

Conflicting Security Laws and The Democratization of Credit: France's Reform of the Napoleonic Code*

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Abstract

We exploit contradictions in a recent security law reform in France (*Ordonnance 2006-343*) to study how access to collateral shapes the composition of corporate debt, the demographics of corporate debt usage, and capital allocation efficiency. *Ordonnance 2006-343* modernized France's 200-year old security law system and facilitated asset pledgeability in a country with a highly unequal access to corporate debt. Despite this, it was undermined by "non-codified laws," introduced in the 1980s due to concerted lobbying by big firms wishing to pledge their liquid assets. Using a triple-differences strategy, we find that firms with the highest demand for hard assets and little access to factoring companies increased debt financing the most after the reform (intensive margin) and that the fraction of "zero-leverage" firms among them dropped from 89% to 29% (extensive margin). Using contract-level data, we find that hard assets allowed for significant reductions in loan mark-ups and increases in debt maturity after the reform. The effects stem from the use of secured debt and affect only private firms. Small, profitable, low risk firms and start-ups benefitted the most from the reform. Our geographical analysis shows that the reform benefitted rural firms, leading to a pronounced drop in the Gini-index of debt concentration. The reform also improved capital allocation efficiency. All together, our results suggest that the reform led to a welfare-enhancing "corporate debt democratization."

Key words: Security Laws, Contractibility, Collateral, Capital Structure, Social Welfare.

JEL classification: G32, K22, O16.

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Abstract

We exploit contradictions in a recent security law reform in France (*Ordonnance 2006-343*) to study how access to collateral shapes the composition of corporate debt, the demographics of corporate debt usage, and capital allocation efficiency. *Ordonnance 2006-343* modernized France's 200-year old security law system and facilitated asset pledgeability in a country with a highly unequal access to corporate debt. Despite this, it was undermined by "non-codified laws," introduced in the 1980s due to concerted lobbying by big firms wishing to pledge their liquid assets. Using a triple-differences strategy, we find that firms with the highest demand for hard assets and little access to factoring companies increased debt financing the most after the reform (intensive margin) and that the fraction of "zero-leverage" firms among them dropped from 89% to 29% (extensive margin). Using contract-level data, we find that hard assets allowed for significant reductions in loan mark-ups and increases in debt maturity after the reform. The effects stem from the use of secured debt and affect only private firms. Small, profitable, low risk firms and start-ups benefitted the most from the reform. Our geographical analysis shows that the reform benefitted rural firms, leading to a pronounced drop in the Gini-index of debt concentration. The reform also improved capital allocation efficiency. All together, our results suggest that the reform led to a welfare-enhancing "corporate debt democratization."

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1 Introduction

Economic policymakers often promote reforms meant to reduce financial market discriminations under the banner of “democratizing access to credit.” But while credit availability is believed to spur growth (LaPorta et al. (1998) and Djankov et al. (2007)), several examples point to the risk of extending credit to society’s marginal borrowers. White (2007) shows that the expansion of credit card use in developed economies caused a spike in personal defaults in the late 1980s and 1990s. More recently, Mian and Sufi (2009) and Keys et al. (2010) argue that the government-backed expansion of housing credit in the U.S. contributed to the subprime crisis.

A commonly-used approach towards stimulating credit expansion is that of strengthening creditor rights. Often, creditor rights are strengthened by changing laws and shaping the judicial system so as to boost creditors’ ability to seize collateral in default states. Examples of such policies include the setting of asset recovery tribunals as in India (Vig (2013)) and the lifting of loan recovery (collateral repossession) restrictions as in Brazil (Assunção et al. (2014)). Policies that focus on enforcing recovery can, however, bear adverse distributive consequences, such as the expansion of credit to riskier borrowers (Lilienfeld-Toal et al. (2012) and Assunção et al. (2014)). The adverse consequences should not be surprising. Riskier borrowers are those who attach the least value to productive assets in liquidation. Thus, they are, on the margin, more likely to demand the newly-extended credit supply than less risky borrowers.

While the emphasis of existing research and policy work has been on boosting control rights and enforcement, it is hard to argue that giving more power to one party or to increase court involvement leads to more contracting. Theory suggests that the scope of the contracting space and information available to agents determine the terms of the contracts that they sign (Aghion and Bolton (1992)). Rarely do we see credit reforms emphasizing these two dimensions. However, a recent reform in France, *Ordonnance 2006-346*, allowed creditors and borrowers to contract upon a larger number of collateral options, while accidentally leaving collateral seizure options in default unchanged. *Ordonnance 2006-346* allows us to study the mechanisms through which collateral laws influence credit access and capital allocation — without being exposed to confounding effects originating from changes in the balance of power between creditors and borrowers, in collateral seizure options, or in the enforcement of laws.

In France, access to credit was long concentrated in the hands of a few large firms located

in the larger cities. For example, as recently as 2005, 88% of the firms in the country held no long-term debt in their balance sheets and the average Gini index of long-term debt concentration was around 0.95. *Ordonnance 2006-346* derogated the possessory nature of asset ownership, in existence under the Napoleonic Civil Code since 1804. As such, it allowed for the creation of security interests over types of assets which could not previously be pledged: primarily hard assets used in modern production processes. The consequences of such changes were far-reaching and transformed the contractual space. The new law allowed for security interests to be charged to more than one creditor, making it feasible for loans to be syndicated under multiple creditors, with multiple priorities and maturity structures.

Changing the nature of asset ownership meant that firms were discretely endowed with assets that could be used as collateral in debt transactions, facilitating the contracting of credit facilities. Yet the new law did not affect firms equally. Long before *Ordonnance 2006-346* was brought to the political debate, big French companies located in the country's financial centers had successfully lobbied for "non-codified" security laws that allowed them to liberally pledge distinct types of assets as collateral. Among the types of assets that these laws allowed firms to use as collateral were liquid securities, cash and equivalents, and accounts receivables (two examples of such laws are the "Daily Law" and the "Pledge of Ready Money").

As it turns out, the distortions inherent in the process in which France reformed its credit laws are unique and allow us to exploit several wrinkles in understanding the distributional and wealth effects of credit expansions. Our first identification strategy relies on the insight that firms operating more heavily with hard assets ("high-fixed assets firms") are more affected by the reform (treatment group) than those operating more heavily with liquid assets ("low-fixed assets firms"; control group). Our second identification strategy relies on the insight that firms depend on the services of a factoring company to pledge their liquid assets. Thus, we expect firms registered in areas in which there are no factoring companies and which thus have a hard time to pledge their liquid assets (treatment group) to more readily use the new opportunity to pledge their hard assets than firms registered in areas in which there are factoring companies (control group). We contrast French firms not only along the types of assets they utilize, but also according to the nature of the credit they access (e.g., secured vs. unsecured, long-term vs. short-term), their stage in the life cycle (e.g., start-ups), and their geographical location (e.g.,

distance from France’s financial centers). We are able to investigate the amount of debt that firms use, the type of debt, the distribution of debt utilization across firms, and how capital allocation efficiency is affected by changes in the collateral regime.

As a starting point, we analyze the effect of *Ordonnance 2006-346* on firm-level (quantity) and contract-level (price) debt data within a triple-differences framework. Our evidence implies that the reform changed the amount of corporate debt held by private French firms. The mean total debt-to-assets ratio increases from a pre-reform average of 10.1% to a post-reform average of 14.4%. Arguing that short-term debt is usually secured by liquid and not hard assets, we show that the reform only affected long-term debt-taking. In addition to debt levels (the intensive margin), we also examine the propensity of firms to have debt (the extensive margin). Before 2006, 89% of firms have no long-term leverage (“zero leverage firms”). During the reform year alone, this figure halves and drops to 33% at the end of our sample period.

Using fixed-assets intensity as identification strategy, our evidence confirms that the increase in debt-taking and the decrease in the proportion of zero leverage firms occurs primarily among high-fixed assets firms — those firms whose asset pledgeability was most pronouncedly affected by the reform. For example, while the long-term leverage of high-fixed assets firms increases by 7.8 percentage points, the long-term leverage of low-fixed firms increases by only 2.8 points. In contrast, short-term leverage does not change across treated or control firms. Using the presence of factoring companies as identification strategy, our evidence confirms that the increase in debt-taking and the decrease in the proportion of zero leverage firms occurs primarily among firms located far away from factoring companies — those firms that had the hardest time to pledge their liquid assets before the reform. For example, firms located in areas in which there are no factoring companies observe a one percentage point greater increase in their long-term leverage after the reform than firms located near factoring companies.

Next, we use detailed loan data to examine whether the reform led to changes in contract terms. We assign to the treatment (control) group those loans that are directly affected by the reform, that is, secured (unsecured) loans. Results show that secured loans become less expensive and with longer maturities compared to unsecured loans after the reform. Specifically, mark-ups of secured loans drop by 120 basis points relative to the mark-ups of unsecured loans (compared with a pre-reform mean of 213 basis points). In addition, the maturity of secured

loans increases by around 41 months relative to unsecured loans (the pre-reform mean is 74 months). Thus, our empirical evidence suggests that banks became more willing to extend loans at more favorable terms after the passage of *Ordonnance 2006-346*.

The next step in our analysis is to investigate whether the credit expansion led to credit democratization. To do so, we perform four alternative tests. We first look at the characteristics of firms that enter the corporate debt market following the reform. In particular, we identify a sub-sample of firms that never holds long-term debt before the reform. Within this sub-sample, we compare firms that borrow in every post-reform year (“debt switchers”) with those that keep a zero long-term leverage policy over the entire period (“non-switchers”). On average, debt switchers are smaller, more cash-strapped, more profitable, and more capital-intensive firms. They are also located further away from metropolitan areas. At first sight, it seems that the reform benefitted borrowers that were previously unlikely to receive credit.

We take our analysis one step further and test whether the reform eased access to credit for a class of firms that the prior literature suggests to be particularly exposed to credit constraints: start-ups. Our evidence shows that the average long-term leverage of start-ups incorporated after 2006 is 1.5 percentage points higher than the average long-term leverage of start-ups incorporated before 2006. Moreover, the proportion of start-ups with no long-term debt in their year of incorporation decreases from 90% to 68% after the passage of the reform. Thus, our results suggest that the reform made prospective entrepreneurs more willing to come into credit markets and compete for funds with incumbents firms (Bertrand et al. (2007)).

The third test is a geographical analysis of the reform’s effects. Several studies emphasize the importance of a firm’s geographic location for financial decisions. A high density of financial institutions, in particular around capital market centers in urban areas, makes the access to information on firms less expensive and so the access to credit easier. As such, firms located in rural areas tend to be discriminated against. We employ Departmental-level data to investigate the geographical effects of the collateral reform. Consistent with the idea that the reform led to credit democratization, we find that firms located farther away from the main French metropolitan areas benefitted the most from the reform. We also use the Gini index to test whether the reform reduced inequalities in long-term debt usage. We find that, before the reform, 90 out of 96 Departments have a Gini index of long-term debt concentration exceeding 0.90, suggesting

that debt markets in those Departments are heavily dominated by a handful of firms. After the reform, the average Gini index drops to 0.70, with only seven French Departments registering values above 0.80. Most notably, the largest drops occur in rural Departments.

A key remaining question is whether the increase in credit access has significant real-side implications. In the final part of the paper, we investigate the real effects of the reform both at micro and macro levels. Our results show that high-fixed assets firms raise their investment more significantly than low-fixed assets firms after *Ordonnance 2006-346*. They also hire more workers. Interestingly, however, it seems firms hire cheaper labor. Our evidence also suggests that the greater increase in capital and labor enable high-fixed assets firms to pursue more profitable and less risky investments. This is an important result from an economic welfare perspective because we show that the “better” investments ultimately contribute to reductions in corporate failure rates. Notably, while high-fixed assets firms are more likely to fail than low-fixed assets firms before 2006, this relationship is reversed after 2006.

Turning to the real effects of the reform at the macro level, we use two tests. First, we follow Wurgler (2000) and use the sensitivity of capital investment to the growth in value-added as proxy for the efficiency of capital allocation. We find that the efficiency proxy jumps significantly from a pre-reform level of 0.43 to a post-reform level of 0.70. Second, we use the external financing dependence proxy of Rajan and Zingales (1998) as alternative capital allocation efficiency proxy. This proxy captures an industry’s technological demand for external funding. To wit, if the economic system operates efficiently, capital should be directed towards those sectors that rely more on external financing. Our results suggest that the collateral reform raises long-term borrowing more strongly in industries that are highly dependent on external financing. Thus, the reform-induced changes in the collateral regime not only created value at the firm level but, ultimately, also improved capital allocation efficiency in the economy.

Our paper contributes to the Law and Finance literature on different fronts. Most previous studies investigate cross-country heterogeneity in bankruptcy regimes (LaPorta et al. (1998), Djankov et al. (2007), and LaPorta et al. (2008)) or changes in some characteristics of the bankruptcy regime (Lilienfeld-Toal et al. (2012) and Vig (2013)). While interesting, the evidence in Haselmann et al. (2009) suggests that such changes are likely to matter less for bank lending than changes in the collateral regime. We add to this literature by providing evidence

on the distributional effects of changes in the collateral regime that lead to more credit democratization, allowing small, profitable, less risky, start-up firms and firms located in rural areas to be less discriminated against in debt markets. We also show that this credit democratization has real-side implications at both the micro-level and the macro-level.

Our paper is also related to the literature linking credit conditions and economic development. On one hand, several studies provide evidence that credit expansion leads to waves of default and repossessions (White (2007), Mian and Sufi (2009), Keys et al. (2010), and Assunção et al. (2014)). On the other, a few studies show that credit expansion leads to more business formation and investment (Chatterji and Seamans (2012) and Campello and Larrain (2014)).

Our article is organized as follows. Section 2 gives details about the French reform. Section 3 describes our data and the identification strategy. Section 4 analyzes the effect of the reform on debt financing. Section 5 identifies those firms that most benefitted from the reform. Section 6 includes the geographical analysis of the reform. Section 7 reports the analyses on the real-side implications of the reform. Section 8 includes robustness tests. Section 9 concludes.

2 Institutional Setting: Security Laws in France

2.1 The Napoleonic Code of 1804

Until recently, security interests in France were governed by the 1804 Napoleonic Civil Law Code. The Code recognized two forms of security interests, the “hypothec” and the “charge.” A hypothec could only be taken out over real property (land and fixtures), whereas a charge could be taken out over all types of assets.¹ Security laws focused on security interests over land — security interests over other assets were deemed as of little importance (Omar (2007)).

The Napoleonic Code was predicated on the premise that borrowers had to be protected from exploitive creditors, while creditors had to be protected from borrowers refusing the surrender of collateral in default (Ancel (2008)). To this end, regulators established a highly formal and costly procedure to create security rights. They also refused to recognize rechargeable hypothecs (which could lead to conflicts about priority) or non-possessory charges (which make it easier for borrowers to not surrender collateral). Ruling out non-possessory charges implied that

¹See Boughida et al. (2011) for more details about French security interests.

security interests over fungible assets or future assets could also not be allowed because such assets cannot be physically transferred to creditors. To further protect borrowers, regulators explicitly forbade out-of-court seizures of collateral in default states.

The Code also failed to promote the creation of a security interest registry covering assets other than land. To compensate for this deficiency, various local registries emerged over time, each covering a narrow range of assets and each restricted to local borrowers. Creditors had to travel to these registries to make inquiries about a firm (*World Bank* (2007)).

2.2 Reforming the Napoleonic Code: *Ordonnance 2006-346*

In the early 2000s, French politicians became increasingly aware of the fact that French security laws were seen as less competitive than the security laws of other countries, especially those of the so-called common law countries (Renaudin (2013)). Thus, in March 2005, President Jacques Chirac called upon the Minister of Justice to reform these laws. In April 2005, a working party set up by the Minister of Justice submitted recommendations on how to reform security laws and in March 2006 the French parliament enacted a sweeping reform.

The reform introduced substantial changes. First, by establishing non-possessory charges and security interests over fungible assets and future assets, it expanded the scope of assets over which security interests could be taken out (Herbet and Sabbah (2006) and Ancel (2008)). Second, the reform established an electronic public registry which covers all types of assets. Third, the reform lowered the costs of creating security interests. For example, it reduced notary fees, taxes, and registration costs associated with creating hypothecs. The reform further lowered transaction costs by allowing security interests to be rechargeable to the same or different creditors, either simultaneously or consecutively (Herbet and Sabbah (2006)).

The reform also aimed at strengthening creditor rights by lifting the ban on out-of-court seizures of collateral in default situations. However, because of a failure to modernize insolvency laws alongside collateral laws, insolvency laws still freeze the right to seize collateral out-of-court once a firm files for bankruptcy, which it is legally obliged to do when it defaults. Thus, despite other intentions, the French reform failed to achieve this objective.

Figure 1 provides an overview of the main differences between the 1804 security law regime and the security law regime introduced in March 2006.

Security rights ...	1804 Security Laws	2006 Security Laws
... can be non-possessory.	No*	Yes
... can be rechargeable.	No	Yes
... can grant super-priority.	No	No
... can be taken out over fungible assets.	No	Yes
... can be taken out over future assets.	No	Yes

Figure 1. Characteristics of the Old and the New French Security Law Regime This table compares the old French security law regime based on the 1804 Napoleonic Civil Law Code with the new regime based on *Ordonnance 2006-346*, enacted on 23 March 2006. The asterisk indicates that the Napoleonic Civil Law Code allowed for non-possessory security rights over real property (land and fixtures).

2.3 Undermining the Reform: Non-Codified Security Laws

Corporate managers had long recognized that the 1804 security laws were outdated. Thus, in the 1970s and 1980s, concerted lobbying by large firms in big French cities spurred new security laws, which were not added to the Napoleonic Code. Of these “non-codified laws,” the most popular were the Dailly Law and the Pledge of Ready Money. The Dailly Law allowed firms to assign their receivables to financial institutions for security purposes, while the Pledge of Ready Money allowed them to assign their cash and cash equivalents for similar purposes. Both laws were remarkably modern insofar as they permitted borrowers to take out security interests over fungible- and future-assets and granted super-priority rights to creditors in bankruptcy.² Their major limitation was that their usage was restricted to liquid corporate assets.

Davydenko and Franks (2008) report that, before the recent reform, around three quarters of the collateral used by French firms was “cash collateral” (i.e., cash, guarantees, and accounts receivables); whereas three quarters of the collateral used by German and U.K. firms was “hard collateral” (i.e., real estate, buildings, and machinery and equipment).

²Super-priority rights allow secured creditors to extract assets out of the estate of a defaulting borrower before bankruptcy proceedings begin. Secured creditors are able to do so because, in legal terms, assets covered by super-priority rights are not the property of the borrower, but of the secured creditors.

3 Methodology and Data

3.1 Methodology

We use a DID (or DIDID) methodology to identify the reform’s effects. We study both firm-level and debt contract-level data. We employ two different identification strategies in the firm-level analysis. The first identification strategy exploits the insight that, while hard assets could not be efficiently pledged before the reform, liquid assets could be. Thus, the reform is expected to have more significantly affected firms operating with more fixed assets than firms operating with more liquid assets. The second identification strategy exploits the insight that firms require the services of a factoring company to pledge liquid assets. Thus, the reform is expected to have more significantly affected firms located far away from factoring companies because these firms had a hard time to pledge either hard or liquid assets before the reform.

To implement the first identification strategy, we start by comparing averages of outcome variables of interest across high- and low-fixed assets firms over the pre- and the post-reform periods. In doing so, we first calculate the simple mean of the outcome variable by fixed assets quartile and year. For each fixed assets quartile, we average the yearly mean of the outcome variable over the pre-reform and the post-reform period and compute the difference. We then take the difference between the differences generated by the highest and lowest quartiles to obtain the DID estimate of the reform’s effect on the outcome variable.

To verify that our outcomes are not driven by differences in observables other than fixed assets, we also run the mean comparisons on a propensity-score (PS) matched sample. To create the matched sample, we use a LOGIT model to estimate the probability that a firm has a fixed assets-to-total assets ratio in the top quartile. We estimate the LOGIT model for each two-digit SIC industry using only data from the year directly preceding the reform (2005). As exogenous variables, we use *Size*, *Profitability*, *Age*, and *TotalLeverage*.³ Using the fitted probabilities, we match each firm in the top fixed assets-to-total assets ratio quartile with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile-firm. We require the absolute difference in fitted probabilities to not exceed 0.1%. Using the PS-matched sample of treated firms and controls, we repeat the mean comparisons.

³We describe the exogenous variables in Section 3.3.1.

While the mean comparisons are intuitive, resulting estimates are possibly biased due to firm-, industry-, and year-fixed effects. Thus, we also estimate multivariate models including such effects. The multivariate models can be written as:

$$Y_{i,t} = \alpha_i + \alpha_k + \alpha_t + \beta Post_t \times Treated_i + \mathbf{X}_{i,t}\gamma + \varepsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ is the value of the outcome variable for firm i in year t , with $Y_{i,t} \in \{TotalLeverage, ShortTermLeverage, LongTermLeverage, DummyNoTotalLeverage, DummyNoShortTermLeverage, DummyNoLongTermLeverage, Growth, Employees, AverageWage, Sales, Profitability, ProfitVolatility\}$. We define these variables in the next subsection. $Post_t$ is a dummy variable that equals one for year 2006 onwards and zero otherwise. Using the first identification strategy, we set $Treated_i$ equal to $Treated_i^{FixedAssets}$, a dummy variable equal to one if firm i 's mean fixed assets-to-total assets ratio is in the top quartile and else zero. Using the second identification strategy, we set $Treated_i$ equal to $Treated_i^{Factor}$, a dummy variable equal to one if firm i is registered in an area in which there are no factoring companies and else zero. \mathbf{X} is a vector of firm-specific controls. α_i , α_k , and α_t are firm-, industry-, and year-fixed effects, respectively, where industry fixed-effects are based on two-digit SIC codes. We cluster standard errors at the firm level. The β coefficient in Equation (1) can be interpreted as the regression-based DID estimate after accounting for the control variables and the fixed effects.

We also combine the two identification strategies and run multivariate regressions containing both treatment variables. These models can be written as:

$$\begin{aligned} Y_{i,t} = & \alpha_i + \alpha_k + \alpha_t + \beta Post_t \times Treated_i^{FixedAssets} \times Treated_i^{Factor} \\ & + \delta Post_t \times Treated_i^{FixedAssets} + \gamma Post_t \times Treated_i^{Factor} + \mathbf{X}_{i,t}\eta + \varepsilon_{i,t}. \end{aligned} \quad (2)$$

where β , δ , γ , and η are free parameters.

We use a model analogous to model (1) in the loan-level analysis:

$$Z_{i,j,t} = \alpha_k + \alpha_t + \beta Post_t \times Treated_{i,j,t}^{Secured} + \mathbf{X}_{i,t}\delta + \mathbf{W}_{i,j,t}\gamma + \varepsilon_{i,j,t}, \quad (3)$$

where $Z_{i,j,t}$ is the value of the outcome variable for loan j taken out by firm i in year t , with $Z_{i,j,t}$

$\in \{LoanCost, LoanMaturity\}$. $Treated_{i,j,t}^{Secured}$ is a dummy variable that equals one for secured loans and zero for unsecured loans. In addition to the firm-specific controls, this model also includes loan-contract specific controls, contained in the vector $\mathbf{W}_{i,j,t}$.⁴

3.2 Data

We use data from several different sources. Our primary source of financial data is Bureau van Dyck’s AMADEUS Top 250,000 database. AMADEUS contains comprehensive data on both public and private firms from 35 European countries that satisfy a certain size threshold.⁵ We collect data on French firms as well as on firms in other important European civil law countries (Belgium, Italy, Spain, and Portugal). We use the non-French data to conduct placebo tests. We exclude firms in the financial (SIC Code 600–699), services (700–899), utilities (490–494), and public administration (900–999) sectors. Our main tests exclude public firms because these hardly ever borrow secured. We later run a falsification test on public firms.

To do the loan-level analysis, we merge the AMADEUS database with the LPC-Dealscan database. To do so, we manually match the Dealscan name of French borrowers who took out a loan in the 2001–2009 period (mnemonic “BorrowerIssuerName”) with the AMADEUS firm name (“CompanyName”). We succeed in doing so for about 90% of all borrowers. We only keep revolver and term loans in this analysis because revolver and term loans are most reflective of pricing terms and credit restrictiveness (Campello and Gao (2014)).

We collect data on the addresses of French factoring companies from both AMADEUS and BANKSCOPE (another Bureau van Dyck product). We use the United Nations’ General Industrial Statistics (UNIDO) database (INDSTAT-4) to gather information on gross fixed capital formation and value added for 124 four digit-*ISIC* manufacturing industries. Finally, we use COMPUSTAT data to create an industry-level external financial dependence index.

⁴As is customary in the loan contracting literature, we do not use firm-fixed effects because roughly 70% of the loan observations correspond to firms that contribute only one single observation to the sample.

⁵For France, the database includes all companies that meet at least one of the following criteria: (1) revenues of at least €15 million, (2) total assets of at least €30 million, and (3) at least 200 employees.

3.3 Variable Construction

3.3.1 Firm-Level Analysis Variables

To examine the effects of the reform on firm debt, we use as outcome variables total, long-term, and short-term leverage. *TotalLeverage* is the sum of short-term debt (loan) and long-term debt (ltdb) divided by total assets (toas).⁶ *ShortTermLeverage* is defined as short-term debt divided by total assets, and *LongTermLeverage* as long-term debt divided by total assets. We further define three dummy variables, *DummyNoTotalLeverage*, *DummyNoShortTermLeverage*, and *DummyNoLongTermLeverage*; these equal one if the corresponding leverage variable is equal to zero. We define *FixedAssets* as fixed assets (fias) divided by total assets.

To study real-side effects, we use as outcome variables growth, employees, the average wage, sales, profitability, and risk. *Growth* is the change in intangible fixed assets (ifas) plus the change in inventories (stok), both from the prior fiscal year end to the current, divided by the average of total assets over the two fiscal year ends. *Employees* is the log number of employees (emp). *AverageWage* is total wages divided by the number of employees. *Sales* is the log of sales; *Profitability* is earnings before interest and taxes (ebta) divided by total assets; and *LossDummy* is a dummy variable equal to one if *Profitability* is negative, else zero. *ProfitVolatility* is the standard deviation of *Profitability* over the most recent four fiscal years (including the current).

Our models use standard controls, including *Size* and *Age*. *Size* is the log of total assets. *Age* is the log of the difference between the current year and the year of incorporation. Additional variables considered in our tests include *Cash*, defined as cash reserves (cash) divided by total assets, and *CapitalToLabor*, defined as the log of the ratio of tangible fixed assets (tfas) to the number of employees. Continuous variables are winsorized at the first and last percentiles.

Our analysis also uses the availability of factoring companies per French postal code and the physical distance between each sample firm and the biggest French cities or the Paris Bourse. To calculate these variables, we use resources from geonames.org to find the longitude and latitude associated with each French postal code, including those of the centers of the ten largest cities. Using the AMADEUS and BANKSCOPE postal code variables, we assign longitudes and latitudes to each sample firm (including the factoring companies). *NoFactoringCompanies* is a dummy variable equal to one if there are no factoring companies in the postal code area in

⁶The items in parentheses are the AMADEUS variable mnemonics.

which a firm is registered and else zero. To calculate a firm’s distance to the closest of the five (*Distance5*) or ten (*Distance10*) biggest cities and its distance to the Paris Bourse (*Distance-Bourse*), we calculate the spherical distance between points i and j using:

$$DIST_{i,j} = \arccos(\text{deglatlon}) \times r, \quad (4)$$

where deglatlon is given by: $\cos(LAT_i)\cos(LGT_i)\cos(LAT_j)\cos(LGT_j) + \sin(LGT_i)\sin(LGT_j) + \cos(LAT_i)\sin(LGT_i)\cos(LAT_j)\sin(LGT_j)$, LGT_x and LAT_x are the longitude and latitude of point x , respectively, and r is the radius of the earth (Coval and Moskowitz (2001)).

3.3.2 Loan-Level Variables

Our loan-level analysis uses loan mark-ups and maturity as outcome variables. *Spread* is the log of the sum of a loan’s coupon and annual fees scaled by its nominal value minus the six month LIBOR rate, in basis points. *Maturity* is the log of the difference between the loan’s maturity date and its initiation date, in months. We use *Secured*, a dummy variable equal to one for secured loans and else zero, to assign firms to treatment. Following Chava and Roberts (2008), Roberts and Sufi (2009), and Campello et al. (2011), our control variables are size, age, profitability, total leverage, the existence of a credit rating, the credit spread, the term spread, loan size, loan type, and the other endogenous variable. *Rating* is a dummy variable that equals one if the firm taking out the loan is rated, else zero. *CreditSpread* is the mean monthly difference between the yield of a corporate bond index and the yield of a long-term government bond index taken over the current year. *TermSpread* is the mean monthly difference between the yield of a long-term government bond index and the yield of a short-term government bond index taken over the same period. *LoanSize* is the log of the notional loan value; *LoanType* is a dummy variable equal to one for term loans and zero for revolver loans.

3.3.3 Industry-Level Variables

To study capital allocation efficiency, we first use four-digit ISIC industry-level data for French manufacturing sectors. *InvestmentGrowth* is the natural log of the ratio of current gross fixed capital formation to its one year-lagged value; *ValueAddedGrowth* is the natural log of the ratio

of current value added to its one year-lagged value.⁷ Both variables are deflated using the U.S. GDP deflator. Second, we create an external financial dependence index similar to Rajan and Zingales (1998). Following Larrain (2014), we define the index as the median ratio of capital expenditures net of cash flows from operations to total capital expenditures for public U.S. firms in each three-digit SIC industry averaged over the 1975-2005 period.

4 Financial Outcomes

4.1 Debt Usage

We first examine how the reform affected firms' debt levels and propensity to have debt. For the sake of brevity, the mean comparisons only use fixed assets as identification variable; the multivariate tests use both fixed assets and the availability of factoring companies.

4.1.1 Total Debt

Figure 2 shows the mean total leverage ratios of the firms in the different fixed assets quartiles over our sample period. The figure suggests that the security law reform led to a pronounced upward jump in total debt-taking in the reform year. Interestingly, the effect of the reform is monotonically increasing in the use of fixed assets in the production process: the higher the use of fixed assets, the larger is the increase in debt-taking after the reform.

To rule out that firm-specific differences drive the patterns in Figure 2, Table 1 uses the PS-matched sample to study the reform's effect on total leverage. Column (1) shows that the pre-reform mean total leverage ratio of the sample firms hovers around 10%, without trending upward or downward. During the reform year alone, however, this magnitude shoots up to nearly 14%, and it continues to increase to 15% in the last year of our sample period, 2009. A comparison between average leverage ratios before and after the reform shows an increase of 4.3 percentage points. This increase is equivalent to 43% of mean total leverage before the reform,

⁷Gross fixed capital formation is defined as the cost of new and used fixed assets minus the value of sales of used fixed assets, where fixed assets include land, buildings, and machinery and equipment. Value added is defined as the value of shipments of goods (output) minus the cost of intermediate goods and required services, with adjustments made for inventories of finished goods, work-in-progress, and raw materials.

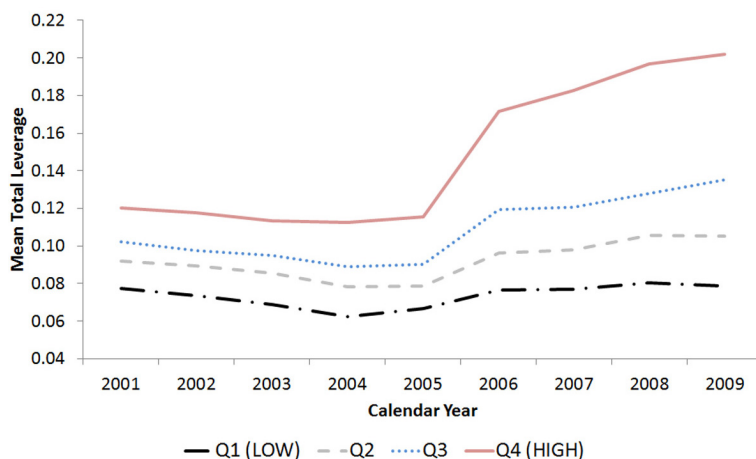


Figure 2. Evolution of Mean Total Leverage This figure shows the evolution of the mean total leverage ratio by fixed asset quartile (Q) over the 2001–2009 period. Q1, Q2, Q3, and Q4 indicate the first, second, third, and fourth fixed assets quartile, respectively.

and it is thus an economically meaningful magnitude.

TABLE 1 ABOUT HERE

In the next columns, we separately consider the fixed assets-intensive treated firms and the matched controls. This exercise confirms that the increase in total leverage is entirely driven by firms that make heavy use of fixed assets. While neither high- nor low-fixed assets firms display systematic changes in total leverage before the 2006 reform, the high-fixed assets firms exhibit far greater jumps in total leverage in the reform year. A comparison of total leverage ratios before (10.2%-11.2%) and after (16.3%-19.1%) the reform shows that the high-fixed assets firms observe a 7.2 percentage points increase in total leverage, while the matched low-fixed assets firms observe a 2.2 points increase. The 5.1 percentage points spread, the DID estimate of the collateral reform on total leverage, is highly statistically significant. Because the difference is equal to 68% of average pre-reform total leverage, it is also economically meaningful.

4.1.2 Short-term versus Long-term Debt

Next, we split total leverage into short-term and long-term leverage. We do so because short-term debt is usually secured using liquid (rather than hard) assets, and it is thus not expected to be particularly affected by the reform. In line with this reasoning, Figure 3 shows that the

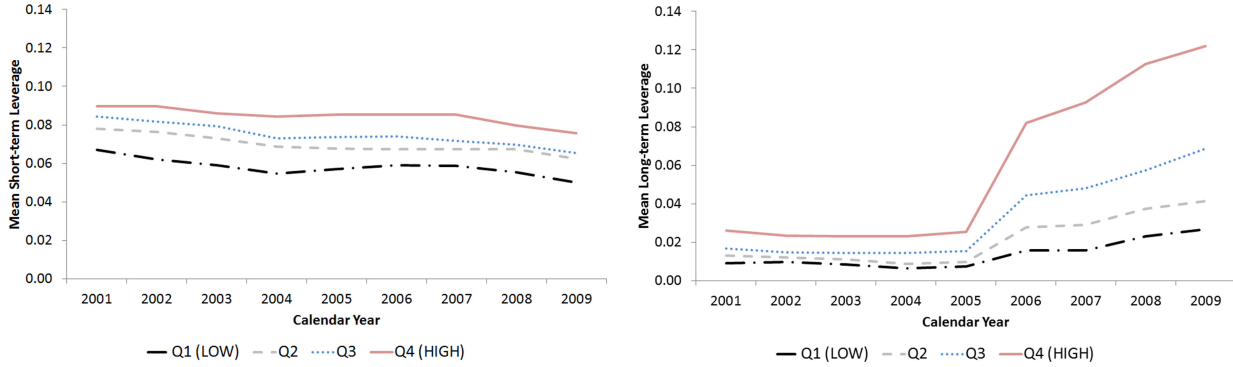


Figure 3. Evolution of Mean Short-term vs. Mean Long-term Leverage This figure shows the evolution of mean short-term and mean long-term leverage by fixed assets quartile (Q) over the 2001–2009 period. Q1, Q2, Q3, and Q4 indicate the first, second, third, and fourth fixed assets quartile, respectively.

mean short-term leverage ratios of all fixed assets-quartiles remain fairly stable over our sample period. In contrast, we see upward jumps in mean long-term leverage in the reform year, with their magnitude monotonically increasing over the fixed assets quartiles.

Table 2 repeats the former analysis using the PS-matched sample. While short-term leverage drops by around 3.8 percentage points over the 2001–2009 period, the drop is not concentrated in specific years. Also, the high-fixed assets firms and low-fixed assets firms observe similar drops in short-term debt from the pre-reform to the post-reform period. To be specific, high-fixed assets firms observe a drop of 3.9 percentage points, while low-fixed assets firms observe a 3.8 points drop. The 0.1% difference, the DID estimate for the effect of the reform on short-term leverage, is neither statistically nor economically significant.

TABLE 2 ABOUT HERE

In contrast, the patterns observed in long-term leverage closely resemble those observed in total leverage. Mean long-term leverage remains almost flat at 1.7% until the reform, but climbs to an average of 6.7% after the reform. Conditioning on different fixed assets-levels, we find that high-fixed assets firms experience far larger increases in long-term debt than low-fixed assets firms. While the mean long-term leverage of firms in the high fixed assets group increases by 7.8 percentage points, the low fixed assets group firms observe an increase of only 2.8 points. The 5.0 points DID spread is highly statistically significant. Since this spread is a striking 293% of average pre-reform long-term leverage, it is also highly economically significant.

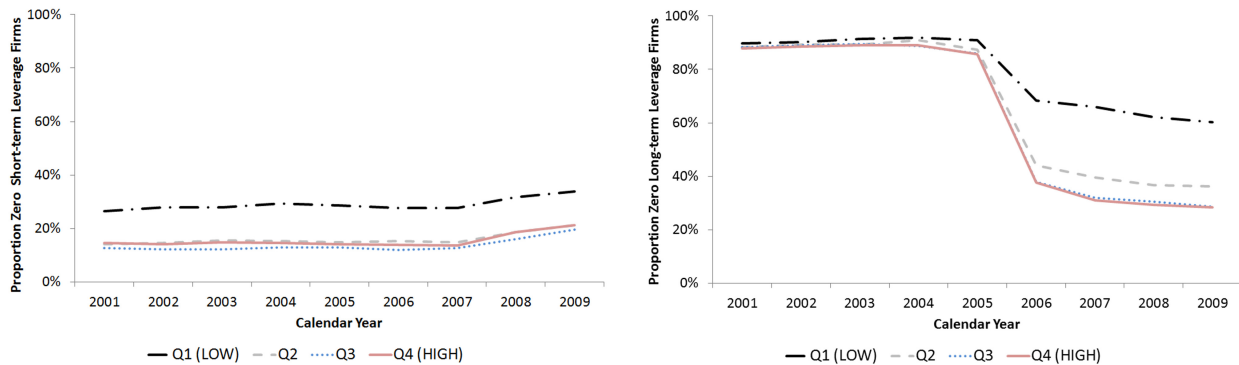


Figure 4. Evolution of the Proportion of Zero Short-term vs. Zero Long-term Leverage Firms
 This figure shows the evolution of the proportion of zero short-term and zero long-term leverage firms by fixed assets quartile (Q) over the 2001–2009 period. Q1, Q2, Q3, and Q4 indicate the first, second, third, and fourth fixed assets quartile, respectively.

4.1.3 Zero Leverage Firms

We next study the reform’s effect on the distribution of short-term and long-term debt. To do so, we consider the proportion of firms with no short-term debt or no long-term debt (“zero-leverage firms”). Figure 4 suggests that the proportion of zero short-term debt firms in the different fixed-assets quartiles is low and stable over our sample period. In contrast, there are profound drops in the proportion of zero long-term leverage firms in the reform year, with the magnitude of the drops monotonically increasing over the fixed-assets quartiles.

Repeating the analysis using the PS-matched sample, Table 3 confirms that the proportion of zero short-term debt firms is stable over our sample period, both for the whole sample and the treated- and control-firm subsamples. Accordingly, the DID estimate of the reform’s effect on the proportion of zero short-term debt firms is insignificant (1.2%).

In sharp contrast, the proportion of zero long-term debt firms is close to 90% before the reform, but it is cut in half (down to 42%) in the reform year and continues to decline to 33% in 2009. As before, high-fixed assets firms are responsible for this pattern. In particular, while the proportion of zero long-term debt firms in the low-fixed assets group drops by 46 percentage points, the corresponding drop in the high-fixed assets group is much larger (60 points). The DID estimate for the effect of the reform on the proportion of zero long-term leverage firms across different fixed assets usage firms is statistically and economically significant (–14 points).

TABLE 3 ABOUT HERE

4.1.4 Regression Results

While the results in Tables 1 to 3 are interesting, they could potentially be driven by unobservable firm- or industry-characteristics or macroeconomic time-variant effects. To rule out this possibility, Table 4 shows the results of DID regressions using fixed-assets intensity as identification strategy and including both control variables and firm-, industry-, and year-fixed effects. In the first three columns, we use as outcome variables total, short-term, and long-term leverage, respectively. In the next three columns, we use dummy variables equal to one if total, short-term, or long-term debt is zero and else zero (“zero-leverage firms”), respectively. Even after accounting for the effects of the controls and the fixed effects, results are essentially identical to before. The controls are also highly significant with the expected signs.

TABLE 4 ABOUT HERE

As an alternative, we repeat the former DID regressions using the availability of factoring companies as identification strategy. Table 5 shows that the effects of the reform are more pronounced in areas in which there are no factoring companies. For example, we find a one percentage point larger increase in long-term debt-taking and a 12.2 points greater drop in the proportion of zero long-term leverage firms among firms located in these areas.

TABLE 5 ABOUT HERE

Finally, we estimate DIDID regressions combining the above identification strategies. Table 6 shows that the reform’s effects on debt levels are multiplicative. In the presence of factoring companies, high-fixed assets firms observe a 3.4 percentage points greater increase in their long-term debt than low-fixed assets firms. Also, the long-term debt of low-fixed assets firms located in areas with no factoring companies increases by 0.6 points more than the long-term debt of low-fixed assets firms located in areas with factoring companies. However, high-fixed assets firms located in areas with no factoring companies observe a 5.4 percentage points greater increase in their long-term debt than low-fixed assets firms located in areas with factoring companies, which is 1.4 points larger than the sum of the former two effects. In contrast, the

reform’s effect on the proportion of zero long-term debt firms are not multiplicative.

TABLE 6 ABOUT HERE

The regression results confirm that *Ordonnance 2006-346* led firms operating with more fixed assets and being located further away from factoring companies to significantly raise their long-term borrowing (intensive margin). In addition, it lowered the fraction of zero long-term debt firms among these firms (extensive margin). In contrast, neither low-fixed assets firms nor firms located closer to factoring companies raised their long-term borrowing, and the proportion of zero long-term debt firms dropped less significantly among them.

4.2 Debt Contracting Terms

Next, we study whether debt contract terms changed following the reform. To this end, we focus on the pricing and maturity terms of secured (treated) and unsecured (control) loans before and after the reform. The DID regressions in Table 7 show that secured loans become significantly cheaper and carry longer terms compared to unsecured loans after the reform, independent of whether we control for firm characteristics. In column (1), the log all-in-drawn spread of secured loans decreases by 1.29 more than the spread of unsecured loans, implying a 120 basis points greater reduction in loan mark-ups. The estimations in the table also suggest that the maturity of secured loans increases by 41 months more than the maturity of unsecured loans — a very significant time window. The estimates on the controls are consistent with those found elsewhere (e.g., Chava and Roberts (2008)). Overall, our evidence implies that the reform made banks more willing to extend secured credit on more favorable terms.

TABLE 7 ABOUT HERE

5 Which Firms Were Most Affected By the Reform?

5.1 Characteristics of Debt Switchers and Non-Switchers

Previous work shows that sweeping collateral reforms often only benefit certain types of firms, generating additional distortions in access to credit (Lilienfeld-Toal et al. (2012) and Assunção et al. (2014)). Thus, as a next step, we examine which type of firms experienced the greatest increases in borrowing due to the reform. We compare the attributes of those firms that most significantly increased their borrowing following the reform with the attributes of (i) the average firm and (ii) those firms that did not increase their borrowing. We focus on firms that have no access to credit before the reform: the “marginal borrowers” brought into credit markets by the reform. To this end, we focus on the subsample of firms with zero long-term leverage in every pre-reform year. We split these firms into two groups, one including those firms that have a positive long-term leverage ratio in every year after the reform (“debt switchers”) and another one including firms that keep a zero long-term leverage ratio every year after the reform (“non-switchers”). To study the attributes of the average firm, we look at the whole sample of firms, including those with and without long-term debt before the reform.

Comparing debt switchers with the average firm, Table 8 shows that debt switchers tend to be smaller and more mature. On average, they also employ around four fewer people than the average firm. Remarkably, their profitability tends to exceed the profitability of the average firm by 0.9 percentage points, and their profit volatility tends to be 0.6 points lower than the profit volatility of the average firm. They are also 4.3 percentage points less likely to run a loss than the average firm. The former findings are a first indication that outdated collateral laws can distort the capital allocation process. Debt switchers tend to hold less cash, have higher capital-to-labor ratios, and are more tangible-assets and fixed-assets intensive. Also, they tend to be located further away from the largest French cities and the Paris Bourse.

TABLE 8 ABOUT HERE

Comparing debt switchers with non-switchers, we again find that debt switchers tend to be smaller, but slightly more mature firms. However, in contrast to the former comparisons, debt switchers employ an average of around 20 more people than non-switchers. Also, they tend to

have a 1.5 percentage points higher profitability and a 1.2 points lower profit volatility than non-switchers and are less than half as likely to run a loss (6.6% vs. 13.9%, respectively). Debt switchers hold less cash, have higher capital-to-labor ratios, and are more tangible-assets and fixed-assets intensive than non-switchers. Notably, they again tend to be located farther away from the five or ten biggest French cities and from the Paris Bourse.

Overall, our evidence shows that the collateral reform incentivized creditors to extend long-term financing to young, profitable, low-risk firms. One could argue that these are the better marginal corporate borrowers. Our evidence also suggests that the firms that benefitted most from the reform were located far from the big cities, implying that the reform extended the reach of credit markets deeper into the interior areas of the country.

5.2 The Financing of Start-Ups

Many studies argue that access to capital fosters entrepreneurship. For example, Bertrand et al. (2007), Kerr and Nanda (2009), and Chatterji and Seamans (2012) find that deregulation of the banking sector or credit card markets leads to more new business formations. They argue that these findings are driven by deregulation leading to a reduction in financial constraints, making prospective entrepreneurs more willing to compete with incumbents firms.

Table 9 shows that French entrepreneurs benefitted from the enhanced access to credit facilitated by the reform.⁸ Before the reform, start-ups have an average long-term leverage of about 2.6% in their year of incorporation. However, this figure increases to an average of 4.1% after the reform. In a similar vein, before the reform, 90% of all start-ups have no long-term debt in their year of incorporation. However, this figure drops to 68% after the reform. Critically, consistent with our prior results, a firm's fixed assets-usage again conditions these results. In particular, the high fixed assets-firms show a 18.7 percentage points greater drop in their proportion of zero long-term debt firms than the low fixed assets-firms.

TABLE 9 ABOUT HERE

⁸Because each start-up only shows up in one sample year (its year of incorporation), we are unable to perform the PS-matching using only data from 2005. Thus, we match the treated- and control-firms by each year in our sample period. Also, because the sample of start-ups is small, we do not match by industry.

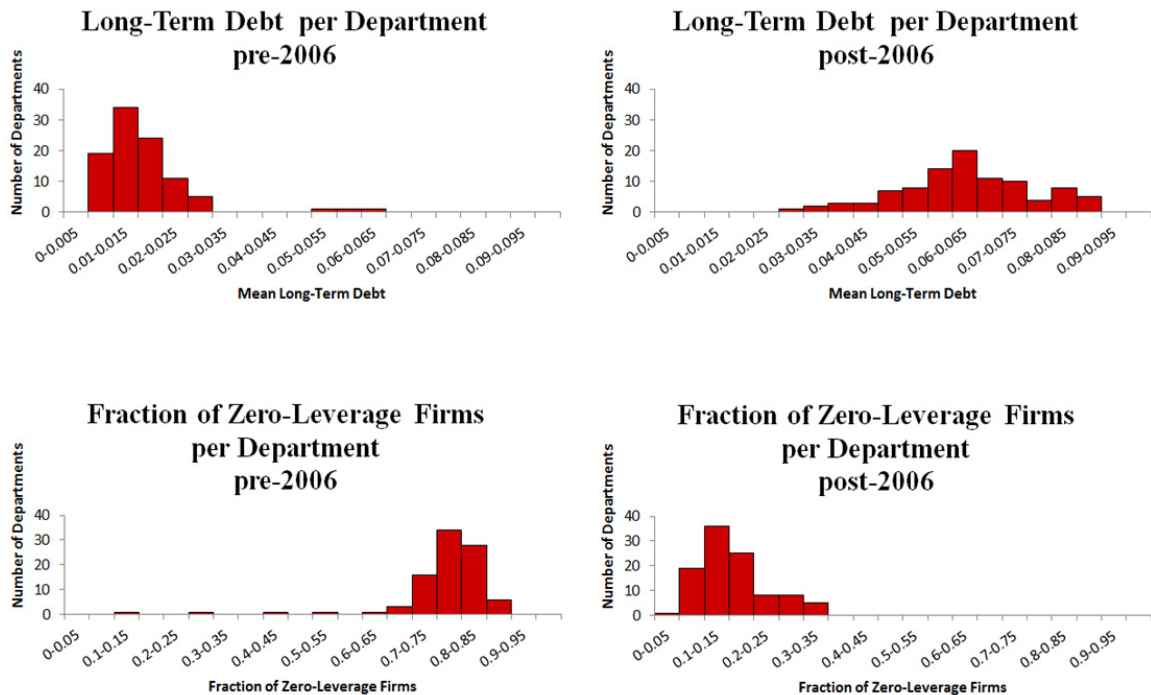


Figure 5. The Long-term Leverage Distribution of French Departments Before and After the Reform The histograms show the distribution of mean long-term leverage and the distribution of the proportion of zero long-term leverage firms for the 96 French Departments separately for the pre-reform and the post-reform period. The histogram entries are the mean of long-term leverage and the proportion of zero long-term leverage firms by Department and year, separately averaged over the pre-reform and the post-reform years.

6 The Geography of the Collateral Reform

6.1 Financing Effects of the Reform Across Departments

Lilienfeld-Toal et al. (2012) find evidence that security law reforms can redistribute credit from firms headquartered in rural areas to those in larger cities. These findings raise concerns about the distributional effects of security law reforms because firms in larger cities have more options to raise external capital (e.g., via equity issuances) than those in rural areas.

Our setting and data allow us to study the geographical effects of the reform. We proceed in two steps. First, we study whether the reform had a general effect on all French Departments, or whether it only affected single Departments. To do so, we calculate mean long-term leverage and the proportion of zero long-term leverage firms by Department and year. For each Department, we then average over the pre-reform- and the post-reform-years. Figure 5 shows histograms of the distributions of Departments' mean long-term leverage (Panel A) and proportions of zero long-term debt firms (Panel B) before and after the reform. Before the reform, the vast majority

of Departments have a mean long-term leverage between 1% and 3% and a mean proportion of zero long-term leverage firms between 60% and 90%. However, these figures change significantly following the reform. The reform shifts the bulk of the mean long-term leverage distribution to the right, so that after the reform most Departments have a mean long-term leverage ratio between 4% and 9%. Similarly, it shifts the bulk of the mean proportion of zero long-term leverage firms distribution to the left, so that after the reform most Departments have a mean proportion of zero long-term leverage firms between 0% and 30%. Thus, the reform seems to have benefitted firms all over France, not just those located in specific French areas.

Next, we study how specific Departments were affected by the collateral reform. In doing so, we proceed as follows. For each Department, we compute the change in mean long-term leverage from before to after the reform. For each Department, we also compute the proportion of firms that never have any long-term leverage before the reform, but that report a positive long-term leverage every year after the reform (“debt switchers”). Figure 6 plots the results of the calculations. The darker (more red) a Department’s color in map, the more the Department is affected by the reform. The yellow-rimmed Departments are those featuring cities with more than one million inhabitants. The maps show that the largest increases in long-term leverage and the largest drops in the proportion of zero long-term leverage firms occur far away from the big cities. Thus, not only did the reform abstain from favoring firms located in big cities, it actually channeled the largest amount of new capital to firms in rural areas.

6.2 Inequalities in Access to Long-term Debt

Our empirical results suggest that the collateral reform produced the largest increase in long-term borrowing among firms that are often expected to have less access to capital markets, such as small firms or firms located in rural areas. In turn, these results suggest that the reform spurred a democratization of credit. We examine this idea further by computing the Gini index measure of concentration for long-term debt at the Departmental level. We do this for the period before and after the reform. The Gini index is computed as follows:

$$GiniIndex_{k,t} = \frac{1}{2\mu_{k,t}N_{k,t}^2} \sum_{i=1}^{N_{k,t}} \sum_{j=1}^{N_{k,t}} |LongTermLeverage_{i,t} - LongTermLeverage_{j,t}|, \quad (5)$$

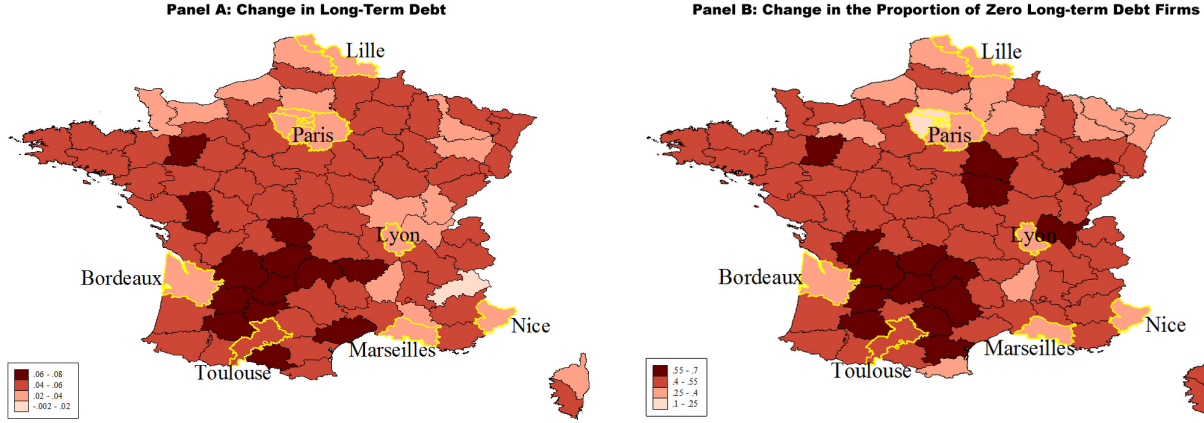


Figure 6. Change in Mean Long-term Leverage and Proportion of Debt Switchers by Department
 The maps show the change in mean long-term leverage from the pre-reform to the post-reform period (Panel A) and the proportion of firms that have no long-term debt in any year preceding the reform, but that have a positive long-term leverage ratio in every year preceding the reform (Panel B) by French Department. Mean long-term debt is long-term leverage, first averaged by Department and year and then by Department and pre-reform or post-reform period. The darker a Department's color in the maps, the greater is the change in mean long-term leverage (Panel A) or the proportion of debt switchers (Panel B). The yellow-rimmed Departments in the maps are those featuring cities with more than one million inhabitants.

where $GiniIndex_{k,t}$ is Department k 's Gini index in year t , $\mu_{k,t}$ is the average long-term leverage of the firms in Department k , i and j are firm indexes, and $N_{k,t}$ is the number of firms. We average $GiniIndex_{k,t}$ by Department and year and then by Department and pre-reform period or post-reform period. The calculated Gini index ranges from zero to one, and a higher value indicates a more unequal distribution of long-term debt usage across firms.

Figure 7 shows each Department's pre-reform and post-reform Gini index values. The darker a Department's color in the maps, the higher is its Gini index. Before the reform, only six out of 96 Departments observe a Gini index below 0.90, suggesting that long-term debt is heavily concentrated in the hands of a few large firms. Following the reform, however, the Gini index drops significantly. Most notably, while all Departments observe significant declines in their Gini index values, it is the rural Departments that experience the most dramatic drops in the index. Overall, the collateral reform greatly decreased inequalities in the access to credit, and it is again the countryside that experienced the greatest improvements.

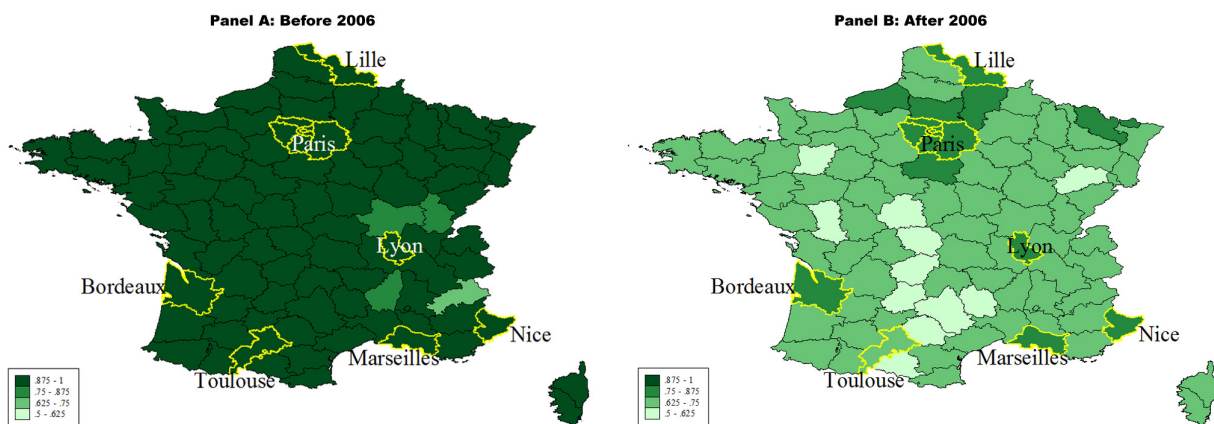


Figure 7. Pre- and Post-Reform Long-term Debt-Gini Index By Department The maps show each Department’s long-term debt-Gini index value for the pre-reform period (Panel A) and the post-reform period (Panel B). To create the Gini index, we calculate it by Department and year and then average by Department and pre-reform- or post-reform-period. The darker a Department’s color in the maps, the greater is its index value. The yellow-rimmed Departments are those containing cities with more than one million inhabitants.

7 Real Outcomes

It is important to study how the firms that increased their long-term borrowing the most after the reform used the new capital that they raised. This is interesting since the greater borrowing could have led to undesirable outcomes, such as a higher corporate default risk. On the other hand, if the firms that increased their borrowing invested in profitable, low-risk projects, the leverage-induced increase in default risk could have been dominated by profitability- and volatility-induced declines in default risk. It is an empirical question how the reform affected the default risk of the firms that observed the largest increases in access to collateral.

7.1 Performance

Table 10 reports the results of DID regressions using as outcome variables investment, employees, the average wage, sales, profitability, and profit volatility. Our evidence shows that the firms that benefitted most from the collateral reform (i.e., high fixed assets-firms) increase investment more and hire more people after the reform. For example, high-fixed assets firms experience an around 5.1 percentage points higher employment growth than low-fixed assets firms. Interestingly, the average wage paid by high fixed-asset firms decreases relative to the average wage paid by low-fixed assets firms, suggesting that the high fixed-assets firms mostly hire cheap labor. In

addition, the high fixed assets-firms raise their output more and become more profitable and less risky than the other firms after the reform. For example, high fixed-assets firms observe a 4.1 percentage points higher sales growth than low-fixed assets firms after the reform.

TABLE 10 ABOUT HERE

7.2 Survival Analysis

In addition to looking at post-reform performance, we are also interested in studying firms' survival rates. Arguably, changes in default risk following the reform are a more direct measure of whether the reform has inadvertently extended credit to risky borrowers at the margin of the capital markets. We use a Cox proportional hazard model to fit the number of years until a firm fails. The Cox proportional hazard model assumes that the hazard rate is given by:

$$\lambda_{i,t} = \phi_t \exp(\beta Post_t \times Treated_i^{FixedAssets} + \mathbf{X}_{i,t}\gamma), \quad (6)$$

where the hazard rate, $\lambda_{i,t}$, is defined as the probability of firm i failing at time t conditional on surviving until this time. We back out the year in which a firm fails using the “legal status” variable in AMADEUS. We set the failure year equal to the calendar year during which a firm’s legal status changes from one of the active statuses to one of the failure statuses (if it does so at all).⁹ ϕ_t is the “baseline” hazard rate common to all firms, and the exponential function allows for cross-sectional variations in hazard. Using a partial likelihood estimator, we are able to obtain estimates of β and γ without imposing any structure on ϕ_t . The same estimator is also able to account for right-censoring of data (i.e., firms that do not fail within the sample period or that leave the sample for reasons unrelated to performance).

In Table 11, we show the results from the Cox proportional hazard models. In the absence of controls, firms with a lot of fixed assets experience a 28.6 percentage points ($e^{-0.338}$) greater decline in their failure rates than other firms after the reform. However, controlling for leverage and thus for the fact that high-fixed assets firms increased their debt levels more than others, the difference in the change in failure rates between high- and low-fixed assets firms widens. Finally,

⁹The failure statuses are: “default of payment,” “insolvency proceedings,” “receivership,” “bankruptcy,” “dissolved (bankruptcy),” “dissolved (liquidation),” and “in liquidation.”

adding profitability and profit volatility more than halves the difference again. The implication is that the high-fixed assets firms' greater increases in profitability and greater declines in profit volatility partially explain why their failure rates decline relative to the failure rates of low-fixed assets firms after the passage of the collateral reform in 2006.

TABLE 11 ABOUT HERE

7.3 Capital Allocation Efficiency

Our previous findings show that the reform made creditors less reluctant to lend to profitable, less risky firms. The ultimate question is whether this lower reluctance led to a more efficient allocation of capital in the economy. We use two approaches to answer this question.

First, we follow Wurgler's (2000) argument that an efficient capital allocation process implies that firm-level investments increase in sectors with better growth opportunities and decrease in sectors with worse growth opportunities. Using industry value added-growth as proxy for growth opportunities, we estimate the sensitivity of capital investment to value added-growth as a way to gauge the efficiency of capital allocation. In particular, we study how this sensitivity changes from the pre-reform to the post-reform period. To do so, we run the following regression on industry-level panel-data from before and from after the reform:

$$InvestmentGrowth_{k,t} = \alpha + \beta ValueAddedGrowth_{k,t} + \varepsilon_{k,t}, \quad (7)$$

where $InvestmentGrowth_{k,t}$ is the percentage change in industry k 's gross fixed capital formation from year $t - 1$ to year t , and $ValueAddedGrowth_{k,t}$ is the percentage change in value-added over the same period. Using pre-reform data, Table 12 shows that the elasticity between investment and value-added (β) is 0.43. Using post-reform data, it becomes 0.70. The difference between the two numbers, 0.27, is statistically significant at the 10% level (t-statistic: 1.79). Based on Wurgler's (2000) interpretation of these elasticities, the reform thus led to a statistically and economically significant increase in capital allocation efficiency.

TABLE 12 ABOUT HERE

Second, we use the external financial dependence measure of Rajan and Zingales (1998) as alternative proxy for capital allocation efficiency. This measure captures an industry’s technological demand for external financing. In an efficient economic system, capital should be directed towards those sectors that are more reliant on external financing. Thus, if the reform relaxed lending constraints, we should observe a significantly larger increase in borrowing among firms operating in sectors highly dependent on external financing. To test this hypothesis, we split our sample into two groups, one including sectors with a high dependence on external financing (proxy value above median) and one including sectors with a low dependence (proxy value below median). Next, we split the firms in the high and low dependence-subsamples into treated firms and control firms, where treatment status is assigned to firms with a mean fixed assets-to-total assets ratio within the top quartile and control status to other PS-matched firms.

Table 13 documents that firms with a high dependence on external financing show greater increases in their long-term leverage from the pre-reform period to the post-reform period than other firms. While the mean long-term leverage ratio of highly dependent firms increases by 6.3 percentage points (column (1)), the mean long-term leverage ratio of firms with a weaker dependence increases by only 3.6 points (column (4)). The difference between the two numbers is highly statistically and economically significant. More importantly, the difference is larger for treated firms. In fact, while highly dependent-high fixed assets-usage firms observe a mean increase in their long-term leverage of 7.5 percentage points, the mean long-term leverage of less dependent-high fixed assets-usage firms increases by only 4.6 points.

TABLE 13 ABOUT HERE

Taken together, our tests suggest that the collateral reform helped France move towards allocating more capital to high-value added sectors as well as sectors that require more external financing. These results point to positive welfare gains achieved by the reform.

8 Robustness

8.1 Self-Selection and Parallel Trends

The validity of DID tests rests on a number of assumptions, of which the most important one is that firms cannot self-select into treatment. In our context, this implies that we need to rule out that firms manage their assets in such a way that they always benefit from the prevailing security law regime. Results in Table 14 suggest that it is unlikely that firms do so. Over the 2001–2005 period, a mere 6.8% of firms moved from any of the lower three fixed assets quartiles into the highest one, which is indistinguishable from the 7.0% that moved into the other direction. Notably, the 2001–2005 migration rates do not differ much from migration rates calculated over other five year-periods within or before our sample period.

TABLE 14 ABOUT HERE

If the random assignment to treatment-assumption is fulfilled, treated and control firms should display parallel trends in outcome variables before the reform. We have verified that this is the case in Figures 1 and 2, in which neither treated nor control firms observe any significant trends in total or long-term leverage during the period preceding the reform. While omitted to save space, additional statistical tests confirm this result (available upon request).

8.2 Autocorrelation in Outcome Variables

Bertrand et al. (2004) show that autocorrelation in the outcome variable can create upward-biased inference levels in DID tests. Following their work, we separately average each analysis variable over the pre- and the post-reform period and repeat the DID regressions using the collapsed data. Results are virtually identical to those reported and are omitted for brevity.

8.3 Placebo Tests

8.3.1 Public Firms

Wood (2007) argues that public firms only borrow secured credit facilities under exceptional circumstances, for example, when they are distressed or when they borrow for project financing

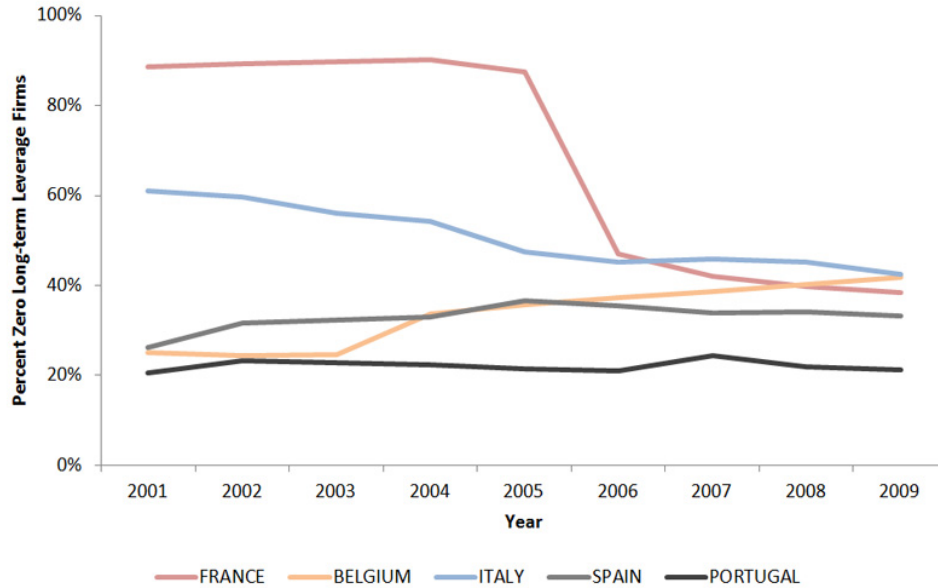


Figure 8. Evolution of the Proportion of Zero Long-term Leverage Firms By Country The line chart shows the proportion of zero long-term leverage firms in France and each of the placebo countries (Belgium, Italy, Spain, and Portugal) over the 2001–2009 period.

purposes. We use this insight to repeat our tests on public firms, expecting that their borrowing is less affected by the reform than that of private firms. Table 15 confirms this intuition. The table shows that, while public firms also experience a slight increase in long-term leverage from the pre-reform to the post-reform period, there is no evidence to suggest that this increase is driven by high-fixed assets firms. If anything, results work in the opposite direction. While firms in the low fixed assets group increase their long-term leverage by an average of 4.4 percentage points, those in the highest fixed assets group decrease it by an average of 5.2 points. Running DID regressions on public firms confirms these conclusions (not reported).

TABLE 15 ABOUT HERE

8.3.2 Other Civil Law Countries

As a final falsification test, we repeat our tests on European civil law countries that did not reform their security laws during the sample period. These countries are Belgium (a neighbor with the same language and close cultural and economic ties), Italy, Spain, and Portugal. Figure 8 uses line charts to graphically illustrate the results of the falsification tests. The figure displays the proportion of zero long-term leverage firms for France and each placebo country

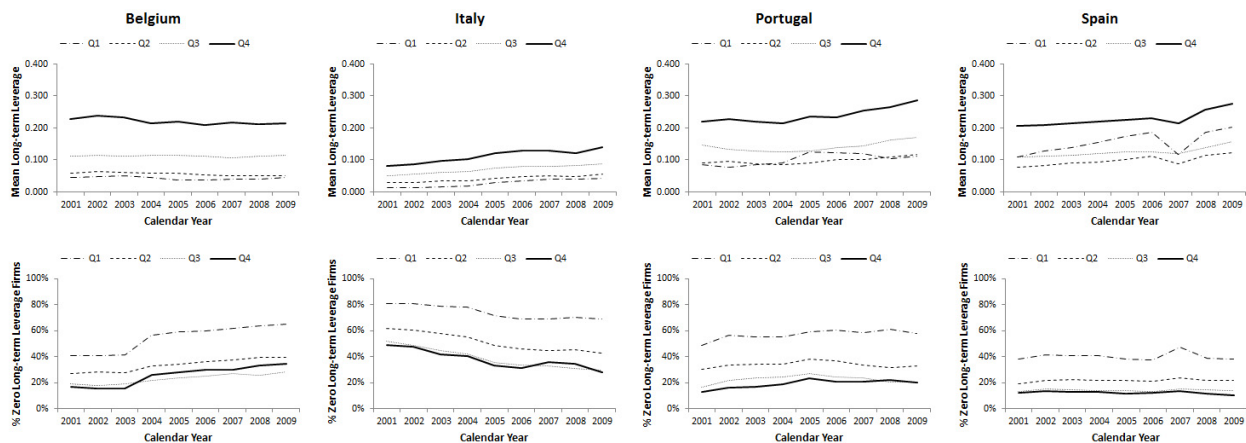


Figure 9. Evolution of Mean Long-term Leverage and the Proportion of Zero Long-term Leverage Firms in the Placebo Countries The figure shows the evolution of mean long-term leverage (upper panels) and the proportion of zero long-term leverage firms (lower panels) for each placebo country and fixed assets quartile (Q) over the 2001–2009 period. The placebo countries are Belgium, Italy, Spain, and Portugal. Q1, Q2, Q3, and Q4 indicate the first, second, third, and fourth fixed assets quartile, respectively.

over our sample period. The most important conclusion that we can draw from the figure is that none of the placebo countries experiences an as stark decrease in the proportion of zero long-term debt firms as France. Also interesting is that the French reform made the proportion of zero long-term leverage firms in France far more similar to those in the other countries, which already had efficient security laws in place at the start of our sample period.

Figure 9 contains charts showing the mean value of long-term leverage and the proportion of zero long-term leverage firms per fixed assets group and placebo country. We again conclude that none of the placebo countries displays effects similar to those found in France. Untabulated DID regressions using data from the placebo countries confirm our inferences.

9 Conclusion

Prior studies show that security law reforms can sometimes raise the volume of corporate borrowing (Vig (2013), Assunção et al. (2014), and Campello and Larrain (2014)). However, raising the volume of corporate borrowing is only the tip of the iceberg. The crucial question is whether such reforms can open up credit to firms that have historically been financially-constrained, that is, small and young firms, start-ups, firms located in the country side. Opening up credit to such firms is crucial since they often contribute most to a country’s value-added.

We offer a more comprehensive analysis of the effects that security law reforms can have

than other studies. We do not only look at the magnitude of debt financing, but also, and more importantly, on the composition of debt, the demographics of corporate debt usage, and capital allocation. To achieve these goals, we consider a security law reform recently implemented by a developed country: France’s *Ordonnance 2006-346*. Looking at a reform in a developed country has the advantage that we have access to more detailed data, allowing us to analyze the reform from more and different angles than previous studies. Our focus on France is also helpful to shed light on the distributional effects of collateral reforms. Up to the recent reform, access to credit was highly unequally distributed across French firms and Departments.

Our evidence shows that the firms expected to be most affected by the reform — firms whose asset composition is heavily tilted towards fixed assets and firms located in areas without factoring companies — significantly increase their long-term leverage from the pre-reform to the post-reform period. Also, the fraction of zero long-term debt firms among them drops from a pre-reform average of close to 90% to a post-reform average of about 30%. In contrast, control firms do not show any particular pattern in long-term debt. Loan contract data suggest that the above credit expansions are driven by cheaper and more long-term secured credit, leading us to conclude that the reform made debt capital more attractive.

Critically, our evidence also speaks to how security law reforms can change the demographics of corporate debt usage. We show that a large amount of the newly available capital went to small, profitable firms located in rural areas. The reform also led to increases in long-term debt usage among start-ups. By opening up access to credit, the reform produced significant decreases in the Gini index of long-term debt concentration all over the country. Overall, our evidence suggests that the reform produced a “democratization” of corporate credit.

Finally, our results suggest that firms used the new debt financing in social welfare-enhancing ways. The firms expected to be most affected by the reform became more profitable, less risky, and less likely to fail than others after the reform. Also, the reform led to a significant increase in the elasticity between investment and value-added and to significantly more borrowing to firms operating in industries heavily reliant on external financing. These findings point to reform-induced improvements in the efficiency of capital allocation in the economy.

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Table 1
Effect of the Reform on Total Leverage: ANOVA Tests

The table shows the mean total leverage ratio for the full sample (“All”) and treated- and control firm-subsamples over the 2001–2009 period. Total leverage (“*TotalLeverage*”) is defined as the sum of short-term and long-term debt scaled by total assets. We create the sample of treated (“High Fixed Assets Group”) and matched control firms (“Low Fixed Assets Group”) using Propensity-score (PS) matching. To do so, we use a LOGIT model to estimate the probability that a firm’s fixed assets-to-total assets ratio is in the top quartile. We run this LOGIT model separately by two-digit SIC industry and using only data from 2005. As exogenous variables, we use *Size*, *Profitability*, *Age*, and *TotalLeverage*. Using the fitted probabilities, we match each top fixed assets-to-total assets ratio-firm with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile firm. We discard the pair if the absolute difference between the firms’ fitted probability values exceeds 0.1%. At the bottom, we report averages over the pre-reform period means (2001–2005) and the post-reform period means (2006–2009) and their differences. The final column shows the differences in means across treated and control firms. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively, based on standard errors that are clustered at the firm level.

Year	All	Fixed Assets Group		Diff.
		High	Low	High–Low
2001	0.105	0.112	0.099	0.013***
2002	0.102	0.109	0.097	0.012***
2003	0.100	0.105	0.096	0.009***
2004	0.097	0.102	0.093	0.010***
2005	0.101	0.102	0.101	0.001
2006	0.138	0.163	0.117	0.047***
2007	0.141	0.173	0.115	0.059***
2008	0.149	0.185	0.122	0.063***
2009	0.150	0.191	0.122	0.069***
Mean 2001–2005 (1)	0.101	0.106	0.097	
Mean 2006–2009 (2)	0.144	0.178	0.119	
Diff. (2)–(1)	0.043***	0.072***	0.022***	0.051***

Table 2

Effect of the Reform on Mean Short-term and Long-term Leverage: ANOVA Tests

The table shows the mean short-term and long-term leverage ratio for the full sample (“All”) and treated- and control-firm subsamples over the 2001–2009 period. The upper number in each row is short-term (ST) leverage; the lower number is long-term (LT) leverage. Short-term leverage (“*ShortTermLeverage*”) is defined as short-term debt scaled by total assets, whereas long-term leverage (“*LongTermLeverage*”) is defined as long-term debt scaled by total assets. We create the sample of treated (“High Fixed Assets Group”) and matched control firms (“Low Fixed Assets Group”) using Propensity-score (PS) matching. To do so, we use a LOGIT model to estimate the probability that a firm’s fixed assets-to-total assets ratio is in the top quartile. We run this LOGIT model separately by two-digit SIC industry and using only data from 2005. As exogenous variables, we use *Size*, *Profitability*, *Age*, and *TotalLeverage*. Using the fitted probabilities, we match each top fixed assets-to-total assets ratio-firm with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile firm. We discard the pair if the absolute difference between the firms’ fitted probability values exceeds 0.1%. At the bottom, we report averages over the pre-reform period attributes (2001–2005) and the post-reform period attributes (2006–2009) and their differences. The final column shows the differences in attributes across treated and control firms. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively, based on standard errors that are clustered at the firm level.

Year	Debt		Fixed Assets Group		Diff.
	Type	All	High	Low	High–Low
2001	ST	0.085	0.087	0.084	0.003
	LT	0.018	0.023	0.014	0.009***
2002	ST	0.083	0.085	0.082	0.003
	LT	0.017	0.022	0.014	0.007***
2003	ST	0.081	0.081	0.081	0.000
	LT	0.017	0.020	0.014	0.007***
2004	ST	0.079	0.079	0.079	-0.001
	LT	0.016	0.020	0.012	0.009***
2005	ST	0.081	0.078	0.084	-0.007***
	LT	0.018	0.022	0.015	0.007***
2006	ST	0.080	0.080	0.080	-0.001
	LT	0.056	0.081	0.035	0.046***
2007	ST	0.078	0.080	0.077	0.002
	LT	0.060	0.091	0.036	0.055***
2008	ST	0.075	0.075	0.075	0.000
	LT	0.072	0.108	0.045	0.062***
2009	ST	0.069	0.070	0.068	0.002
	LT	0.079	0.118	0.052	0.066***
Mean 2001-2005 (1)	ST	0.114	0.114	0.114	
	LT	0.017	0.021	0.014	
Mean 2006-2009 (1)	ST	0.076	0.076	0.075	
	LT	0.067	0.099	0.042	
Diff. (2)–(1)	ST	-0.038***	-0.038***	-0.039***	0.001
	LT	0.050***	0.078***	0.028***	0.050***

Table 3**Effect of the Reform on the Proportions of Zero Short-term and Long-term Leverage Firms: ANOVA Tests**

The table shows the fraction of zero short-term and long-term leverage firms for the full sample (“All”) and treated- and control-firm subsamples over the 2001–2009 period. The upper number in each row is short-term (ST) leverage, the lower number long-term (LT) leverage. Short-term leverage (“*ShortTermLeverage*”) is defined as short-term debt scaled by total assets; long-term leverage (“*LongTermLeverage*”) is defined as long-term debt scaled by total assets. We create the sample of treated (“High Fixed Assets Group”) and matched control firms (“Low Fixed Assets Group”) using Propensity-score (PS) matching. To do so, we use a LOGIT model to estimate the probability that a firm’s fixed assets-to-total assets ratio is in the top quartile. We run this LOGIT model separately by two-digit SIC industry and using only data from 2005. As exogenous variables, we use *Size*, *Profitability*, *Age*, and *TotalLeverage*. Using the fitted probabilities, we match each top fixed assets-to-total assets ratio-firm with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile firm. We discard the pair if the absolute difference between the firms’ fitted probability values exceeds 0.1%. At the bottom, we report averages over the pre-reform period attributes (2001–2005) and the post-reform period attributes (2006–2009) and their differences. The final column shows the differences in attributes across the treated and control firms. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively, based on standard errors that are clustered at the firm level.

Year	Debt Type	All	Fixed Assets Group		Diff. High–Low
			High	Low	
2001	ST	0.153	0.128	0.171	-0.043***
	LT	0.885	0.889	0.882	0.007
2002	ST	0.145	0.116	0.165	-0.049***
	LT	0.890	0.893	0.889	0.004
2003	ST	0.147	0.122	0.165	-0.043***
	LT	0.896	0.899	0.893	0.006
2004	ST	0.153	0.131	0.172	-0.041***
	LT	0.899	0.898	0.900	-0.002
2005	ST	0.142	0.123	0.162	-0.039***
	LT	0.864	0.868	0.860	0.008
2006	ST	0.137	0.117	0.154	-0.037***
	LT	0.421	0.355	0.477	-0.123***
2007	ST	0.144	0.121	0.162	-0.041***
	LT	0.365	0.285	0.428	-0.142***
2008	ST	0.183	0.172	0.191	-0.018**
	LT	0.348	0.274	0.402	-0.129***
2009	ST	0.209	0.193	0.221	-0.028***
	LT	0.334	0.254	0.388	-0.134***
Mean 2001–2005 (1)	ST	0.148	0.124	0.167	
	LT	0.887	0.889	0.885	
Mean 2006–2009 (2)	ST	0.168	0.151	0.182	
	LT	0.367	0.292	0.424	
Diff. (2)–(1)	ST	0.020***	0.027***	0.015***	0.012*
	LT	-0.520***	-0.597***	-0.461***	-0.137***

Table 4
Effect of the Reform on Leverage: DID Regressions Using Fixed Assets as ID Variable

The table shows the results from the following regression:

$$Y_{i,t} = \alpha_i + \alpha_k + \alpha_t + \beta Post_t \times Treated_i^{FixedAssets} + \mathbf{X}_{i,t}\gamma + \varepsilon_{i,t},$$

where $Y_{i,t}$ is either total, short-term, or long-term leverage or dummy variables indicating whether the former quantities are zero. Total (short-term) [long-term] leverage is the sum of short-term and long-term (short-term) [long-term] debt scaled by total assets. Dummy no total (short-term) [long-term] leverage is a dummy variable equal to one if total (short-term) [long-term] leverage is zero and else zero. $Post$ is a dummy variable equal to one for years greater or equal to 2006 and else zero. $Treated^{FixedAssets}$ is a dummy variable equal to one for firms whose mean fixed assets-to-total assets ratio is in the top quartile and else zero. $\mathbf{X}_{i,t}$ is a vector of control variables, including size, profitability, and age. $Size$ is the log of total assets; $Profitability$ is the ratio of earnings before interest and taxes to total assets; and Age is the log of the current year minus the year of incorporation. β and γ are free parameters. α_i , α_k , and α_t indicate firm-, industry-, and year-fixed effects. $\varepsilon_{i,t}$ is the residual. Parameter estimates are in normal letter, while t-statistics are in square parentheses. T-statistics are calculated from standard errors that are clustered at the firm-level. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively. The sample period is 2001 to 2009.

	Dependent Variable									
	Intensive Margin					Extensive Margin				
	Total Leverage	Short-term Leverage	Long-term Leverage	Dummy No Total Leverage	Dummy Short-term Leverage	Dummy No Long-term Leverage	Dummy Short-term Leverage	Dummy No Long-term Leverage	Dummy Short-term Leverage	Dummy No Long-term Leverage
$Post \times Treated^{FixedAssets}$	0.040*** [22.42]	-0.002* [-1.85]	0.044*** [32.53]	0.004 [1.26]	0.006 [1.60]	-0.121*** [-19.27]				
$Size$	0.021*** [16.40]	0.013*** [13.53]	0.008*** [11.88]	-0.071*** [-27.09]	-0.077*** [-26.91]	-0.047*** [-15.45]				
$Profitability$	-0.188*** [-36.29]	-0.127*** [-32.75]	-0.050*** [-16.44]	0.077*** [7.15]	0.095*** [7.60]	0.105*** [7.46]				
Age	-0.002 [-0.85]	0.001 [0.81]	-0.003** [-2.32]	-0.034*** [-7.18]	-0.044*** [-8.02]	0.079*** [11.78]				
R-squared	0.029	0.007	0.058	0.009	0.007	0.210				
Observations	228,782	228,782	228,782	228,782	228,782	228,782				

Table 5
Effect of the Reform on Leverage: DID Regressions Using Lack of Financing Companies as ID Variable
The table shows the results from the following regression:

$$Y_{i,t} = \alpha_i + \alpha_k + \alpha_t + \beta Post_t + Treated_i^{Factor} + \mathbf{X}_{i,t}\gamma + \varepsilon_{i,t},$$

where $Y_{i,t}$ is either total, short-term, or long-term leverage or dummy variables indicating whether the former quantities are zero. Total (short-term) [long-term] leverage is the sum of short-term and long-term (short-term) [long-term] debt scaled by total assets. Dummy no total (short-term) [long-term] leverage is a dummy variable equal to one if total (short-term) [long-term] leverage is zero and else zero. $Post$ is a dummy variable equal to one for years greater or equal to 2006 and else zero. $Treated^{Factor}$ is a dummy variable equal to one for firms registered in postal code areas in which there are no factoring companies and else zero. $\mathbf{X}_{i,t}$ is a vector of control variables, including size, profitability, and age. $Size$ is the log of total assets; $Profitability$ is the ratio of earnings before interest and taxes to total assets; and Age is the log of the current year minus the year of incorporation. β and γ are free parameters. α_i , α_k , and α_t indicate firm-, industry-, and year-fixed effects. $\varepsilon_{i,t}$ is the residual. Parameter estimates are in normal letter, while t-statistics are in square parentheses. T-statistics are calculated from standard errors that are clustered at the firm-level. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively. The sample period is 2001 to 2009.

	Dependent Variable							
	Intensive Margin				Extensive Margin			
	Total Leverage	Short-term Leverage	Long-term Leverage	Dummy No Total Leverage	Dummy No Short-term Leverage	Dummy No Long-term Leverage	Dummy No Short-term Leverage	Dummy No Long-term Leverage
$Post \times Treated^{Factor}$	0.013*** [9.58]	0.003*** [3.06]	0.010*** [10.73]	0.007** [2.32]	0.002 [0.68]	-0.122*** [-21.78]		
$Size$	0.020*** [15.83]	0.013*** [13.52]	0.008*** [10.82]	-0.071*** [-27.09]	-0.077*** [-26.82]	-0.045*** [-14.96]		
$Profitability$	-0.188*** [-35.96]	-0.128*** [-32.78]	-0.049*** [-15.73]	0.077*** [7.12]	0.096*** [7.63]	0.094*** [6.69]		
Age	0.000 [-0.19]	0.001 [0.78]	-0.002 [-1.22]	-0.034*** [-7.08]	-0.044*** [-7.89]	0.072*** [10.63]		
R-squared	0.029	0.017	0.055	0.009	0.006	0.243		
Observations	227,305	227,305	227,305	227,305	227,305	227,305		

Table 6

Effect of the Reform on Leverage: DIDID Regressions Using Fixed Assets and Lack of Financing Companies as ID Variables

The table shows the results from the following regression:

$$Y_{i,t} = \alpha_i + \alpha_k + \alpha_t + \beta Post_t + \gamma Treated_{i,t}^{FixedAssets} \times Treated_{i,t}^{Factor} + \delta Post_t \times Treated_{i,t}^{FixedAssets} + \gamma Post_t \times Treated_{i,t}^{Factor} + \mathbf{X}_{i,t}\eta + \varepsilon_{i,t},$$

where $Y_{i,t}$ is total, short-term, or long-term leverage or dummy variables indicating whether the former quantities are zero. Total (short-term) [long-term] leverage is the sum of short-term and long-term (short-term) [long-term] debt scaled by total assets. Dummy no total (short-term) [long-term] leverage is a dummy variable equal to one if total (short-term) [long-term] leverage is zero and else zero. $Post$ is a dummy variable equal to one for years greater or equal to 2006 and else zero. $Treated_{i,t}^{FixedAssets}$ is a dummy variable equal to one for firms whose mean fixed assets-to-total assets ratio is in the top quartile and else zero. $Treated_{i,t}^{Factor}$ is a dummy variable equal to one for firms registered in postal code areas in which there are no factoring companies and else zero. $\mathbf{X}_{i,t}$ is a vector of control variables, including size, profitability, and age. $Size$ is the log of total assets; $Profitability$ is the ratio of earnings before interest and taxes to total assets; and Age is the log of the current year minus the year of incorporation. $\beta, \delta, \gamma,$ and η are free parameters. $\alpha_i, \alpha_k,$ and α_t indicate firm-, industry-, and year-fixed effects. $\varepsilon_{i,t}$ is the residual. Parameter estimates are in normal letter, while t-statistics are in square parentheses. T-statistics are calculated from standard errors that are clustered at the firm-level. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively. The sample period is 2001 to 2009.

	Dependent Variable					
	Intensive Margin			Extensive Margin		
	Total Leverage	Short-term Leverage	Long-term Leverage	Dummy Total Leverage	Dummy Short-term Leverage	Dummy Long-term Leverage
$Post \times Treated_{i,t}^{FixedAssets} \times Treated_{i,t}^{Factor}$	0.021*** [5.84]	0.007*** [2.78]	0.014*** [5.34]	-0.018*** [-2.86]	-0.024*** [-2.96]	-0.020 [-1.60]
$Post \times Treated_{i,t}^{FixedAssets}$	0.027*** [8.91]	-0.006*** [-3.18]	0.034*** [16.04]	0.016*** [2.86]	0.022*** [3.26]	-0.105*** [-9.97]
$Post \times Treated_{i,t}^{Factor}$	0.007*** [5.36]	0.001 [1.24]	0.006*** [7.10]	0.011*** [3.21]	0.008** [1.99]	-0.116*** [-18.05]
$Size$	0.021*** [16.22]	0.013*** [13.47]	0.008*** [11.72]	-0.071*** [-27.05]	-0.077*** [-26.77]	-0.047*** [-15.52]
$Profitability$	-0.188*** [-36.03]	-0.128*** [-32.78]	-0.049*** [-15.93]	0.077*** [7.11]	0.095*** [7.62]	0.094*** [6.75]
Age	-0.001 [-0.72]	0.001 [0.82]	-0.003** [-2.13]	-0.034*** [-7.11]	-0.044*** [-7.93]	0.075*** [11.18]
R-squared	0.042	0.017	0.091	0.009	0.006	0.246
Observations	227,305	227,305	227,305	227,305	227,305	227,305

Table 7

Effect of the Reform on Loan Contracting Terms: DID Regressions

The table shows the results from the following regression:

$$Z_{i,j,t} = \alpha_k + \alpha_t + \beta Post_t \times Treated_{i,j,t}^{Secured} + \mathbf{X}_{i,t}\delta + \mathbf{W}_{i,j,t}\gamma + \varepsilon_{i,j,t},$$

where $Z_{i,j,t}$ is either the loan spread, defined as the log of the sum of a loan’s coupon and annual fees scaled by its nominal value minus the six month LIBOR rate (in basis points), or the loan time-to-maturity, defined as the log of the difference between the loan’s maturity date and its initiation date (in months). $Post$ is a dummy variable equal to one for years greater or equal to 2006 and else zero. $Treated^{Secured}$ is a dummy variable equal to one for secured loans and else zero. $\mathbf{X}_{i,t}$ is a vector of firm-specific control variables. $\mathbf{W}_{i,j,t}$ is a vector of loan contract-specific control variables. $Size$ is the log of total assets; Age is the log of the current year minus the year of incorporation; $Profitability$ is the ratio of earnings before interest and taxes to total assets; and $TotalLeverage$ is the ratio of the sum of short-term and long-term debt to total assets. $Rating$ is a dummy variable equal to one if the firm taking out the loan is rated, else zero. $CreditSpread$ is the mean monthly difference between the yield of a corporate bond index and the yield of a long-term government bond index taken over the current year, and $TermSpread$ the mean monthly difference between the yield of a long-term government bond index and the yield of a short-term government bond index taken over the same period. $LoanSize$ is the log of the notional value of the loan; and $LoanType$ is a dummy variable equal to one for term loans, else zero. We also add the other endogenous variable to the control variables. β , δ , and γ are free parameters. α_k and α_t indicate industry- and year-fixed effects. Industry effects are based on 2-digit SIC codes. $\varepsilon_{i,j,t}$ is the residual. Parameter estimates are in normal letter, while t-statistics are in square parentheses. T-statistics are calculated from standard errors clustered at the industry-level. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively. The sample period is 2001 to 2009.

	Dependent Variable			
	Loan Spread		Loan Maturity	
$Post \times Treated^{Secured}$	-1.291*** [-4.09]	-1.257*** [-5.16]	0.786*** [6.27]	0.871*** [6.94]
$Size$		0.032 [1.07]		-0.023 [-1.47]
Age		-0.031 [-1.24]		0.033*** [3.83]
$Profitability$		0.265 [0.67]		-0.082 [-0.38]
$TotalLeverage$		-0.065 [-0.43]		0.100 [0.75]
$Rating$		-0.035 [-0.15]		0.168** [1.99]
$CreditSpread$	1.078*** [5.58]	1.086*** [11.70]	-0.298*** [-3.44]	-0.388*** [-7.02]
$TermSpread$	0.195*** [5.98]	0.183*** [4.29]	-0.079*** [-3.65]	-0.066*** [-2.73]
$LoanAmount$	-0.063*** [-2.87]	-0.080*** [-5.25]	-0.020 [-0.96]	0.001 [0.06]

(continued on next page)

Table 7
Effect of the Reform on Loan Contracting Terms: DID Regressions (continued)

	Dependent Variable			
	Loan Spread		Loan Maturity	
<i>LoanType</i>	0.304*** [11.51]	0.314*** [10.77]	0.032 [1.07]	0.023 [0.74]
<i>LoanMaturity</i>	0.249*** [5.17]	0.333*** [6.05]		
<i>LoanSpread</i>			0.201*** [3.64]	0.236*** [4.88]
<i>Treated^{Secured}</i>	1.550*** [16.77]	1.537*** [8.58]	-0.422*** [-3.69]	-0.576*** [-3.15]
R-squared	0.445	0.473	0.329	0.410
Observations	456	409	456	409

Table 8

Comparison of Debt Switchers and Non-Switchers

The table compares several firm characteristics across the average firm, firms that strongly benefitted from the reform, and firms that did not strongly benefit. In column (1), we consider the whole sample. In column (2), we only look at firms that never have any long-term debt before the reform. From the firms in column (2), the reform beneficiaries are then those firms that have a positive long-term leverage in every year after the reform (column (3)), while the non-beneficiaries are those firms that continue to never have any long-term debt after the reform (column (4)). The difference in firm characteristics across beneficiaries and the average firm is given under “Diff (3)-(1).” The difference across beneficiaries and non-beneficiaries is given under “Diff (4)-(1).” To calculate the table entries, we first average a variable’s pre-reform values by firm and then by group. Assets is total assets in million \$; Firm Age is the current year minus the year of incorporation; and Employees is the number of employees. Profitability is the ratio of earnings before interest and taxes to total assets; Profit volatility is the standard deviation of profitability over the most recent four fiscal years, including the most recent one. We set profit volatility equal to missing if it is based on fewer than three observations. Loss Dummy is a dummy variable equal to one if profitability is negative, else zero. Cash is cash reserves divided by total assets. The capital-to-labor ratio is the log of the ratio of tangible fixed assets to the number of employees; tangibility is the ratio of tangible fixed assets to total assets; and fixed assets is the ratio of fixed assets to total assets. We calculate the three distance variables by applying the formula for the spherical distance between two points to the longitudes and latitudes associated with a firm’s postal code and those associated with the city centers of the five/ten biggest French cities or the Paris Bourse. ***, **, * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

	All Companies		Companies Without Debt		Companies Without Debt		Diff.	
	(1)	(2)	(3)	(4)	(3)-(1)	(4)-(1)	(3)-(4)	
Assets (in million \$)	28.70	26.16	25.12	28.96	-3.58***	28.96	-3.83***	
Firm Age (in years)	23.03	22.75	23.93	22.08	0.90***	22.08	1.85***	
Number of Employees	146.90	132.33	142.75	123.20	-4.15	123.20	19.55	
Operating Profit	0.102	0.105	0.111	0.097	0.009***	0.097	0.015***	
Operating Profit Volatility	0.045	0.044	0.039	0.050	-0.006***	0.050	-0.012***	
Loss Dummy	0.109	0.099	0.066	0.139	-0.043***	0.139	-0.073***	
Cash Reserve	0.115	0.121	0.107	0.137	-0.008***	0.137	-0.030***	
Capital-to-Labor	2.50	2.48	2.73	2.16	0.24***	2.16	0.57***	
Tangible Assets	0.134	0.131	0.165	0.095	0.032***	0.095	0.070***	
Fixed Assets	0.233	0.228	0.270	0.179	0.037***	0.179	0.091***	
Distance to Top 5 City	75.74	76.39	89.71	58.07	13.97***	58.07	31.65***	
Distance to Top 10 City	42.39	42.69	51.04	31.81	8.64***	31.81	19.22***	
Distance to Capital Market	167.38	165.09	190.63	128.04	23.25***	128.04	62.59***	

Table 9**Effect of the Reform on the Leverage of Start-Ups: ANOVA Tests**

The table shows the long-term leverage ratio of start-ups in their year of incorporation for the whole sample (“All”) and the treated- and control-firm subsamples over the 2001–2009 period. We study two attributes of long-term leverage: the average (Panel A) and the proportion of zero long-term leverage firms (Panel B). Long-term leverage (“*LongTermLeverage*”) is defined as long-term debt scaled by total assets. We create the sample of treated (“High Fixed Assets Group”) and matched control firms (“Low Fixed Assets Group”) using Propensity-score (PS) matching. To do so, we use a LOGIT model to estimate the probability that a firm’s fixed assets-to-total assets ratio is in the top quartile. The exogenous variables are *Size*, *Profitability*, *Age*, and *TotalLeverage*. Using the fitted probabilities, we match each top fixed assets-to-total assets ratio-firm with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile firm. We discard the pair if the absolute difference between the firms’ fitted probability values exceeds 0.1%. At the bottom, we report averages over the pre-reform period attributes (2001–2005) and the post-reform period attributes (2006–2009) and their differences. The final column shows the differences in attributes across the high and low fixed assets group. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively, based on standard errors that are clustered at the firm level (whenever possible).

Year	All	Fixed Assets Group		Diff.
		High	Low	High-Low
Panel A: Mean Long-term Leverage				
2001	0.023	0.036	0.011	0.026*
2002	0.011	0.021	0.001	0.020*
2003	0.037	0.054	0.020	0.034
2004	0.036	0.045	0.027	0.018
2005	0.025	0.030	0.020	0.010
2006	0.055	0.079	0.031	0.047*
2007	0.047	0.077	0.016	0.061***
2008	0.027	0.042	0.013	0.029*
2009	0.033	0.042	0.024	0.019
Mean 2001–2005 (1)	0.026	0.037	0.016	
Mean 2006–2009 (2)	0.041	0.060	0.021	
Diff. (2)–(1)	0.014**	0.023**	0.006	0.017
Panel B: Proportion No Long-term Leverage Firms				
2001	0.900	0.886	0.914	-0.029
2002	0.932	0.898	0.966	-0.068
2003	0.872	0.833	0.910	-0.077
2004	0.893	0.857	0.929	-0.071
2005	0.886	0.864	0.909	-0.045
2006	0.625	0.481	0.769	-0.288***
2007	0.692	0.500	0.885	-0.385***
2008	0.667	0.533	0.800	-0.267**
2009	0.740	0.720	0.760	-0.040
Mean 2001–2005 (1)	0.897	0.868	0.926	
Mean 2006–2009 (2)	0.681	0.559	0.803	
Diff. (2)–(1)	-0.216***	-0.309***	-0.122***	-0.187***

Table 10
Effect of the Reform on Firm Performance: DID Regressions

The table shows the results from the following regression:

$$Y_{i,t} = \alpha_i + \alpha_k + \alpha_t + \beta Post_t \times Treated_i^{FixedAssets} + \mathbf{X}_{i,t} \gamma + \varepsilon_{i,t},$$

where $Y_{i,t}$ is growth, employees, the average wage, sales, profitability, or profit volatility. *Growth* is the sum of the change in intangible fixed assets and the change in inventories, where both changes are calculated from the prior fiscal year end to the current one and the sum is scaled by the average of total assets over the two fiscal year ends. *Employees* is the log of the number of employees. *Average Wage* is total wages paid divided by the number of employees. *Sales* is the log of sales. *Profitability* is earnings before interest and taxes scaled by total assets. *Profit Volatility* is the standard deviation of *Profitability* over the most recent four fiscal years, including the most recent one. We set *Profit Volatility* equal to missing if it is based on fewer than three observations. *Post* is a dummy variable equal to one for years greater or equal to 2006 and else zero. *Treated^{FixedAssets}* is a dummy variable equal to one for firms whose mean fixed assets-to-total assets ratio is in the top quartile and else zero. $\mathbf{X}_{i,t}$ is a vector of control variables, including size, total leverage, and age. *Size* is the log of total assets; *TotalLeverage* is total debt scaled by total assets; and *Age* is the log of the current year minus the year of incorporation. β and γ are free parameters. α_i , α_k and α_t indicate firm-, industry-, and year-fixed effects. $\varepsilon_{i,t}$ is the residual. Parameter estimates are in normal letter, while t-statistics are in square parentheses. T-statistics are calculated from standard errors that are clustered at the firm-level. ***, **, * and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively. The sample period is 2001 to 2009.

	Dependent Variable					
	Growth	Employees	Average Wage	Sales	Profitability	Profit Volatility
<i>Post</i> × <i>Treated^{FixedAssets}</i>	0.006*** [6.92]	0.051*** [7.04]	-0.017*** [-6.16]	0.041*** [5.68]	0.005*** [4.42]	-0.002*** [-4.12]
<i>Size</i>	0.046*** [49.20]	0.503*** [59.63]	0.020*** [6.56]	0.780*** [102.63]	0.012*** [12.47]	-0.010*** [-16.94]
<i>TotalLeverage</i>	-0.021*** [-7.70]	-0.011 [-0.66]	-0.009 [-1.30]	-0.224*** [-13.67]	-0.106*** [-36.46]	0.012*** [8.22]
<i>Age</i>	-0.045*** [-22.00]	0.171*** [18.62]	0.014*** [3.45]	0.212*** [24.64]	0.021*** [14.01]	-0.018*** [-10.75]
R-squared	0.013	0.461	0.300	0.591	0.012	0.030
Observations	209,633	174,807	174,505	230,614	228,782	158,227

Table 11
Effect of the Reform on Failure Rates: DID Regressions

The table shows the results from the following proportional Cox hazard model:

$$\lambda_{i,t} = \phi_t \exp(\beta Post_t \times Treated_i^{FixedAssets} + \mathbf{X}_{i,t}\gamma),$$

where the hazard rate, $\lambda_{i,t}$, is the probability of firm i failing at time t conditional on surviving until then, and ϕ_t is the “baseline” hazard rate common to all firms. We set a firm’s failure year to the calendar year in which its legal status changes from an active status to one of the failure statuses: “default of payment,” “insolvency proceedings,” “receivership,” “bankruptcy,” “dissolved (bankruptcy),” “dissolved (liquidation),” and “in liquidation.” We exclude a firm from the analysis if it is unclear whether it became inactive for performance reasons, that is, when legal status changes to: “inactive (no precision),” “unknown,” and “dissolved.” $Post$ is a dummy variable equal to one for years greater or equal to 2006 and else zero. $Treated^{FixedAssets}$ is a dummy variable equal to one for firms whose mean fixed assets-to-total assets ratio is in the top quartile and else zero. $\mathbf{X}_{i,t}$ is a vector of control variables, including total leverage, profitability, and profit volatility. $TotalLeverage$ is the sum of short-term and long-term debt to total assets. $Profitability$ is the ratio of earnings before interest and taxes to total assets. $ProfitVolatility$ is the standard deviation of $Profitability$ over the most recent four fiscal years. We set $ProfitVolatility$ equal to missing if it is based on fewer than three observations. β and γ are free parameters. Parameter estimates are in normal letter, while t-statistics are in square parentheses. T-statistics are calculated from standard errors clustered at the firm-level. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively. The sample period is 2001 to 2009.

Dependent Variable = Time-to-Failure			
$Post \times Treated^{FixedAssets}$	-0.338*** [-4.36]	-0.451*** [-5.62]	-0.204** [-2.13]
$TotalLeverage$		1.196*** [7.70]	0.490*** [2.82]
$Profitability$			-4.956*** [-13.22]
$ProfitVolatility$			2.380*** [2.83]
R-squared	0.001	0.003	0.031
Observations	182,249	182,222	129,946

Table 12
Effect of the Reform on Capital Allocation

The table shows the results from the following panel-data regression:

$$InvestmentGrowth_{k,t} = \alpha + \beta ValueAddedGrowth_{k,t} + \varepsilon_{k,t},$$

where $InvestmentGrowth_{k,t}$ is the percentage change in gross fixed capital formation for industry k from year $t - 1$ to year t , and $ValueAddedGrowth_{k,t}$ is the percentage change in value-added over the same time period. We perform this regression using pre-reform (2001–2005) and post-reform (2006–2007) data. α and β are free parameters, and $\varepsilon_{k,t}$ is the residual. In addition to the parameter estimates and their inference levels (T-Statistic), the table shows the number of observations (Obs) and the R-squared (R-squared) per regression, and it also reports the difference in the slope coefficient across the two periods.

Year	Obs	Elasticity (β)	T-Statistic	R-Squared
2001–2005 (1)	566	0.434	6.15	0.062
2006–2007 (2)	237	0.697	5.34	0.104
Diff. (2)–(1)		0.263	1.79	

Table 13

Effect of the Reform on Firms with a High and Low External Financial Dependence: ANOVA Tests

The table shows the mean long-term leverage ratio for all firms (“All”), high fixed assets-, and low fixed-assets firms with an either high or low dependence on external financing, by year and over the 2001–2009 period. Long-term leverage (“*LongTermLeverage*”) is defined as long-term debt scaled by total assets. Fixed assets (“*FixedAssets*”) is defined as fixed assets scaled by total assets. We obtain an industry-specific measure of external financial dependence (“*ExternalFinancialDependence*”) by calculating the median proportion of capital expenditure that are not financed by cash flows from operations for U.S. public firms. We perform these calculations using COMPUSTAT data over the period from 1975–2005. We classify as firms with a high (low) external financial dependence those operating in industries with an above (below) median *ExternalFinancialDependence* value. We create the sample of treated and matched control firms using Propensity-score (PS) matching. To do so, we use a LOGIT model to estimate the probability that a firm’s fixed assets-to-total assets ratio is in the top quartile. We run this LOGIT model separately by two-digit SIC industry and using only data from 2005. As exogenous variables, we use *Size*, *Profitability*, *Age*, and *TotalLeverage*. Using the fitted probabilities, we match each top fixed assets-to-total assets ratio-firm with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile firm. We discard the pair if the absolute difference between the firms’ fitted probability values exceeds 0.1%. At the bottom of the table, we report averages over the pre-reform period means (2001–2005) and the post-reform period means (2006–2009) and their differences. The penultimate (final) column shows the differences in means across all firms (treated firms) with a high and those with a low external financial dependence. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively, based on standard errors that are clustered at the firm level.

Year	External Financial Dependence						High-Low External Financial		
	High			Low			Dependence Across		
	All (1)	Treated (2)	Controls (3)	All (4)	Treated (5)	Controls (6)	All (1)–(4)	Treated (2)–(5)	Treated (2)–(6)
2001	0.019	0.024	0.015	0.019	0.025	0.016	0.000	0.000	-0.001
2002	0.018	0.022	0.014	0.018	0.024	0.015	0.000	0.000	-0.003
2003	0.017	0.021	0.014	0.017	0.023	0.015	0.000	0.000	-0.002
2004	0.017	0.019	0.015	0.017	0.024	0.013	0.000	0.000	-0.005**
2005	0.021	0.024	0.018	0.018	0.024	0.016	0.003**	0.000	0.000
2006	0.069	0.082	0.056	0.047	0.061	0.039	0.023***	0.020***	0.020***
2007	0.074	0.089	0.058	0.049	0.066	0.041	0.025***	0.023***	0.023***
2008	0.089	0.105	0.071	0.059	0.074	0.051	0.030***	0.031***	0.031***
2009	0.098	0.116	0.077	0.064	0.083	0.055	0.034***	0.033***	0.033***
Mean 2001–2005 (1)	0.019	0.022	0.015	0.018	0.024	0.015	0.001	0.001	-0.002**
Mean 2006–2009 (2)	0.082	0.097	0.065	0.054	0.070	0.046	0.028***	0.027***	0.027***
Diff. (2)–(1)	0.063***	0.075***	0.049***	0.036***	0.046***	0.031***	0.027***	0.027***	0.029***

Table 14**Migration Rates Between the High and Low Fixed Assets Groups**

This table shows migration rates between the high- and the low-fixed assets group over various five year-periods in the 1996–2009 period. The migration rates are calculated using the first and the last year in the five year period. A firm belongs to the high-fixed assets group if its fixed assets-to-total assets ratio is in the top quartile in a given year. Otherwise, it belongs to the low-fixed assets group.

Period	Moved Into the High-Fixed Asset Group	Stayed in the Same Group	Moved Into the Low-Fixed Asset Group
1996-2000	0.077	0.852	0.071
1997-2001	0.084	0.845	0.071
1998-2002	0.078	0.853	0.069
1999-2003	0.073	0.857	0.070
2000-2004	0.075	0.860	0.065
2001-2005	0.068	0.862	0.070
2002-2006	0.065	0.867	0.068
2003-2007	0.066	0.869	0.065
2004-2008	0.064	0.866	0.070
2005-2009	0.060	0.869	0.071

Table 15**Effect of the Reform on the Long-term Leverage of Public Firms: ANOVA Tests**

The table shows the mean long-term leverage ratio of public firms for the full sample (“All”) and treated- and control-group subsamples over the 2001–2009 period. Long-term leverage (“*LongTermLeverage*”) is defined as long-term debt scaled by total assets. We create the sample of treated (“High Fixed Assets Group”) and matched control firms (“Low Fixed Assets Group”) using Propensity-score (PS) matching. To do so, we use a LOGIT model to estimate the probability that a firm’s fixed assets-to-total assets ratio is in the top quartile. We run this LOGIT model using only data from 2005. As exogenous variables, we use *Size*, *Profitability*, *Age*, and *TotalLeverage*. Using the fitted probabilities, we match each top fixed assets-to-total assets ratio-firm with the one firm from the other quartiles whose fitted probability is closest to the one of the top quartile firm. We discard the pair if the absolute difference between the firms’ fitted probability values exceeds 0.1%. At the bottom of the table, we report averages over the pre-reform period means (2001–2005) and the post-reform period means (2006–2009) and their differences. The final column shows the differences in means across the treated and control firms. ***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively, based on standard errors clustered at the firm level.

Year	All	Fixed Assets Group		Diff.
		High	Low	High–Low
2001	0.108	0.221	0.068	0.153***
2002	0.118	0.187	0.087	0.099***
2003	0.112	0.180	0.078	0.101***
2004	0.094	0.143	0.066	0.077***
2005	0.084	0.087	0.080	0.006
2006	0.114	0.131	0.102	0.029
2007	0.114	0.082	0.131	-0.049
2008	0.135	0.117	0.144	-0.027
2009	0.108	0.114	0.104	0.010
Mean 2001–2005 (1)	0.103	0.164	0.076	
Mean 2006–2009 (2)	0.118	0.111	0.120	
Diff. (2)–(1)	0.014	-0.052	0.044*	-0.096**