.0001 [0.0001]	-0.0002*** [0.0001]	-0.0003** [0.0001]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched samples	-	Yes	Yes	-	Yes	Yes	-	Yes	Yes
Treated \times Trend	-	-	Yes	-	-	Yes	-	-	Yes
Number of firms	184,700	163,332	163,332	184,746	163,360	163,360	184,735	163,361	163,361
Number of observations	1,101,582	984,189	984,189	1,104,224	986,176	986,176	1,136,470	1,018,512	1,018,512
R-squared	0.011	0.010	0.010	0.011	0.009	0.009	0.001	0.001	0.001
<i>Predicted% change</i>	-2.99	-7.04	-6.09	-3.72	-6.03	-6.22	2.73	-7.68	-9.88

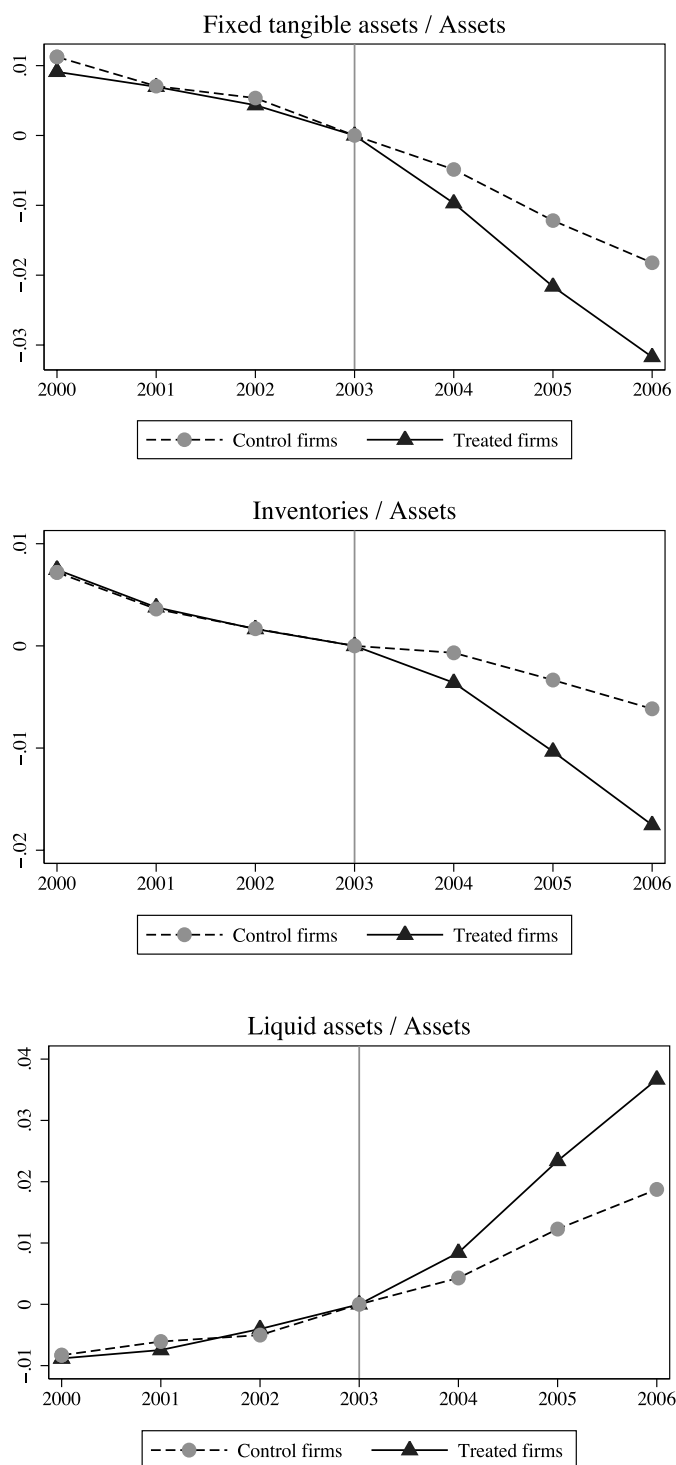


Fig. 3. Effect of the 2004 Law on asset structure.

We run separate panel regressions for the treated and control firms of the variables shown on a set of year dummies, controlling for firm fixed effects. The figures plot the coefficients obtained for the year dummies (2003 is the omitted year). The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Sample period is from 2000 to 2006.

make the ground flat or solid, construction of support walls, [and] dry laying of land". Investments in buildings and land, especially for smaller businesses without large real estate holdings, are thus typically only likely to generate collateralizable assets at a more distant point in time. In addition, long-term credit to treated firms is reduced, thereby typically limiting their funds available for investments (see Table 5). Our results thus indicate that investment decisions are shaped by credit constraints rather than by a potential collateral motive.

Next, we analyze the effect of the law change on asset structure. We distinguish between three types of assets, which we analyze as a proportion of total assets: fixed tangible assets (which include machinery, equipment, buildings, and land), inventories, and liquid assets (cash and equivalents). Fig. 3 plots the time-series averages of these variables for the treated and the control firms, and Table 7 displays the difference-in-differences estimates.

In all three panels of Fig. 3, the lines for treated and control firms track each other closely before the law change. Following the law change, treated firms experience a sharper decrease in tangible assets and inventories, and a larger increase in cash holdings. The decrease in tangible assets corroborates our earlier findings for investment. The estimated coefficient points to an average decline in the fraction of tangible assets of treated firms of 3.2%. In unreported results, we also decompose this effect into the part accounted for by machinery and equipment and the part accounted for by land and buildings, and find that most of the effect is driven by the former. These additional tests corroborate our finding, above, of a sharper drop in investment in machinery and equipment.

The decline in inventory holding is also economically significant. The difference-in-differences estimate in column 5 of Table 7 indicates that the post-law change reduction in inventories is 4.1%. Inventories are an important component of floating liens. Part of this effect, however, may be due to the lower access to credit, which forces treated firms to reduce their working capital requirements, as in Carpenter et al. (1998). The observed increase in treated firms' cash holdings corroborates this view and is consistent with the evidence in Almeida et al. (2004), who find that financially constrained firms save more cash. The difference-in-differences estimates indicate that treated firms increase their holding of liquid assets by 6.7% relative to control firms. It thus appears that treated firms increase the liquidity of their balance sheets to compensate for lower credit availability.

5.4. Capital Intensity, efficiency and profitability

The reduction in collateral capacity decreases firms' ability to borrow and reduces their holdings in movable assets, including machinery and equipment. We now investigate whether there are any changes in the capital intensity and operating efficiency of firms. Finding a decline in capital intensity following the law change would be consistent with Garmaise (2008), who show that financially constrained firms use relatively more labor than physical capital.

Fig. 4 plots—separately for the treated and the control firms—the time series of the value of machinery and equipment per employee (top graph) and of sales per dollar of machinery and equipment (bottom graph). Both graphs show that there are no differential effects of the 2004 law change. The treated and the control firms reduce their capital intensity and increase their productive efficiency at the same rate throughout the period analyzed. In Table 8, we confirm that the effect of the law change on these two variables is both statistically and economically negligible.

In Appendix Table 5.2 we provide supplementary evidence of how the reform affects a range of firm profitability measures and sales growth, such as return on assets, the operating margin, net margin and

Table 7
Asset structure.

Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. Predicted% change is the Treated × Post-law coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Tangible assets / Assets			Inventories / Assets			Liquid assets / Assets		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated × Post-law	-0.0073*** [0.00066]	-0.0084*** [0.00089]	-0.0054*** [0.00092]	-0.0111*** [0.00047]	-0.0072*** [0.00065]	-0.0027*** [0.00067]	0.0134*** [0.00079]	0.0113*** [0.0011]	0.00337*** [0.00126]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched samples	-	Yes	Yes	-	Yes	Yes	-	Yes	Yes
Treated × Trend	-	-	Yes	-	-	Yes	-	-	Yes
Number of firms	193,576	168,472	168,472	193,575	168,471	168,471	193,574	168,471	168,471
Number of observations	1,342,101	1,170,065	1,170,065	1,341,776	1,169,770	1,169,770	1,340,583	1,168,709	1,168,709
R-squared	0.013	0.013	0.013	0.005	0.006	0.006	0.007	0.008	0.008
Predicted% change	-2.75	-3.18	-2.05	-6.37	-4.11	-1.55	8.05	6.74	2.02

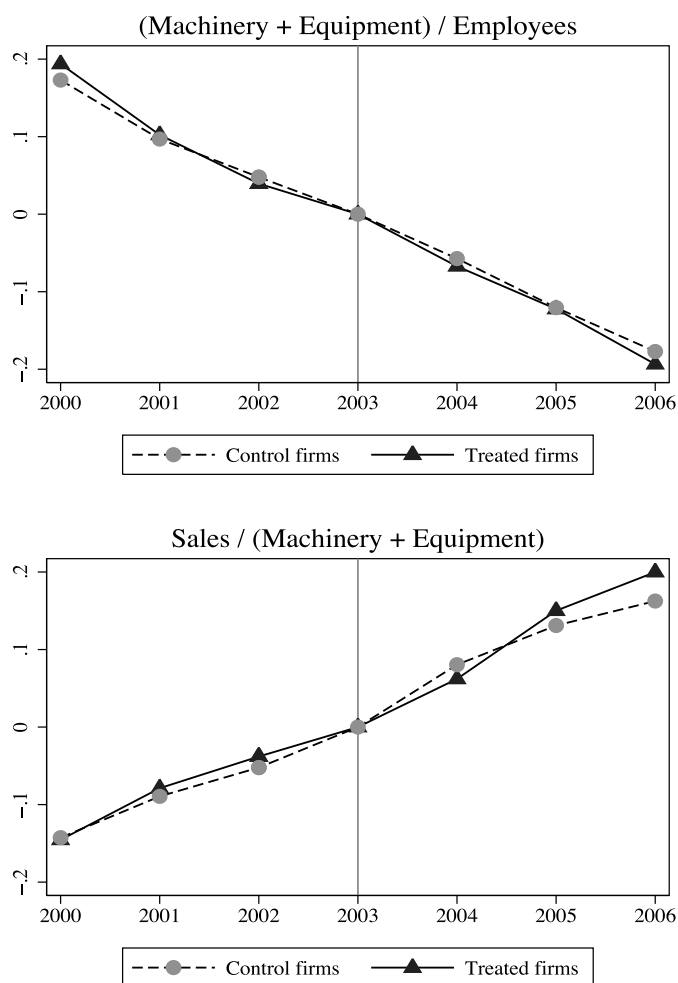


Fig. 4. Effect of the 2004 Law on capital intensity and productivity. We run separate panel regressions for the treated and control firms of the variables shown on a set of year dummies, controlling for firm fixed effects. The figures plot the coefficients obtained for the year dummies (2003 is the omitted year). The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Sample period is from 2000 to 2006.

debt service. Consistent with the notion that scale impacts firm profitability for fixed technology, for example due to the presence of fixed costs, we find that treated firms experience a drop in all five profitability measures following the reform.

Our evidence therefore suggests that following the change in the law treated firms are scaling down the level of their operations rather than changing their production technology, resulting in lower profitability.¹³

5.5. Firm growth

We next investigate the effect of the 2004 law change on firm growth. Fig. 5 displays the time series of the growth rates of both employment (top graph) and assets (bottom graph). We compute the growth rate as the annual change in the logarithm of the variable. Before the law change, the treated firms experience slightly higher growth rates than their control peers. However, growth rates of treated firms plunge relative to the control firms following the change in the law. We assess the economic significance of these effects in Table 9. The difference-in-differences estimates in columns 2 and 5 indicate, respectively, a reduction in the growth rate of employment of 1.8 percentage points and a reduction of 2.7 percentage points in the growth rate of assets. We note that the reduction in the asset growth rate is not only driven by the lower investment rates we documented in Table 6, but also by a reduction in other balance sheet items such as inventories (Table 7). These meaningful effects underline the importance of credit availability for firm growth.

5.6. Did the 2004 law reduce bankruptcy rates?

One of the intentions of the 2004 law change was to reduce creditors’ incentives to liquidate financially distressed companies. The abolishment of their special priority rights effectively reduced the liquidation payoff to lien holders, since after the law change the assets

¹³ In Table 8 we showed that “Sales/Machinery and Equipment” did not change after the change in law, i.e., Sales and M&E respond similarly to the change in law, while ROA (Net earnings/Total assets) has been shown to fall. Since Net earnings are Sales minus Costs and Total Assets are Machinery and Equipment plus (Fixed Assets and Liquid Assets), the fact that ROA falls must be driven by the fact that either costs are not falling proportionately to Sales, for example due to fixed costs, or Fixed and Liquid Assets are not reduced at the same pace as M&E as the firm is shrinking.

Table 8
Capital intensity and efficiency.

Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. Predicted% change is the Treated \times Post-law coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	(Machinery + Equipment) / Employees			Sales / (Machinery + Equipment)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated \times Post-law	-0.0404*** [0.00767]	-0.0137 [0.0105]	0.00830 [0.0121]	0.0368** [0.0181]	0.00369 [0.0243]	-0.0231 [0.0272]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Matched samples	-	Yes	Yes	-	Yes	Yes
Treated \times Trend	-	-	Yes	-	-	Yes
Number of firms	193,051	168,026	168,026	27,228	24,248	24,248
Number of observations	1,299,839	1,135,551	1,135,551	105,352	93,531	93,531
R-squared	0.007	0.007	0.007	0.014	0.017	0.017
Predicted% change	-4.69	-1.59	0.96	1.29	0.13	-0.81

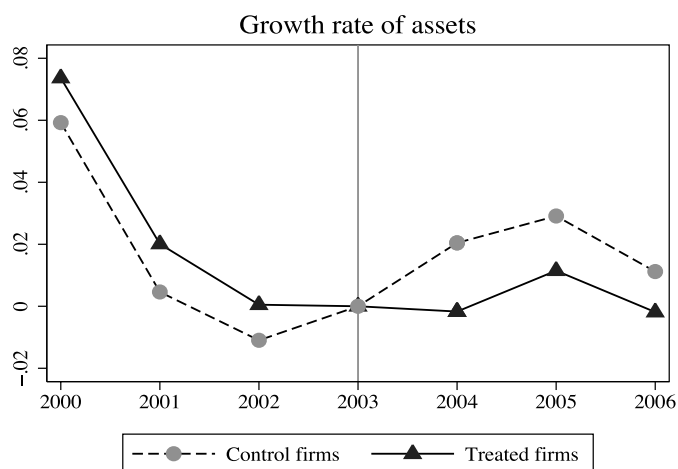
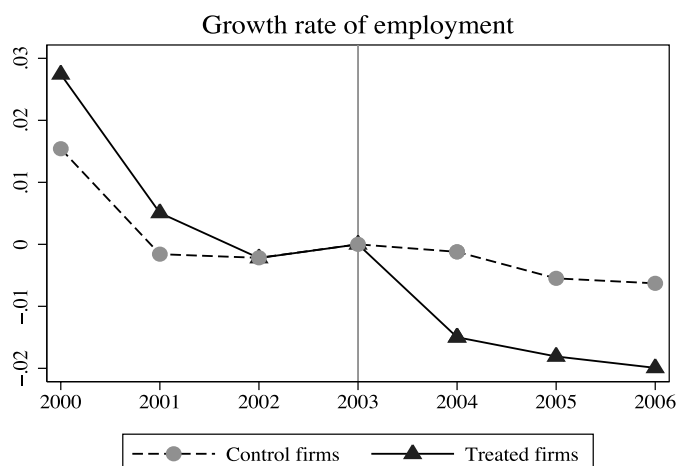


Fig. 5. Effect of the 2004 Law on firm growth.

We run separate panel regressions for the treated and control firms of the variables shown on a set of year dummies, controlling for firm fixed effects. The figures plot the coefficients obtained for the year dummies (2003 is the omitted year). The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Sample period is from 2000 to 2006.

backing a floating lien had also to satisfy other claims, such as the costs of bankruptcy or reorganization procedures, and taxes. On the one hand, this should reduce the ex-ante incentives of floating lien holders to push firms into bankruptcy. On the other, the resulting reduction in credit availability could also mean that firms become more likely to experience financial distress following the law change. How the 2004 law affects bankruptcy is thus an empirical question.

We provide a visual representation of our results in Fig. 6 and display the corresponding regression results in Table 10. The figure shows that the two groups behave identically prior to the 2004 law change, confirming once again that our control firms provide a good counterfactual. For example, the identical pre-trends address the concern that our results might be due to differences in risk. The graph does not clearly show whether treated firms become more or less likely to file for bankruptcy after the change in the law. Although the difference-in-differences estimates in Table 10 indicate a decline in bankruptcy rates, the effects on matched samples are statistically insignificant.

6. Additional results and robustness tests

6.1. Cross-sectional heterogeneity of treatment effects

We analyze heterogeneous treatment effects by exploiting cross-sectional differences among the treated firms in a triple-differences setting. We compare treated firms along four dimensions, all of which are measured in the period 2000–03: leverage ratio, share of long-term debt, share of liquid assets, and asset value (in logs). We convert each of these four variables into a dummy that equals one for firms with above-median values for the particular variable and zero.

We present, in Appendix Table 5.3, the results from triple-difference regressions where the *Treated* \times *Post-law* variable is further interacted with each of the four dummies that indicate whether the firm has an above-median leverage ratio (panel A), fraction of long-term debt (panel B), fraction of liquid assets (panel C), or firm asset size (panel D). We saturate the regressions with firm fixed effects and all possible second-level interactions.

Panel A shows that more leveraged firms were more negatively affected by the 2004 law change. These firms experience a sharper drop in their leverage ratio, fraction of long-term debt, and investment, and lower growth rates. These results are not surprising since firms with more leverage, while riskier, are likely to be more dependent on collateral securing their lending.

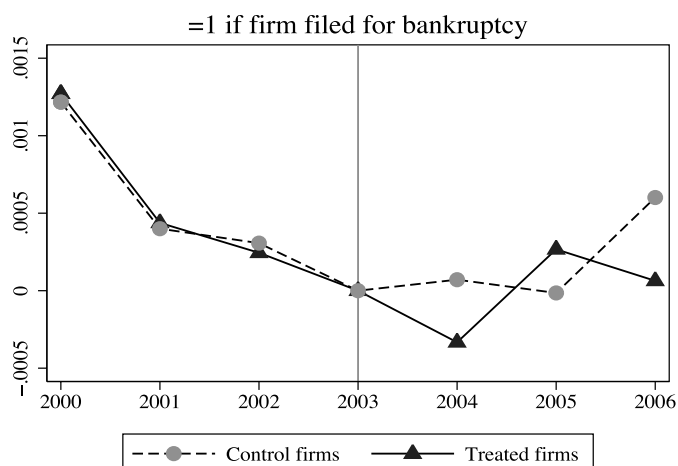
Panel B shows that treated firms that borrow long term are also more negatively affected. Recall that creditors were granted a one-year window to renegotiate contract terms and could demand full repayment

Table 9

Employment and asset growth.

Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. *Predicted% change* is the *Treated* × *Post-law* coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Employment growth			Asset growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-law	−0.0185*** [0.00114]	−0.0180*** [0.00148]	−0.00735*** [0.00252]	−0.0278*** [0.00246]	−0.0278*** [0.00343]	−0.0208*** [0.00687]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Matched samples	–	Yes	Yes	–	Yes	Yes
Treated × Trend	–	–	Yes	–	–	Yes
Number of firms	155,690	137,372	137,372	193,593	168,485	168,485
Number of observations	1,078,953	952,211	952,211	1,322,569	1,154,558	1,154,558
R-squared	0.002	0.002	0.002	0.002	0.001	0.001
<i>Predicted% change</i>	−84.90	−82.74	−33.82	−34.75	−34.77	−25.93

**Fig. 6.** Effect of the 2004 Law on firm bankruptcy.

We run separate panel regressions for the treated and control firms of the variables shown on a set of year dummies, controlling for firm fixed effects. The figures plot the coefficients obtained for the year dummies (2003 is the omitted year). The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. The treated and the control groups are matched exactly on industry (at the five-digit SNI level) and on firm age. Sample period is from 2000 to 2006.

of the loan if, for example, a borrower did not have additional collateral to post (see Section 2 for details). Since firms with a high share of long-term debt tend to have more illiquid asset structures, an unexpected renegotiation or termination of a lending agreement may force some affected firms to scale down more abruptly.¹⁴ Consistent with this view, the results in panel C show that treated firms with higher cash holdings were relatively less affected by the change in the law.

Finally, the results in panel D show that larger firms were more negatively affected. Floating liens can involve substantial fixed costs for lenders in monitoring the asset pool. Lending against floating liens is therefore more attractive towards larger firms that have substantial

¹⁴ Hart and Moore (1994) show that, under limited enforcement, firms match the maturity of their assets and their debts. In cross-sectional regressions that we do not report, we find strong evidence of maturity matching in our sample. In particular, firms that borrow more long-term have a much higher share of illiquid assets, such as fixed assets and inventories, and a lower share of liquid assets, such as cash.

Table 10

Firm bankruptcy.

Sample period is from 2000 to 2006. Post-law is a dummy that equals one in years 2004 to 2006, and equals zero in years 2000–2003. The treated group contains firms that pledged floating liens before 2004. The control group contains firms that did not pledge floating liens before 2004. “Matched samples” refers to the exact matching of treated and control firms on industry (based on 5-digit SNI codes) and on firm age. *Predicted% change* is the *Treated* × *Post-law* coefficient divided by the sample mean of the dependent variable in the pre-treatment period for the treated group, in percent. The standard errors shown in brackets are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Firm filed for bankruptcy		
	(1)	(2)	(3)
Treated × Post-law	−0.000340** [0.000173]	−0.000228 [0.000233]	−0.00010 [0.000446]
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Matched samples	–	Yes	Yes
Treated × Trend	–	–	Yes
Number of firms	193,286	168,330	168,330
Number of observations	1,351,767	1,177,300	1,177,300
R-squared	0.000	0.000	0.000
<i>Predicted% change</i>	−12.22	−8.19	−3.41

amounts of assets. In addition, for a given nominal amount of movable assets, those of larger firms have on average higher book value as collateral; because these larger firms tend to be active in more mature sectors, their assets have higher liquidation value. When the use of nominal floating lien collateral towards banks is restricted by law, we should therefore expect a stronger response of banks’ credit to large firms (and therefore of firm specific real variables) because banks lose more book value collateral for larger firms.

Overall, the heterogeneity of impact the law change had, confirms our general finding that the change negatively impacted upon leveraged, long-term borrowing, or larger firms, while businesses with more liquidity were less affected.

6.2. Log-linear specifications

In the main regressions we express the dependent variables as ratios (as a percentage of total debt or assets). We also analyze the effect of the law change on the same variables using a log-linear model. We

report the results in these regressions, which essentially confirm our main findings and conclusions, in Appendix Table 6.1.

6.3. Definition of treatment

To make assignment to the treatment group more exogenous with regard to the change in law, we instead categorize treated firms as those with floating liens in 2000 (instead of 2000–2003) and other firms as control firms. Appendix Table 6.2 shows that our main results on debt structure, investment and asset growth are similar to those presented earlier.

As an alternative to the exact matching on firm age and 5-digit industry code level, we use a coarsened exact matching algorithm and match treated and control firms on the following variables, averaged for each firm over the 2000–2003 period: Log (Assets) as a proxy for size, Operating margin (EBITDA divided by total sales), Asset growth rate, Net investment/Assets, and Debt/Assets. In Appendix Table 6.3 we show that this matching procedure performs well in making the treated and control group balanced with respect to these covariates. Our main results are unaffected.

To further understand why some firms pledge floating liens while other firms do not, we also ran an additional regression using data from 1999 to 2003 and found that firm fixed effects explain 92% of the variation in floating lien pledge. This suggests that important differences in “style”, as in Bertrand and Schoar (2003), such as risk-aversion or the degree of financial literacy, matter most for firms’ choice between pledging floating liens and other types of collateral.¹⁵ Since we employ firm fixed effects in all our regression, we thereby control directly for such time-invariant differences.

6.4. Collapsing data to a cross-section

Appendix Table 6.4 contains regression results with the data collapsed to a cross-section in which each observation equals the “post” minus the “pre” change in the average level of the outcome variable. This specification addresses the concern in Bertrand et al. (2004) regarding spurious correlations in difference-in-differences models. The results obtained are similar to those presented earlier.

7. Conclusion

In this paper, we establish how movable collateral alleviates financing frictions in the real economy by detailing its importance for the asset and liability structure of firms and its resulting impact on investment and growth. For this purpose, we exploit a unique legal reform in Sweden that reduces the fraction of assets that lenders secured by floating liens are entitled to when a firm is liquidated. Since this legal reform redistributes collateral rights among different creditors without affecting either the quantity or the value of the borrowers’ total assets, our setting provides exogenous variation in a firm’s collateral capacity while keeping constant the size of its balance sheet. By assessing how this zero-sum transfer of collateral rights affects the credit supply, corporate financing, and investment, we take a step forward in identifying the role that collateral plays in the real economy.

Using a panel of all incorporated firms in Sweden, we find that the seniority structure and distribution of collateral rights are quantitatively important in supporting corporate debt capacity, and that this legal reform has a “collateral-damaging” effect on businesses. We present new evidence that reveals the channels through which firms’ ability to pledge movable collateral affects their overall debt capacity, debt structure, asset and liability composition and thereby investment,

growth, and performance. A lessened ability to post movable collateral to banks is shown to reduce both the amount and the maturity of firm debt. This in turn is associated with an increase in the holdings of liquid assets and forces firms to cut back on investment, reduce inventories and employment, and grow at a slower rate.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jfi.2019.05.001.

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¹⁵ In Cerqueiro et al (2016) we were able to use very granular collateral data to explicitly create a control group that used other collateral than floating liens. When studying the universe of Swedish businesses, the collateral data is less granular and we cannot do an equally good match on the collateral variable.