Access to Collateral and The Democratization of Credit: France's Reform of The Napoleonic Code*

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Abstract

We exploit wrinkles of a contracting framework to show how access to collateral shapes the composition of corporate borrowing and the demographics of credit access. France's *Ordonnance 2006-346* repudiated the 200-year old Napoleonic security code, easing the pledge of hard assets in a country where corporate credit was highly concentrated. The reform was undermined by "non-codified" laws pushed by firms in large cities, which allowed them to pledge liquid assets to factoring companies. Using a differences-test strategy, we show that firms with high utilization of hard assets and limited access to factoring services increased their leverage ratios following the reform (*intensive margin*), with the fraction of "zero-leverage" firms among them dropping from 89% to 29% (*extensive margin*). Using contract-level data, we show that hard assets allowed for significant reductions in loan mark-ups and increases in loan maturities. Small, profitable, low-risk firms benefitted the most from derogating the Napoleonic code. Start-up firms registered unprecedented increases in the use of debt financing at incorporation. Department-level analysis allows us to map the effects of *Ordonnance 2006-346* on credit access inequality within and across different areas of the country. The reform reached firms in rural areas, leading to a pronounced decline in the Gini index of credit access inequality across France's countryside.

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

JEL classification: G32, K22, O16.

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Abstract

We exploit wrinkles of a contracting framework to show how access to collateral shapes the composition of corporate borrowing and the demographics of credit access. France's *Ordonnance 2006-346* repudiated the 200-year old Napoleonic security code, easing the pledge of hard assets in a country where corporate credit was highly concentrated. The reform was undermined by "non-codified" laws pushed by firms in large cities, which allowed them to pledge liquid assets to factoring companies. Using a differences-test strategy, we show that firms with high utilization of hard assets and limited access to factoring services increased their leverage ratios following the reform (*intensive margin*), with the fraction of "zero-leverage" firms among them dropping from 89% to 29% (*extensive margin*). Using contract-level data, we show that hard assets allowed for significant reductions in loan mark-ups and increases in loan maturities. Small, profitable, low-risk firms benefitted the most from derogating the Napoleonic code. Start-up firms registered unprecedented increases in the use of debt financing at incorporation. Department-level analysis allows us to map the effects of *Ordonnance 2006-346* on credit access inequality within and across different areas of the country. The reform reached firms in rural areas, leading to a pronounced decline in the Gini index of credit access inequality across France's countryside.

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1 Introduction

Economic elites are thought to exert pressure over the design of contracting frameworks, seeking arrangements that benefit their interests (see Feijen and Perotti (2006) and Rajan (2006) for formal models). In this vein, Benmelech and Moskowitz (2010) show how *usury laws* (caps on interest rates) were introduced in the settlement of U.S. state laws as a way to protect incumbents' interests against entrants; typically small, risky enterprises whose financing requires credit bearing high interest rates. Other manifestations of such discriminatory arrangements concern access to collateral in credit transactions. To date, several countries have security laws derived from the Napoleonic Code, which is predicated on the *possessory nature* of asset ownership. Those laws were designed around an economic system in which land-based production and ownership took center stage. Under the highly-formalized Napoleonic Code, assets are seen as unique, non-transferrable, and non-substitutable, and these legal fictions limit the types of security interests that can be written on them (see Omar (2007) and Ancel (2008)).

Today's policymakers often grapple with reforming "perverse, endowed contracting frameworks" as a way to democratize access to credit (Rajan (2006)). In underdeveloped contracting environments, financing is necessarily relationship-based, favoring large, well-established, well-connected incumbents over small, young, innovative newcomers (Rajan and Zingales (2003)). Efficient collateral regimes can reduce the problems underlying relationship-based financing, easing credit access (Demirgüç-Kunt and Levine (2008)). In this paper, we argue that a recent reform in France, *Ordonnance 2006-346*, uniquely informs the debate about the link between financial contracting, access to credit (level and distribution effects), and economic outcomes.

Ordonnance 2006-346 derogated the possessory nature of asset ownership in France, in existence since 1804. By doing so, the 2006 legislation allowed French firms to "control and operate" physical assets pledged to third parties. This seemingly simple statutory change significantly enlarged the menu of assets that firms could pledge in credit transactions, particularly *hard movable assets* used in modern business operations (e.g., machinery and equipment). In addition to expanding the set of assets that could be collateralized, the new statute allowed for security interests to be charged to more than one party, making it feasible for loans to be syndicated under multiple creditors, with multiple priority schemes, and multiple maturity structures. In allowing for rechargeable security interests, the reform also enhanced the pledgeability of *hard immovable assets* (e.g., land and buildings). Notably, the reform did not introduce changes in the balance of power between contracting parties in default states, in asset seizure options, or in the judicial system. This differentiates it from most other collateral reforms, which have favored the notion of "strengthening creditors' rights" as a means to stimulate credit (see Lilienfeld-Toal et al. (2012), Vig (2013), and Assunção et al. (2014) for examples of such reforms).

Changing the legal nature of asset ownership meant that firms were discretely endowed with "new assets" that they could pledge as collateral in debt transactions. Yet the new law did not affect all French firms equally. Long before *Ordonnance 2006-346* was brought to the policy debate, companies located in France's financial centers had successfully lobbied for legal exceptions that allowed them to pledge certain types of liquid assets as collateral. Since these exceptions were not added to the Napoleonic Code, they were labeled "non-codified" security laws. Chief among them were those that allowed firms to pledge cash, securities, and accounts receivables to factoring companies.[1] Davydenko and Franks (2008) report that around three quarters of the collateral posted by French firms prior to 2006 consisted of liquid assets under non-codified credit agreements.

The various wrinkles in the process through which France reformed its security laws allow for unique insights into understanding the distributional and wealth effects of easing access to collateral and credit. Our paper's empirical strategy builds on the observation that firms whose operations relied more intensively on hard assets would be favored by a reform that differentially enhanced the ability to pledge that class of assets. A second layer of our strategy uses the observation that French firms needed factoring services to pledge liquid assets. Critically, the operations of factoring companies in France were legally constrained to identifiable geographic areas and targeted businesses locally.[2] As a result, firms located in areas with no offerings of factoring services had a hard time pledging either liquid or hard assets before the reform. Within this setting, we contrast firms not only along the types of assets more intensely used in their production processes (hard *versus* liquid assets), but also according to the nature of the credit that they access (secured *versus* unsecured, long-term *versus* short-term), their status (private *versus* public), and their geographic location (distance from factoring companies and financial centers), among other dimensions that help identify how policy affects credit taking and economic outcomes.

We first study the effect of *Ordonnance 2006-346* on firm-level (quantity) and contract-level (price) credit data within a differences-testing framework. We show that the reform significantly impacted the amount of debt held by French firms; in particular, firms with no access to public security markets. Those firms' mean total debt-to-assets ratio increased from a pre-reform average of 10.1% to a post-reform average of 14.4%. Importantly, the reform only affected long-term debt-taking (secured by hard assets). In addition to debt levels (the intensive margin), we also investigate the propensity of firms to contract *any* debt due to the reform (the extensive margin). Before 2006, 89% of the firms had no long-term leverage ("zero leverage firms"). During the reform year alone, this figure halved, dropping to 33% by the end of our sample period.[3]

[1] Two examples of such laws are the "Dailly Law," adopted in 1981, and the "Pledge of Ready Money," in place since the early 1970s. We discuss these regulations shortly.

[2] To date, well over 90% of the French factoring companies have only one physical branch. Only one factoring company had more than a couple dozen branches in 2006, all confined to a few large cities.

[3] Alternative sources of financing not requiring collateral, such as leasing, were not widely used in France. On

Using firms' fixed assets intensity as a way to identify the effects of the collateral reform, we show that the increase in debt-taking and the decline in the proportion of zero leverage firms occurred primarily among "high-fixed assets" firms. In particular, while the long-term leverage of high-fixed asset intensity firms rose by 7.8 percentage points, the long-term leverage of low-fixed asset firms increased by only 2.8 points. The 5% difference is remarkably significant when compared to the pre-reform average leverage ratio of 1.7%. In contrast, short-term leverage (not secured by hard assets) did not change across high- or low-fixed asset intensity firms. Using the geographical proximity to factoring companies as an additional identification wrinkle, our tests show that the increase in debt-taking and the decline in the proportion of zero leverage firms occurred among firms located far away from factoring companies; that is, among firms that had the hardest time pledging assets of any kind before *Ordonnance 2006-346*.

We next use loan-level data to study whether the reform led to changes in contract terms. We assign to a "treatment group" status those loans that were directly contemplated by the security reform (asset-secured loans) and to a "control group" those loans that were not (unsecured loans). Difference-in-differences estimations show that secured loans became less expensive and were given longer maturities compared to unsecured loans after the reform. Specifically, mark-ups of secured loans dropped by 120 basis points relative to the mark-ups of unsecured loans (the pre-reform mean was 213 basis points), while the maturity of secured loans increased by around 41 months relative to unsecured loans (the pre-reform mean was 74 months). We also find that the number of lenders involved in secured loans increased relative to those involved in unsecured loans. Our evidence shows that banks became willing to extend secured loans at more favorable terms after the passage of *Ordonnance 2006-346*, speaking directly to the argument that an increase in the collateral-offer space has an easing effect on other dimensions of collateralized borrowing. In addition, our evidence shows that *Ordonnance 2006-346* made it easier to set up syndicated secured loans involving a greater number of lenders.

The next step in our analysis is to study whether the reform-induced credit expansion reached firms previously rationed in the credit markets ("credit democratization"). We perform several tests on this front. We first look at the characteristics of firms that never held long-term debt before the reform. Within this subsample, we contrast firms that borrowed in every post-reform year ("new borrowers") with firms that kept a zero-debt policy even after the reform ("non-borrowers"). We also compare the new borrowers with the average firm in the French economy. In all, new borrowers were smaller, more cash-constrained, more profitable, less risky, and more capital-intensive firms. Moreover, they were located further away from financial centers. The preliminary evidence suggests that the reform benefitted seemingly cred-

average, only 11% of the country's total gross fixed investment was funded through leasing. This figure is small when compared to 17% for Germany, 26% for the U.S., and 13% for other European Civil Law countries.

itworthy firms that were previously unlikely to receive credit. This is a key policy issue since the expansion of credit along the "wrong margin" of borrowers may have negative effects on the economy (e.g., see White (2007) and Mian and Sufi (2009)).

We take the analysis one step further and test whether the reform eased credit access for a class of firms that is particularly subjected to credit constraints: start-up firms. Our tests show that the average long-term leverage of start-ups incorporated after 2006 is 1.5 percentage points higher than that of start-ups incorporated before 2006, when the sample average long-term leverage was only 2.6%. Remarkably, the proportion of start-ups with no long-term debt in their year of incorporation drops from 90% to 68% with the reform. The results we report are important in showing that *Ordonnance 2006-346* allowed prospective entrepreneurs to come to the credit market and compete for funds with incumbent firms.[4]

Our third credit democratization test is a geography-based analysis of the reform's effects. Several studies emphasize the importance of firm location for financial contracting. A high density of financial institutions, in particular in and around urban centers, lowers the informational costs of credit acquisition (e.g., Garmaise and Moskowitz (2004) and Dass and Massa (2011)). As such, firms located in rural areas face a disadvantage in accessing credit. We use department-level data to map the effects of *Ordonnance 2006-346* on credit access.[5] In line with the notion that the reform democratized credit access, we find that firms located farther away from the main French metropolitan areas and financial centers benefitted the most from the reform. We also use a Gini index of corporate credit access inequality to assess whether the reform reduced inequalities in credit access *within and across* geographical areas. Before the reform, 89 out of the 96 French departments had a Gini index of long-term debt concentration exceeding 0.90, suggesting that the credit market in virtually every single department was dominated by a handful of firms. After the reform, the average Gini index dropped to 0.70, with no department registering a value above 0.90. Notably, the largest drops occurred in rural departments far from the main French metropolitan areas and financial centers.

A key remaining question is whether the increase in credit access has tangible economic consequences. In the final part of our analysis, we investigate the real effects of the reform both at the micro- and macro-economic levels. At the micro level, we find that high-fixed assets firms increased their investment more significantly than low-fixed assets firms after *Ordonnance 2006-346*. They also hired more workers. Interestingly, firms seemed to hire cheaper labor. Our evidence also suggests that the increases in capital and labor enabled high-fixed assets firms to pursue more profitable, less risky investments. This is an important result from an economic

[4]Our findings are in tandem with those in Bertrand et al. (2007), who show how the French banking deregulation of the 1980s facilitated the entry of new firms in several concentrated product markets.

[5]A French department is roughly equivalent to a state in the United States.

welfare perspective as it implies that the plausibly "better" investments made possible by the collateral reform ultimately contributed to increasing corporate survival rates. Indeed, we show that, while high-fixed assets firms were more likely to fail than low-fixed assets firms before the collateral reform, the relationship is reversed afterwards.

Turning to the real effects of the reform at the macro level, we run two sets of tests. First, following Wurgler (2000), we use the sensitivity of capital investment growth to value-added growth as proxy for the efficiency of capital allocation. We find that Wurgler's efficiency proxy jumps from a pre-reform level of 0.43 to a post-reform level of 0.70. We also use the external financing dependence proxy of Rajan and Zingales (1998) as an additional capital allocation efficiency metric.[6] We find that the reform raised long-term borrowing more strongly in sectors that are highly dependent on external financing. In all, our evidence suggests that the reform-induced changes in the collateral regime ultimately improved the efficiency of the capital allocation process.

Our study contributes to the Law and Finance literature on different fronts. Most existing studies analyze cross-country heterogeneity in bankruptcy regimes (LaPorta et al. (1998) and Djankov et al. (2007)) or changes to bankruptcy regimes (Lilienfeld-Toal et al. (2012) and Vig (2013)). Our focus, instead, is on collateral. We show how collateral regime changes may spur financial democratization, allowing small, profitable, less risky, start-up firms, and firms located in rural areas to enter credit markets. We also show that collateral regime changes may bring about positive real-side implications at both the micro- and macro-economic levels.

Our study adds to recent research looking at how collateral affects bank credit.[7] Cerqueiro et al. (2014) show that a reduction in collateral value triggers increases in interest rates and reductions in credit limits. Calomiris et al. (2016) show that bank lending secured with movable assets is lower in countries with weaker collateral laws. Our study is different in that we focus on firms (rather than banks), and this focus allows us to make inferences about real-side implications of collateralized debt. We are able to show, for example, that a collateral regime change that makes it easier to pledge hard assets led firms whose asset composition is skewed toward such assets to borrow and invest into more profitable, lower risk projects. Closer to our paper, Campello and Larrain (2016) look at a collateral regime change in an emerging market, Romania. In contrast to their study, the length and complexity of the process through which a developed country such as France reformed its security laws allow us to use more refined and granular identification strategies. Our data and setting provide distinct insights into the connections between collateral and credit both at the firm and the contract level. Moreover, we are uniquely able to assess various aspects of credit access distribution and democratization.

[6]This proxy captures an industry's demand for external funding. The working assumption is that when the economic system operates efficiently, capital will be directed toward sectors that need more external funding.

[7]Other studies show the effects of changes in collateral value on firm borrowing (Benmelech and Bergman (2009)) and entrepreneurship (Schmalz et al. (2015)).

Our paper is also related to the literature linking credit conditions and economic development. Some studies suggest that credit expansion can trigger waves of default and repossessions (White (2007), Mian and Sufi (2009), Keys et al. (2010), and Assunção et al. (2014)). Others show that credit expansion leads to more business formation and investment (Chatterji and Seamans (2012)). We add to this literature by showing that collateral regime-induced credit expansions can improve capital allocation efficiency: not only is capital directed toward growth industries, but also toward industries starving for external funding.

Our article is organized as follows. Section 2 discusses the French collateral reform. Section 3 describes our data and our empirical methodology. Section 4 analyzes the effect of the reform on credit access. Section 5 describes the characteristics of firms that most benefitted from the reform. Section 6 performs a geographic analysis of the reform's effects. Section 7 describes real-side implications. Section 8 contains robustness checks. 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 Code. The Code recognized two forms of security interests, the "hypothec" and the "charge." A hypothec could only be taken out over real estate property, while a charge could be taken out over all types of assets.[8] In spirit and practice, the early 19^{th} century code focused on security interests over land. At that time, the French economy was centered around land-based activities and lawmakers deemed security interests over other assets as "of little relevance" (Omar (2007)). In comparison to the statutory schemes for secured transactions in other countries, for example, Article 9 of the Uniform Commercial Code in the U.S., French security laws were uncoordinated and inconsistent (Haimo (1983)). As the need for modern financing demanded new security techniques, the legislative filled gaps in the law with *ad hoc* regulation. The cumulative effect of this approach was a lack of uniformity among requirements for creating and perfecting various types of securities interests over different types of assets.

The 1804 Napoleonic Code was predicated on the premise that borrowers had to be protected from exploitative creditors, while creditors had to be protected from borrowers refusing the surrender of collateral in default. Accordingly, regulators established highly formal, costly procedures for the creation of security rights. Among other distortions, they refused to recognize rechargeable hypothecs since these could lead to conflicts about priority (see Ancel (2006)). In addition, they refused to recognize non-possessory charges since assets pledged with-

[8] See Boughida et al. (2011) for more details on French security interests.

out dispossession could lead future creditors to "misapprehend a borrower's creditworthiness" (see Haimo (1983)).[9] Critically, ruling out non-possessory charges meant that borrowers had to physically transfer pledged assets for creditors to validate — in the legal jargon, "perfect"— a security interest. This statue of the law had profound implications for borrowing. In particular, it implied that firms could not pledge their assets to third parties while at the same time continuing to freely use those assets in their regular (in-house) operations. Notably, because assets were seen as "unique" and "non-substitutable," firms could not offer "similar assets" (nor cash equivalents) in securing a credit transaction. Security interests over "fungible assets" (such as oil, steel, etc.) or "future assets" (such as machinery and equipment under construction, or inventory in process) were also not allowed because such assets could not be easily identified and physically transferred to creditors. To protect borrowers, regulators also explicitly forbade out-of-court seizures of collateral (Ancel (2006)).

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

By the early 2000s, politicians became increasingly aware of the fact that France's security laws were far less competitive than those of other developed countries (Renaudin (2013)). In March 2005, President Jacques Chirac called upon the Minister of Justice to reform the country's security laws. In April 2005, an official working group of academics and practitioners submitted recommendations on how to best achieve this goal (the Grimaldi Report). In March 2006, the French parliament enacted a sweeping reform of French security laws.

Ordonnance 2006-346 introduced substantial changes to the law and practice of secured credit transactions. The reform repudiated the possessory nature of asset ownership and, in the process, allowed for parties to write contracts contemplating non-possessory charges and security interests over various types of corporate assets. The new statute also allowed security interests to be rechargeable to the same or new creditors, either simultaneously or consecutively. Finally, the reform lowered the costs of creating security interests; for example, it reduced notary fees, taxes, and registration cost. Importantly, the new law did not introduce "floating charges" (charges applying to the "general collection" of corporate assets). As such, the identification of assets legally recognizable and eligible for collateralized charges remained a key issue for credit access (see Herbet and Sabbah (2006) and Ancel (2008)).

The 2006 reform had originally aimed at strengthening creditor rights by lifting the ban on out-of-court seizures of collateral in default. This was necessary because a recent attempt at reforming the bankruptcy process (the 2005 "Safeguard Provision") had strengthened the rights of shareholders in distressed firms. However, because the French parliament appointed

[9]The refusal to recognize non-possessory charges is grounded in the belief that only the physical possession of a production asset unequivocally demonstrates a firm's creditworthiness.

two separate groups — accountable to different branches of government — to modernize security laws and bankruptcy laws, little effort was made to ensure that reforms in the two areas would be coherent. As such, even much after the 2006 reform, the bankruptcy code still suspends a creditor's right to seize collateral out-of-court once a firm files for bankruptcy (which a firm is legally obliged to do when it defaults). Notably, the failure to strengthen creditors rights in default makes the French reform unique: a reform that enlarges the contracting space without altering the balance of power between parties, seizure procedures, or court involvement.

2.3 Undermining the 2006 Reform: Non-Codified Security Laws

Managers of large companies in big cities had long recognized that the 1804 security laws were outdated and costly to work with. In the 1970s and 1980s, their concerted lobbying spurred security laws that were purposefully not added to the Napoleonic Code.[10] The most popular of these "non-codified laws" were the Dailly Law and the Pledge of Ready Money. The Dailly Law (named after an influential Parisian businessman, Etienne Dailly) allowed firms to assign their receivables to non-banking financial institutions in securitizing debt contracts. The Pledge of Ready Money allowed them do the same using their cash and cash equivalents. 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.[11] Their major limitation was that their use was restricted to liquid corporate assets.

In practical terms, firms could pledge their receivables and similar liquid assets to factoring companies under non-codified credit schemes.[12] Notably, however, the operations of factoring firms were geographically fragmented in France, a condition stemming from strict regulation of the country's financial system. Of the 96 French departments, only four had more than 20 factoring companies in operation in 2006, and these included France's largest financial centers: the greater Paris area, Lyon, and Lille. The vast majority of factoring firms had only one branch. This severely limited contracting since firms needed to be located near factoring companies to enable transactions involving the use of receivables as collateral. For practical purposes, France's non-codified laws only facilitated credit access to firms located in metropolitan areas.

The regime allows for	1804 Napoleonic Code Laws		1970/80s Non-Codified Laws		2006 Security Laws	
security interests that are ...	Liquid Assets	Hard Assets	Liquid Assets	Hard Assets	Liquid Assets	Hard Assets
... non-possessory	NO	Immovables Only	YES	NO	YES	YES
... rechargeable	NO	NO	YES	NO	YES	YES

Figure 1. Comparison of French Security Law Regimes The table compares the old French security laws introduced by the Napoleonic Civil Law Code in 1804, the non-codified laws enacted in the late 1970s and early 1980s, and the new security laws introduced by *Ordonnance 2006-346* in 2006.

2.4 Identification Strategy

Figure 1 provides an overview of the various security law regimes in place in France since 1804. Relative to the Napoleonic Code laws, the non-codified laws enhanced the pledgeability of liquid assets by allowing firms to create security interests over accounts receivables and other cash-like assets. As discussed, they benefitted firms that were physically close to factoring companies. The 2006 reform enhanced the pledgeability of hard assets. It did so most notably by allowing for the creation of non-possessory security interests over hard movable assets such as equipment and machines. In addition, the reform also allowed for rechargeable security interests over hard immovable assets such as land and buildings.

We use the various wrinkles in the French security laws to design two firm-level identification strategies. The first strategy exploits the observation that, while hard assets could not be easily used as collateral before the 2006 reform, liquid assets could already be liberally pledged. Accordingly, the reform is expected to have more significantly affected firms whose operating assets are composed of more fixed assets than firms whose operating assets are more liquid. The second strategy exploits the insight that firms needed the services of a factoring company to pledge their liquid assets. Accordingly, the reform is expected to have affected more significantly firms located far away from factoring companies, since these firms had a hard time pledging *either* hard *or* liquid assets before the 2006 reform. Differently put, we use the geographic proximity to factoring companies as a factor moderating the effect of the 2006 reform. As a final contrast, we use a loan-level identification strategy that compares loans that were directly affected by the collateral reform (secured loans) with loans that were not (unsecured loans).

[10]Bypassing the formal Code was a way to facilitate their implementation on an "exceptional basis."

[11]Super-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.

[12]A Dailly assignment of receivables by way of security may only be granted by a borrower (and not by a guarantor or a third party security grantor) and only in favor of a French licensed factoring credit institution.

3 Methodology and Data

3.1 Methodology

We use a difference-estimator methodology to trace the effects of the collateral reform. To implement our first firm-level identification strategy, we compare outcome variables across high-*versus* low-fixed assets utilization firms over the pre- and the post-reform periods. As a baseline, we categorize firms in the top quartile of the fixed assets-to-total assets ratio distribution as "high-fixed" assets firms.[13] To ensure outcomes are not driven by observables other than fixed assets, we also run comparisons based 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 of that asset distribution. We estimate the model within each industry, using data from the year directly preceding the reform (2005). As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with that within-industry counterpart belonging to the other quartiles and having the closest logit-model fitted probability.

While subsample mean comparisons are informative, resulting estimates are possibly biased due to unobserved firm-, industry-, and year-specific effects. As a result, we also estimate multivariate models including such effects. The multivariate models can be written as:

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

where $Y_{i,t}$ is the value of the outcome variable for firm i in year t, with $Y_{i,t} \in \{$*LongTermLeverage*, *ShortTermLeverage*, *DummyNoLongTermLeverage*, *DummyNoShortTermLeverage*, *Growth*, *Employees*, *AverageWage*, *Sales*, *Profitability*, *ProfitVolatility*$\}$. While these variables have intuitive labels, we define them in detail shortly. $Post_t$ is a dummy variable that equals one for year 2006 onward and zero otherwise. Using the first identification strategy, we set $Treated_i$ equal to $Treated_i^{HighFixedAssets}$, a dummy variable equal to one if firm i's 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 $Treated_i^{NoFactoring}$, a dummy variable equal to one if firm i is registered in a geographical area (defined shortly) without 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. We cluster standard errors at the firm level.[14] The β coefficient in Equation (1) can be interpreted

[13] Using alternative quantiles (such as quintiles or terciles) does not alter our inferences. Our choice is solely guided by the natural trade-off between bias and efficiency one faces in these cases.

[14] We experimented with more saturated models in which we included industry-year fixed effects ($\alpha_k \times \alpha_t$) instead of separate year and industry fixed effects to capture time variation within industries. Those other fixed effects and error clustering combinations yielded results similar to those reported in the paper.

as the regression-based DID estimate after accounting for various controls and fixed effects.

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

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

where β, δ, γ, and η are free parameters, with γ returning the DIDID estimate of interest.

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

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

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 $\mathbf{W}_{i,j,t}$.[15]

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.[16] We collect data on French firms as well as on firms in other European Civil Law countries (Belgium, Italy, Spain, and Portugal).[17] We exclude firms in the financial, services, utilities, and public administration sectors.[18] Our baseline tests exclude public firms as these hardly ever borrow under secured debt facilities. However, we later run a falsification test on public firms.

To run our 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 (mnemonic BorrowerIssuerName) with the AMADEUS firm name (CompanyName). We succeed in doing so for 90% of all borrowers in Dealscan. We only keep revolver and term loans because those facilities are most reflective of pricing terms and credit restrictiveness (Campello and Gao (2016)).[19]

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

[16] For 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.

[17] We use the non-French data to conduct placebo tests described later.

[18] Restricting the analysis to manufacturing firms does not change our conclusions.

[19] A term loan is a loan that has to be repaid according to a fixed repayment schedule. A revolver loan allows for a specific amount to be withdrawn, repaid, and redrawn again in any manner and any number of times. We

We collect data on the addresses of French firms from AMADEUS and BANKSCOPE. 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.[20]

3.3 Variable Construction

3.3.1 Firm-Level 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 (mnemonic loan) and long-term debt (ltdb) divided by total assets (toas). *ShortTermLeverage* is defined as short-term debt divided by total assets, and *LongTermLeverage* as long-term debt divided by total assets. We further define two dummy variables, *DummyNoShortTermLeverage* and *DummyNo-LongTermLeverage*; the dummy variables equal one if the corresponding leverage variable is equal to zero. We define *FixedAssets* as fixed assets (fias) divided by total assets.

To study firm-level real-side effects, we use as outcome variables firms' growth, number of employees, 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 average total assets over the two fiscal year ends. *Employees* is the log number of employees (emp). *AverageWage* is the log of total wages (staf) divided by the number of employees. *Sales* is the log of sales (sale). *Profitability* is earnings before interest and taxes (ebta) divided by total assets. *LossDummy* is equal to one if *Profitability* is negative, else zero. *ProfitVolatility* is the standard deviation of *Profitability* over the most recent four years.

Our models use standard controls, such as *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 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 five or ten biggest French cities or the Paris Bourse. We use resources from geonames.org to identify the longitude and latitude associated with each French postal code, including those of the centers of the ten biggest cities. Using the AMADEUS and BANKSCOPE postal code variables, we assign longitudes and latitudes to

do not find significant differences in loan costs or maturity times across the two types of loans in our data.

[20]The analysis of the UNIDO data is restricted to the 2001–2007 period because data on gross fixed capital formation and value added are not available after 2007.

each sample firm as well as factoring companies. *NoFactoring* is a dummy variable equal to one if there are no factoring companies in the postal code area in which a firm is registered, and zero otherwise. To calculate a firm's distance to the closest of the five (*Distance5*) or ten (*Distance10*) largest French cities and its distance to the Paris Bourse (*DistanceBourse*), we calculate the spherical distance between points i and j using:

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

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 Contract-Level Variables

Our contract-level analysis uses loan mark-ups, time-to-maturity, and the number of lenders 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. *NumberLenders* is the log of the numbers of lenders involved in a loan. We use *Secured*, a dummy variable equal to one for secured loans and else zero, to assign firms to treatment status. Following Chava and Roberts (2008), Roberts and Sufi (2009), and Campello et al. (2011), the control variables in our loan contract model include firm size, age, profitability, total leverage, the existence of a credit rating, loan size, and loan type. *Rating* is a dummy variable that equals one if the firm taking out the loan is rated, else zero. *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 industrial 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.[21] Second, we create an external financial dependence index similar to Rajan and Zingales (1998). Following Larrain (2015), we define the index as the median ratio of capital expenditures net of cash flows from operations to total

[21]Both variables are GDP-deflated. Gross 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 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.

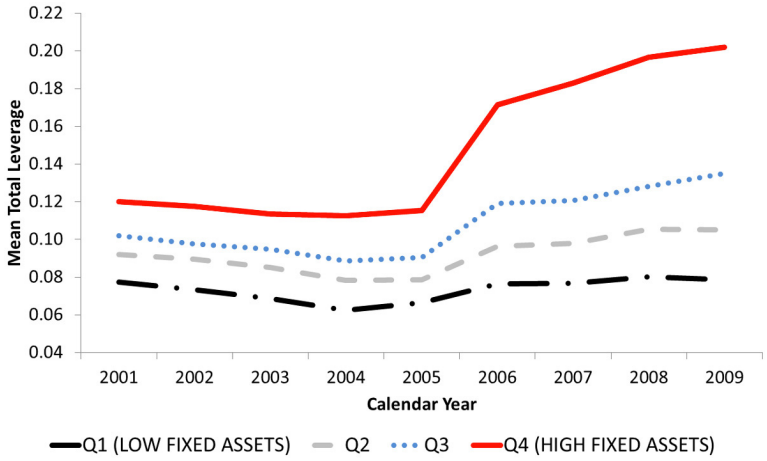

Figure 2. Evolution of Mean Total Leverage This figure shows the evolution of the mean total leverage ratio by fixed asset quartile (Q) over the 2001–2009 period. Q1, Q2, Q3, and Q4 indicate the first, second, third, and fourth fixed assets quartile, respectively.

capital expenditures for public U.S. firms. We calculate the median ratio per three-digit SIC industry and year and then average over the 1975–2005 period.

4 Financial Outcomes

4.1 Debt Usage

4.1.1 Total Debt

Figure 2 displays the mean total leverage ratios of firms in 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 2006 (the reform year). Notably, the effect of the reform is monotonically increasing in the rate with which firms employ fixed assets in their production process: the higher the use of fixed assets, the larger the increase in debt-taking after the reform.

Table 1 uses the PS-matched sample to study the reform's effect on total leverage. Column (1) shows that pre-reform mean total leverage hovers around 10%. During the reform year alone, however, it rises to nearly 14%, reaching 15% in the last year of the sample period. A comparison between average leverage ratios before and after the reform shows an increase of 4.3 percentage points. This increase is economically meaningful since it is equivalent to 43% of average total leverage before the reform.

TABLE 1 ABOUT HERE

In the next columns, we separately consider the fixed assets-intensive "treated" firms and their

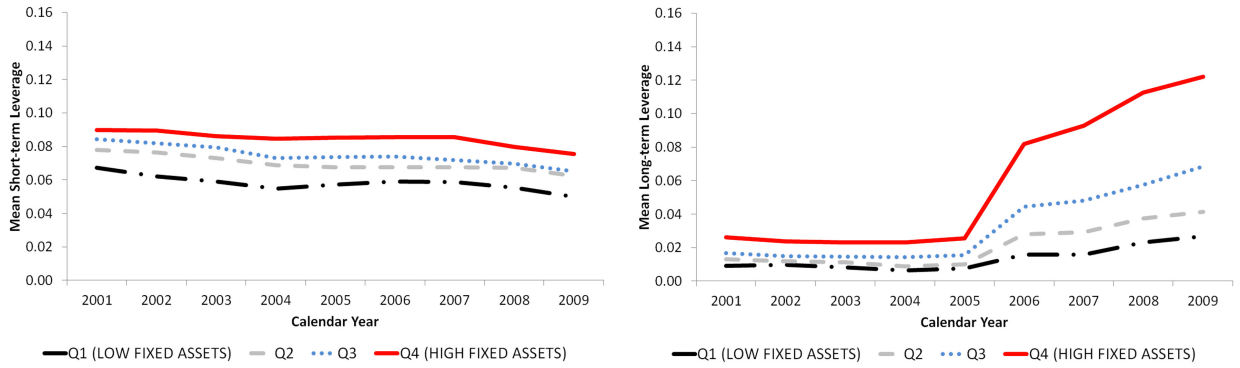

Figure 3. Evolution of Mean Short-term vs. Mean Long-term Leverage This figure shows the evolution of mean short-term and mean long-term leverage by fixed assets quartile (Q) over the 2001–2009 period. Q1, Q2, Q3, and Q4 indicate the first, second, third, and fourth fixed assets quartile, respectively.

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 increases in total leverage than the low-fixed assets firms in the reform year. A comparison of total leverage ratios before and after 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 five percentage points spread, the DID estimate of the reform on total leverage, is highly statistically significant. Since the difference is equal to 68% of average pre-reform total leverage, it is also economically meaningful.[22]

4.1.2 Short-term versus Long-term Debt

We split total leverage into short-term and long-term leverage. We do so because short-term debt is customarily secured by liquid — rather than hard — assets and is thus not expected to be particularly affected by the reform (Vernimmen et al. (2015)). Accordingly, Figure 3 shows that the mean short-term leverage ratios of all fixed assets quartiles remain fairly stable over our sample period. Long-term leverage ratios, in contrast, jump upward in the reform year, with effect magnitudes increasing monotonically over the fixed assets quartiles.

Table 2 repeats the former analysis using the PS-matched sample. Short-term leverage drops by around 3.8 percentage points over the 2001–2009 period. Notably, the high-fixed assets firms and low-fixed assets firms observe similar declines in short-term debt from the pre-reform to the post-reform period. In particular, high-fixed assets firms observe a 3.9 percentage points drop,

[22] We also studied the debt usage effects of the reform on firms with different fiscal year ends. Consistent with the reform being enacted in March 2006, high fixed assets firms with a fiscal year end of January, February, and March only display increases in their total leverage ratios in 2007, whereas firms with a fiscal year end between April and September (October and December) already display mild (pronounced) increases in 2006.

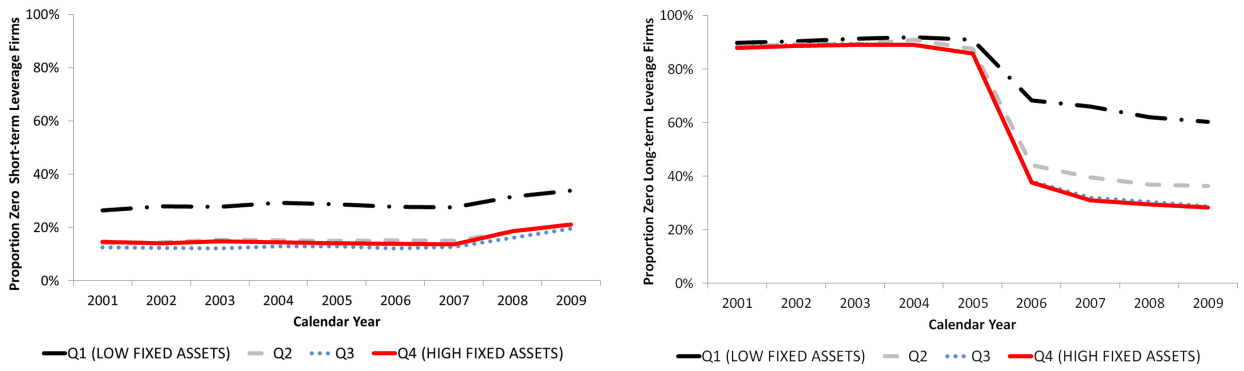

Figure 4. Evolution of the Proportions of Zero Short-term Leverage vs. Zero Long-term Leverage Firms This figure shows the evolution of the proportion of zero short-term and zero long-term leverage firms by fixed assets quartile (Q) over the 2001–2009 period. Q1, Q2, Q3, and Q4 indicate the first, second,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

while low-fixed assets firms observe a 3.8 points drop. The 0.1% difference, the DID estimate for the reform's effect on short-term leverage, is neither statistically nor economically significant.

TABLE 2 ABOUT HERE

In sharp contrast, the patterns in long-term leverage closely resemble those in total leverage. Mean long-term leverage remains almost flat at 1.7% until the reform, but climbs up to 6.7% on average after the reform. Conditioning on different fixed-assets categories, 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 five points DID spread is highly statistically significant. In economic terms, this spread is equivalent to nearly 300% of the average pre-reform long-term leverage.

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 or no long-term debt ("zero-leverage firms"). Figure 4 suggests that the proportion of zero short-term debt firms across all fixed-assets quartiles remains low and stable throughout the sample period. In contrast, there are striking reductions in the proportions of zero long-term leverage firms in the reform year. Notably, the magnitude of those reductions increase monotonically over the range of fixed-assets quartiles.

Results are similar when we perform PS-matched sample tests. Table 3 confirms that the proportion of zero short-term debt firms is stable over the sample period, both for the entire sample as well as the high- and low-fixed assets 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, declining 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 proportion of zero long-term debt firms in the high-fixed assets group drops by 60 points. The difference between the two numbers is statistically and economically significant.

TABLE 3 ABOUT HERE

4.1.4 Regression Results

The results in Tables 1, 2, and 3 could possibly be driven by unobservable firm- or industry-characteristics, or even macroeconomic time-variant effects.[23] To address concerns about confounding effects, Table 4 shows the results of DID regressions that use fixed-assets intensity as the identification wrinkle and include firm control variables, as well as firm-, industry-, and year-fixed effects. In the first two columns, we use as outcome variables long-term and short-term leverage, respectively. In the next two columns, we use dummy variables that equal one if long-term or short-term leverage is zero and else zero ("zero-leverage firms"), respectively. Even after accounting for the effects of the control variables and the fixed effects, our results remain. Notably, the control variables are highly significant with the expected signs.

TABLE 4 ABOUT HERE

Next, we repeat our DID regressions using the proximity to factoring companies for identification. Table 5 shows that the effects of the reform are more pronounced in areas in which there are no factoring companies. To wit, 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, Table 6 reports the results of DIDID regressions combining both identification strategies above. In areas with factoring companies, high-fixed assets firms observe a 3.4 percentage points greater increase in their long-term debt than low-fixed assets firms after the reform (see the slope coefficient of $Post \times Treated^{HighFixedAssets}$ in column (1)). Most notably, high-fixed

[23]Notably, as already depicted in the graphs above, excluding 2009 (the year the global financial crisis hits France) does not alter any of our empirical inferences.

assets firms located in areas with no factoring companies observe a 5.4 points greater increase in their long-term debt than low-fixed assets firms located in areas with factoring companies (note the sum of the slope coefficients on all three interaction terms). The effect is striking when compared to the average long-term leverage of firms with high-fixed asset usage (alternatively, firms with no access to factoring companies) prior to the 2006 collateral reform, which was 2.3% (or 1.4%). Estimates for the change in the proportion of zero-leverage firms point to a similar direction. Economically, they imply that the proportion of zero-leverage firms among high-fixed assets firms located in areas with no factoring companies drops by 24 percentage points more than the proportion of zero-leverage firms among low-fixed assets firms located in areas with factoring companies (consider again the sum of the slope coefficients on all three interaction terms). In stark contrast, no such effects are found for firms' short-term debt usage.

Table 6 About Here

The results from this section show that the passage of *Ordonnance 2006-346* allowed firms operating with more fixed assets and located further away from factoring companies to significantly raise their long-term borrowing (intensive margin). Additionally, the reform lowered the fraction of zero long-term debt firms among these firms (extensive margin). 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 among them dropped less significantly.

4.2 Loan Terms

We also study whether debt contract terms changed following the collateral reform. To do so, 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 became significantly cheaper and carried longer maturities 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 dropped by 1.29 more than the spread of unsecured loans, implying a 120 basis points greater reduction in loan mark-ups. The estimations also suggest that the maturity of secured loans increased by 41 months more than the maturity of unsecured loans — a significant amount of time. In addition to credit contract terms, we also study the number of lenders involved in secured and unsecured loans. We find that the number of lenders involved in secured loans increased from the pre-reform to the post-reform period relative to those involved in unsecured loans. Notably, coefficient estimates for our controls are consistent with those

found elsewhere in the literature (e.g., see Chava and Roberts (2008)).

TABLE 7 ABOUT HERE

Our evidence suggests that the 2006 collateral reform made banks more willing to extend secured credit on more favorable terms, supporting the argument that an increase in the collateral space has an easing effect on several dimensions of collateralized borrowing. In addition, our evidence also suggests that the 2006 collateral reform made it easier for secured loans to be syndicated under a larger number of lenders.

5 Who Benefitted from the Collateral Reform?

5.1 Attributes of Firms Accessing Credit after the Reform

Firms coming into credit markets following sweeping credit reforms are not necessarily those targeted by policymakers, and enlarged credit access may ultimately lead to undesirable economic outcomes (see, e.g., Lilienfeld-Toal et al. (2012) and Assunção et al. (2014)). Our analysis is unique in studying the outcomes of a credit reform that expanded the range of collateral that firms and borrowers could contract on, while keeping other parameters fixed. It is important to analyze the attributes of firms that accessed credit in this new contracting environment.

We study the characteristics of those firms that enjoyed the greatest gains in credit access under *Ordonnance 2006-346*, contrasting them to other groups of firms. In these tests, we place a special focus on firms that had no access to credit before the reform: likely "marginal borrowers" brought into credit markets by the reform. We start from the subsample of firms with zero long-term leverage in every pre-reform year. We split this subsample into two further subsamples, one subsample composed of firms that have a positive long-term leverage ratio in every post-reform year ("new borrowers") and another subsample composed of firms that continue to keep a zero long-term leverage ratio in every post reform-year ("non-borrowers").

Contrasting new borrowers with the average firm (compare columns (2) and (1)), Table 8 shows that new borrowers tend to be slightly smaller and more mature. In addition, they employ fewer people than the average firm. Notably, the profitability of new borrowers exceeds that of the average firm by 0.9 percentage points, and their profit volatility is 0.6 points lower than that of the average firm. New borrowers are also 4.3 percentage points less likely to run a loss than the average firm. New borrowers tend to hold less cash, have higher capital-to-labor ratios, and are more tangible-assets and fixed-assets intensive. They tend to be located further away

from the largest cities and the Paris Bourse.

TABLE 8 ABOUT HERE

Contrasting new borrowers with non-borrowers, we again find that new borrowers tend to be smaller, yet slightly more mature firms (compare columns (2) and (3)). In contrast to the former comparisons, however, new borrowers employ more workers than non-borrowers. Moreover, new borrowers tend to have higher profitability and lower profit volatility than non-borrowers, and they are less than half as likely to run a loss (6.6% versus 13.9%, respectively). New borrowers are more cash-strapped, have higher capital-to-labor ratios, and are more tangible-assets and fixed-assets intensive than non-borrowers. Notably, they are located farther away from the largest French cities and from the Paris Bourse than the non-borrowers.

The basic comparisons in Table 8 suggest that the collateral reform incentivized creditors to extend long-term financing to small, profitable, low-risk firms. One could argue that these firms are "desirable" marginal borrowers that were brought into credit markets by the reform. 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 France's countryside. We examine these distributional effects in more detail below.

5.2 The Financing of Start-Ups

Several studies argue that access to capital fosters entrepreneurship. Bertrand et al. (2007), Kerr and Nanda (2009), and Chatterji and Seamans (2012), for example, find that deregulation of the banking sector and credit card markets leads to more new business formation. It is argued that deregulation leads 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 triggered by *Ordonnance 2006-346*. Before the reform, start-ups had an average long-term leverage of about 2.6% in their year of incorporation. Remarkably, this figure increased to an average of 4.1% after the reform. Similarly, before the reform, 90% of all start-ups had no long-term debt in their year of incorporation. However, this figure dropped to 68% after the reform. More importantly, consistent with our prior findings, a firm's fixed assets usage conditions these results. In particular, the high-fixed assets start-up firms show a 18.7 percentage points greater drop in their proportion of zero long-term debt firms than the low-fixed assets start-ups.

TABLE 9 ABOUT HERE

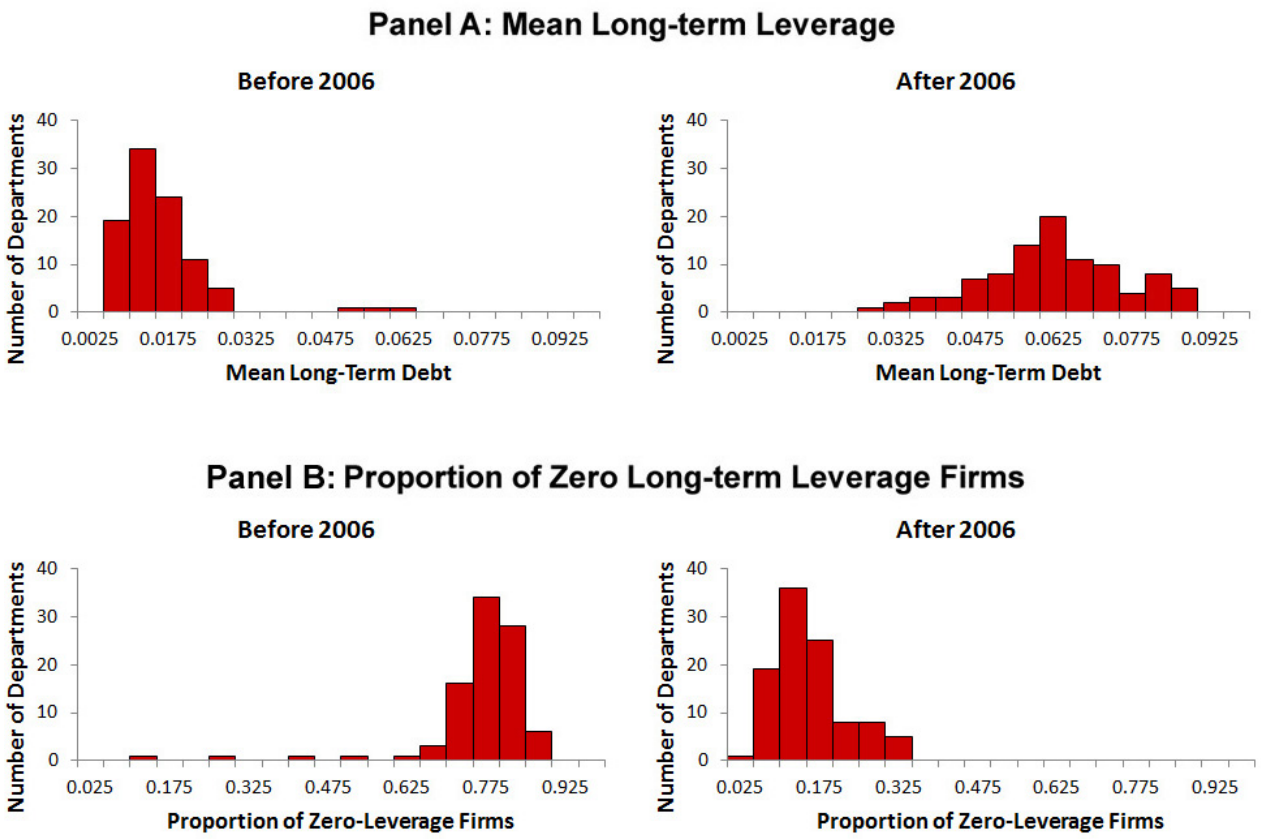

Figure 5. The Long-term Leverage Distribution of French Departments Before and After the Reform The histograms show the distribution of mean long-term leverage and the distribution of the proportion of zero long-term leverage firms for the 96 French departments and the pre- or post-reform period. The 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 Reform Effects

6.1 Effects of the Credit Reform Across Departments

Lilienfeld-Toal et al. (2012) argue that collateral reforms may redistribute credit from firms in rural areas toward those in larger cities. This dynamic raises policy concerns since firms in urban areas commonly have more options to raise capital than firms in rural areas.

Our setting and data allow us to study the geographic effects of a reform that enlarges access to collateral. We proceed in two steps. First, we study whether the 2006 reform affected the use of debt financing differentially across French departments. Figure 5 shows the distributions of departments' mean leverage ratios (Panel A) and proportions of zero leverage firms (Panel B) before and after *Ordonnance 2006-346*. Before 2006, the vast majority of departments had a mean long-term leverage between 1% and 3%, and a mean proportion of zero long-term leverage firms ranging between 60% and 90%. These figures changed significantly following the collateral

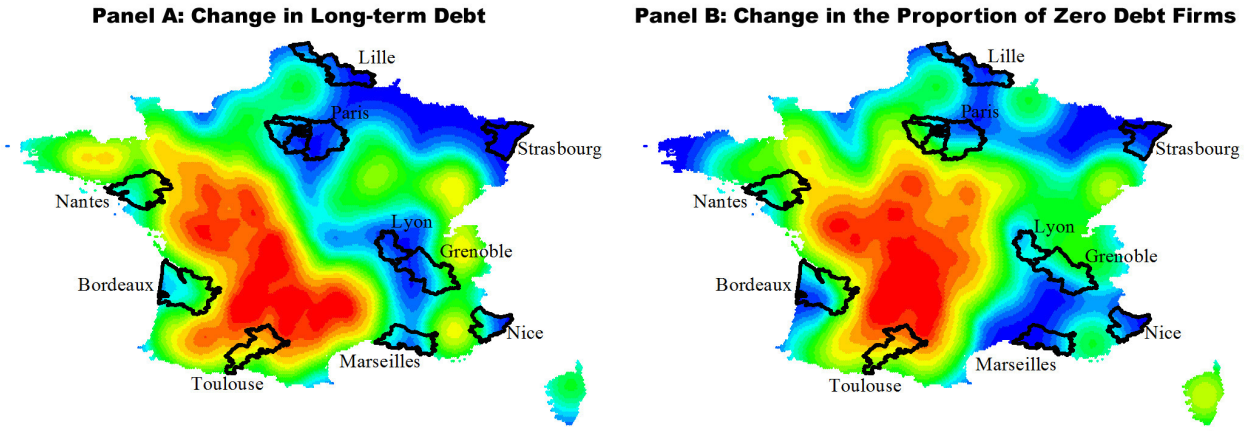

Figure 6. Change in Mean Long-term Leverage and Proportion of New Borrowers The maps show those geographical areas in France where the proportion of firms with an above 5% change in mean long-term leverage from the pre-reform to the post-reform period exceeds the median (Panel A) and where the proportion of firms with no long-term debt in any year preceding the reform, but with a positive long-term leverage ratio in every year proceeding the reform exceeds the median (Panel B). 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 more red (blue) a region's color in the maps, the greater (weaker) is the clustering of French departments with above median changes in mean long-term leverage (Panel A) or the proportion of new borrowers (Panel B). The black-rimmed departments in the maps are those featuring cities with more than one million inhabitants.

reform. Notably, the reform shifted the bulk of the mean long-term leverage distribution to the right (see right-side histogram in Panel A), so that after the reform most departments registered a mean long-term leverage ratio between 4% and 9%. Likewise, the reform shifted the bulk of the proportion of zero long-term leverage firms distribution to the left (see right-side histogram in Panel B), so that after the reform most departments registered a mean proportion of zero long-term leverage firms between 0% and 30%. The 2006 collateral reform changed the distribution of debt usage across virtually all French departments.

We next estimate the probability distribution of changes in corporate debt-taking across the country after the reform. We proceed as follows. We first identify firms with a change in mean long-term debt above 5% and select those geographical areas (departments) where the fraction of these firms is above the median. We then generate kernel estimates of the probability distribution of these areas. We also identify firms that never have any long-term leverage before the reform, but that report a positive long-term leverage ratio every year after the reform ("new borrowers"). We again select those geographical areas where the fraction of these firms is above the median. The estimation of the likelihood that firms in an area increase their long-term leverage above a certain threshold allows us to build "heat maps" of the change in credit utilization in France around *Ordonnance 2006-346*.[24]

[24]Spatial point analysis allows us to determine whether the distribution of an event s in a geographical surface R exhibits statistically significant clustering, as opposed to being random. To identify clustering, we estimate the probability density function of events, p(s). The function defines the probability of observing an event in

The heat maps are shown in Figure 6. A deeper red in the map indicates a greater clustering of areas significantly affected by the reform. The black-rimmed areas represent the departments featuring the largest ten French cities by inhabitants. The maps show that both the largest increases in leverage ratios and the largest drops in the proportion of zero leverage firms occur far away from the metropolitan areas. The inference one draws from this experiment is striking. The reform did not favor firms located in financial centers in accessing new credit. Instead, it channeled the largest amount of credit to firms in rural areas.

6.2 Changes in Credit Access Inequality

Our results show that the collateral reform produced the largest increases in long-term borrowing among firms that often have less access to capital markets, such as small firms or firms registered in rural areas. They suggest that the reform spurred a "democratization of credit." We study this idea further by computing the Gini index measure of credit access inequality at the departmental level. The Gini index is computed as follows:

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

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 first calculate $GiniIndex_{k,t}$ by department and year and then average by department across pre-reform versus post-reform periods. To avoid confounding effects arising from changes in sample composition, the calculation uses a fixed number of observations (the 20,000 largest firms by their total assets values) in each year. The Gini index ranges from zero to one. A higher value indicates a more unequal distribution of long-term debt usage within a department.

Figure 7 shows each department's pre- and post-reform Gini index values. Red (blue) departments have a relatively high (low) index value. Before the reform, only seven out of 96 departments had an index value below 0.90, suggesting that long-term debt was heavily concentrated in the hands of a few large firms. Following the reform, the index values dropped significantly. Most notably, while all departments observed declines in their index values, it was

a certain location within R and can be estimated by means of non-parametric methods; for example, kernel estimators. In our case, the surface R represents France and is divided into a set of more than 60,000 contiguous cells (grid). The events of interest are: (1) companies with large changes in mean long-term debt; and (2) companies that never have any long-term debt before the reform, but that have positive long-term debt every year after the reform. Having identified these companies, we select the corresponding geographical coordinates of the areas where the proportion of these companies are above the median. Based on these coordinates, we follow Bailey and Gatrell (1995) in using kernel estimators to generate a spatially smooth estimate of p(s) over the grid of cells covering R. The maps display the results of this estimate.

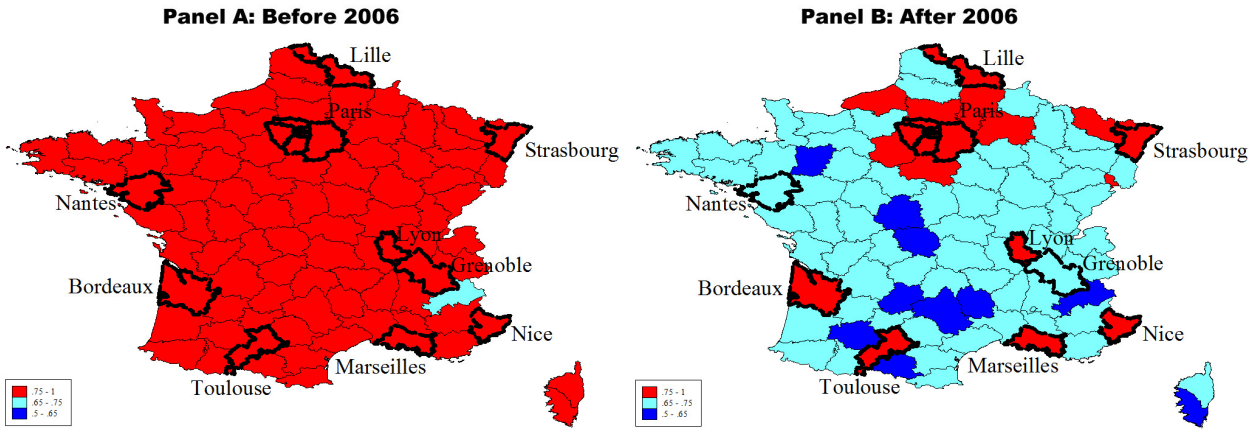

Figure 7. Pre- and Post-Reform Long-term Debt-Gini Index By Department The maps show each department's long-term debt Gini index value for the pre-reform period (Panel A) and the post-reform period (Panel B). We calculate the Gini index by department and year and then average by department and pre-reform period or post-reform period. The calculation only uses the 20,000 largest firms (in terms of their total assets) in each year. Red (blue) departments have a high (low) Gini index value. The black-rimmed departments are those containing cities with more than one million inhabitants.

the rural departments that experienced the most dramatic drops. The results are remarkable in showing that the collateral reform greatly decreased inequalities in the access to credit, with firms in the countryside experiencing the greatest gains in credit access.

7 Real Outcomes

We conclude our analysis investigating how firms used the credit made available by the collateral reform. If firms used the new capital to invest into profitable projects, the resulting increase in their performance could be seen as a positive consequence of credit expansion. If, on the other hand, the impact of the reform was simply an increase in firm indebtness, one would worry about increased rates in corporate default and the negative consequences of extending credit to marginal firms in the economy. Understanding the ultimate consequences of credit creation is of much interest for researchers and policymakers alike.

7.1 Firm Performance

Table 10 shows the results of DID regressions featuring the following outcome variables: fixed investment, number of employees, average wage, sales, profitability, and profit volatility. Our evidence suggests that the firms that benefitted the most from the reform-induced credit access (high-fixed assets firms) increased investment more and hired more people after the reform. High-fixed assets firms observed a 5.1 percentage points higher employment growth than low-fixed

assets firms. Interestingly, the average wage paid by high-fixed assets firms decreased relative to the average wage paid by low-fixed assets firms, suggesting that the high-fixed assets firms hired cheaper labor. In addition, high-fixed assets firms raised their output more and became more profitable as well as less risky after the reform. For example, high-fixed assets firms observed a 4.1 percentage points higher sales growth than low-fixed assets firms after the reform.

TABLE 10 ABOUT HERE

7.2 Firm Survival

We also study firms' default rates. Arguably, changes in default rates following the reform more directly measure whether the reform has inadvertently extended credit to risky borrowers at the margin of the capital markets. We use a proportional Cox hazard model to fit the number of years until a firm fails. This model assumes that the hazard rate is given by:

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

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 identify 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).[25] 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 obtain estimates for β and γ without imposing any structure on ϕ_t. The same estimator is also able to account for right-censoring of data due to firms that do not fail within the sample period or that leave the sample for reasons unrelated to performance.

Table 11 reports the results from the proportional Cox hazard models. In the absence of controls, firms operating more fixed assets experience a 28.6 percentage points ($e^{-0.338}$) greater decline in their failure rates than other firms after the reform. 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, adding profitability and profit volatility reduces 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

[25] The failure statuses are: "default of payment," "insolvency proceedings," "receivership," "bankruptcy," "dissolved (bankruptcy)," "dissolved (liquidation)," and "in liquidation."

firms after the passage of the collateral reform.

TABLE 11 ABOUT HERE

7.3 Capital Allocation Efficiency

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

First, we follow Wurgler's (2000) argument that an efficient capital allocation process implies that corporate investment increases in sectors with better growth opportunities and decreases in sectors with worse growth opportunities. Using industry value added-growth as proxy for growth opportunities, we estimate the sensitivity of investment to value-added growth as a way to gauge the efficiency of capital allocation. We study how this sensitivity changes from the pre- to the post-reform period. We run the following regression on industry-level data:

$$InvestmentGrowth_{k,t} = \alpha + \beta ValueAddedGrowth_{k,t} + \varepsilon_{k,t}, \tag{7}$$

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 reports that the elasticity between investment growth and value-added growth (β) is 0.43. Using post-reform data, it jumps to 0.70. The difference between the numbers is 0.27. Based on Wurgler's (2000) interpretation of the elasticities, the reform led to an 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 toward those sectors that need more external funding. If the reform relaxed credit constraints, we should observe significantly larger increases 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. The control status is assigned to other PS-matched firms.

Table 13 reports that firms with a high dependence on external financing observed greater reform-induced increases in their long-term leverage than all other firms. While the mean long-term leverage ratio of highly dependent firms increased by 6.3 percentage points (see column (1)), the mean long-term leverage ratio of firms with a weaker dependence increased by only 3.6 points (column (4)). The difference is statistically and economically significant. More importantly, the difference is larger for treated firms. Highly dependent high-fixed assets firms observed a mean increase in their long-term leverage of 7.5 percentage points.

Table 13 About Here

Taken together, our tests suggest that the 2006 collateral reform helped the French economy move toward allocating more capital to high-value added sectors as well as sectors that require more external financing. These findings point to positive welfare gains achieved by the reform in reducing credit distortions.

8 Robustness

8.1 Self-Selection and Parallel Trends

The validity of DID tests rests on a number of assumptions, one of which 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 seem to benefit from changing security laws. 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 fixed assets quartile. 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 applies, treated and control firms should display parallel trends in outcome variables before the reform. We have shown 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.

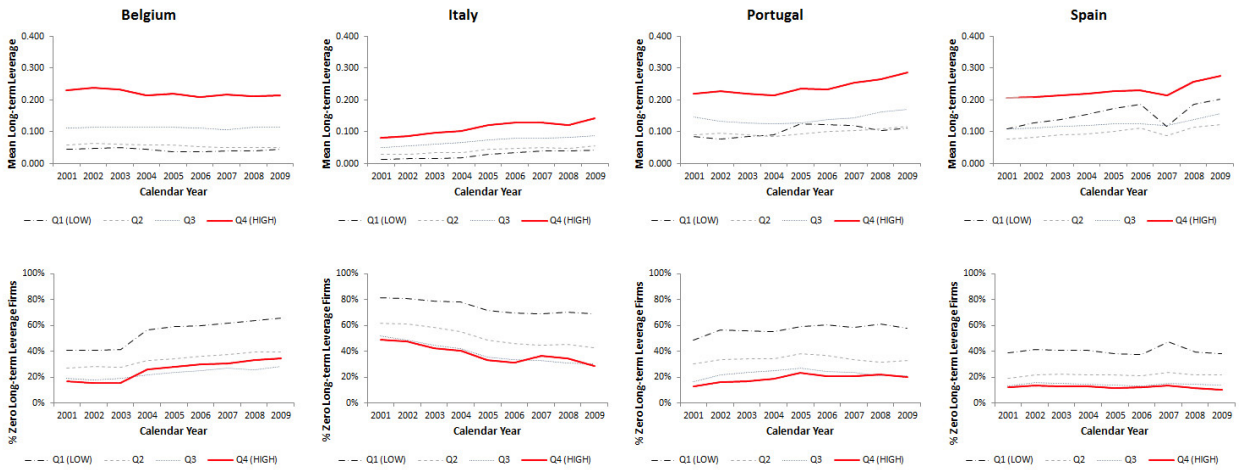

Figure 8. Evolution of Mean Long-term Leverage and the Proportion of Zero Long-term Leverage Firms in the Placebo Countries The figure shows the evolution of mean long-term leverage (upper panels) and the proportion of zero long-term leverage firms (lower panels) for each placebo country and fixed assets 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.

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 those authors, we separately average each variable analyzed over the pre- and post-reform periods and repeat our 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 take on secured credit facilities under exceptional circumstances; for example, when they are distressed or when they borrow for project financing purposes. We use this insight to replicate our tests on public firms data, expecting that their borrowing is less affected by the reform than the borrowing of private firms. Table 15 confirms this intuition. The table shows that, while public firms experience a slight increase in long-term leverage from the pre- to the post-reform period, there is no evidence to suggest that the increase is driven by high-fixed assets firms. In fact, if anything, results work in the opposite direction. While firms in the low-fixed assets group raise their leverage, firms in the highest-fixed assets group reduce it significantly. Running DID regressions on public firms confirms these inferences.

TABLE 15 ABOUT HERE

8.3.2 Other Civil Law Countries

One concern with our results is that changes affecting firms across Europe in 2006 (and not necessarily *Ordonnance 2006-346*) may explain the growth in debt taking by high-fixed asset firms in France. As a final falsification test, we repeat our analysis using data from European Civil Law countries that already had reformed their security laws at the start of our sample period: Belgium (a neighbor with close economic ties), Italy, Spain, and Portugal. Figure 8 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 infer 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 Concluding Remarks

Studies show that security law reforms can sometimes raise the volume of corporate borrowing (see, e.g., Campello and Larrain (2016)). But raising the volume of corporate borrowing is arguably only the tip of the iceberg. The crucial question is whether those reforms open up credit to firms that have historically been credit-rationed; often firms that are small, young, and located away from financial centers (in rural areas). Opening up credit to such firms can be critical since they often contribute significantly to a country's common wealthfare.

We offer a comprehensive analysis of the effects that security law reforms can bring to bear. We not only look at changes in debt usage ratios, but also — and more importantly — at the composition of debt, the demographics of corporate debt usage, and ultimately at real capital allocation effects. To achieve these goals, we consider a security law reform recently implemented by a developed country: France's *Ordonnance 2006-346*.

Our evidence suggests that the firms expected to be most affected by the collateral reform — those firms whose asset composition is heavily tilted toward fixed assets and which are located in areas without factoring companies — significantly increased their long-term leverage. The fraction of zero debt firms among them also dropped significantly. Low-fixed assets firms or firms located in areas with factoring companies observe no such patterns. Loan contract data further suggest that the credit expansion was driven by cheaper and more long-term secured credit, leading us to conclude that the reform made debt contracting more attractive.

Our evidence also speaks to how security law reforms can change the demographics of corporate debt utilization. We show that a large amount of the newly created debt went to small, profitable firms located in rural areas. The reform also led to increases in long-term debt usage among start-ups. By easing up access to credit, the reform produced significant

decreases in the Gini index of credit access inequality all over the country. Taken all together, our evidence suggests that the reform produced a "democratization" of corporate credit.

The results from our study suggest that firms used the new debt financing in welfare-enhancing ways. The firms that benefitted the most from the collateral reform became more profitable, less risky, and less likely to fail. In addition, the reform led to a significant increase in the elasticity between investment and value added. It also allowed for significantly more borrowing by firms operating in industries heavily reliant on external financing. All combined, these results point to a significant improvement in capital allocation efficiency in the economy. This is an important effect from a policymaking perspective since reforms of this type may require less resources to implement and maintain than reforms that focus, for example, on law enforcement and judicial intervention.

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Table 1
Effect of the Reform on Total Leverage: PS-Matched 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 has a fixed assets-to-total assets ratio in the top quartile of that asset distribution. We estimate the model within each industry using only data from the year directly preceding the reform (2005). As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with the one firm belonging to the other quartiles that has the closest fitted probability. 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. Standard errors are clustered at the firm level.

		Fixed Assets Group		Diff.
Year	All	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***	0.050***

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 2
Effect of the Reform on Mean Short-term and Long-term Leverage: PS-Matched 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 leverage; the lower number is long-term 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 has a fixed assets-to-total assets ratio in the top quartile of that asset distribution. We estimate the model within each industry using only data from the year directly preceding the reform (2005). As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with the one firm belonging to the other quartiles that has the closest fitted probability. 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. Standard errors are clustered at the firm level.

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

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 3
Effect of the Reform on the Proportions of Zero Short-term and Long-term Leverage Firms: PS-Matched 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 leverage, the lower number long-term 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 has a fixed assets-to-total assets ratio in the top quartile of that asset distribution. We estimate the model within each industry using only data from the year directly preceding the reform (2005). As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with the one firm belonging to the other quartiles that has the closest fitted probability. 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. Standard errors are clustered at the firm level.

	Debt		**Fixed Assets Group**		Diff.
Year	Type	All	High	Low	High–Low
2001	Short-term	0.153	0.128	0.171	-0.043***
	Long-term	0.885	0.889	0.882	0.007
2002	Short-term	0.145	0.116	0.165	-0.049***
	Long-term	0.890	0.893	0.889	0.004
2003	Short-term	0.147	0.122	0.165	-0.043***
	Long-term	0.896	0.899	0.893	0.006
2004	Short-term	0.153	0.131	0.172	-0.041***
	Long-term	0.899	0.898	0.900	-0.002
2005	Short-term	0.142	0.123	0.162	-0.039***
	Long-term	0.864	0.868	0.860	0.008
2006	Short-term	0.137	0.117	0.154	-0.037***
	Long-term	0.421	0.355	0.477	-0.123***
2007	Short-term	0.144	0.121	0.162	-0.041***
	Long-term	0.365	0.285	0.428	-0.142***
2008	Short-term	0.183	0.172	0.191	-0.018**
	Long-term	0.348	0.274	0.402	-0.129***
2009	Short-term	0.209	0.193	0.221	-0.028***
	Long-term	0.334	0.254	0.388	-0.134***
Mean 2001–2005 (1)	Short-term	0.148	0.124	0.167	
	Long-term	0.887	0.889	0.885	
Mean 2006–2009 (2)	Short-term	0.168	0.151	0.182	
	Long-term	0.367	0.292	0.424	
Diff. (2)–(1)	Short-term	0.020***	0.027***	0.015***	0.012*
	Long-term	-0.520***	-0.597***	-0.461***	-0.136***

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 4
Effect of the Reform on Leverage: DID Regressions Using Fixed Assets as Identification Variable
The table shows the results from the following regression:

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

where $Y_{i,t}$ is long-term leverage, short-term leverage, or dummy variables indicating whether the former quantities are zero. Long-term leverage is long-term debt scaled by total assets; short-term leverage is short-term debt scaled by total assets. Dummy no long-term (short-term) leverage is a dummy variable equal to one if long-term (short-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. $Treated^{HighFixedAssets}$ 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. T-statistics (in parentheses) are calculated from standard errors clustered at the firm-level.

	Dependent Variable			
	Intensive Margin		Extensive Margin	
	Long-term Leverage	Short-term Leverage	Dummy No Long-term Leverage	Dummy No Short-term Leverage
Post × *Treated*^{HighFixedAssets}	0.044***	-0.002*	-0.121***	0.006
	(32.53)	(-1.85)	(-19.27)	(1.60)
Size	0.008***	0.013***	-0.047***	-0.077***
	(11.88)	(13.53)	(-15.45)	(-26.91)
Profitability	-0.050***	-0.127***	0.105***	0.095***
	(-16.44)	(-32.75)	(7.46)	(7.60)
Age	-0.003**	0.001	0.079***	-0.044***
	(-2.32)	(0.81)	(11.78)	(-8.02)
R-squared	0.058	0.007	0.210	0.007
Observations	228,782	228,782	228,782	228,782

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

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

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

where $Y_{i,t}$ is long-term leverage, short-term leverage, or dummy variables indicating whether the former quantities are zero. Long-term leverage is long-term debt scaled by total assets; short-term leverage is short-term debt scaled by total assets. Dummy no long-term (short-term) leverage is a dummy variable equal to one if long-term (short-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. $Treated^{NoFactoring}$ 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. T-statistics (in parentheses) are calculated from standard errors clustered at the firm-level.

	Dependent Variable			
	Intensive Margin		Extensive Margin	
	Long-term Leverage	Short-term Leverage	Dummy No Long-term Leverage	Dummy No Short-term Leverage
Post × $Treated^{NoFactoring}$	0.010***	0.003***	-0.122***	0.002
	[10.74]	[3.07]	[-21.73]	[0.67]
Size	0.008***	0.013***	-0.045***	-0.077***
	[10.82]	[13.52]	[-14.90]	[-26.87]
Profitability	-0.049***	-0.127***	0.095***	0.096***
	[-15.79]	[-32.74]	[6.77]	[7.62]
Age	-0.002	0.001	0.071***	-0.044***
	[-1.16]	[0.95]	[10.58]	[-7.89]
R-squared	0.028	0.007	0.218	0.006
Observations	226,800	226,800	226,800	226,800

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 6
Effect of the Reform on Leverage: DIDID Regressions Using Fixed Assets and Lack of Financing Companies as Identification Variables
The table shows the results from the following regression:

$$\begin{aligned} Y_{i,t} &= \alpha_i + \alpha_k + \alpha_t + \beta Post_t \times Treated_i^{HighFixedAssets} + \delta Post_t \times Treated_i^{NoFactoring} \\ &\quad + \gamma Post_t \times Treated_i^{HighFixedAssets} \times Treated_i^{NoFactoring} + \mathbf{X}_{i,t}\eta + \varepsilon_{i,t}, \end{aligned}$$

where $Y_{i,t}$ is long-term leverage, short-term leverage, or dummy variables indicating whether the former quantities are zero. Long-term leverage is long-term debt scaled by total assets; short-term leverage is short-term debt scaled by total assets. Dummy no long-term (short-term) leverage is a dummy variable equal to one if long-term (short-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. $Treated^{HighFixedAssets}$ 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. $Treated^{NoFactoring}$ 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, δ, γ, and η are free parameters. α_i, α_k, and α_t indicate firm-, industry-, and year-fixed effects. T-statistics (in parentheses) are calculated from standard errors clustered at the firm-level.

	Dependent Variable			
	Intensive Margin		Extensive Margin	
	Long-term Leverage	Short-term Leverage	Dummy No Long-term Leverage	Dummy No Short-term Leverage
$Post \times Treated^{HighFixedAssets}$	0.034***	-0.007***	-0.105***	0.021***
	[16.05]	[-3.26]	[-10.02]	[3.23]
$Post \times Treated^{NoFactoring}$	0.006***	0.001	-0.116***	0.008**
	[7.09]	[1.23]	[-17.98]	[1.98]
$Post \times Treated^{HighFixedAssets} \times Treated^{NoFactoring}$	0.015***	0.007***	-0.021	-0.024***
	[5.37]	[2.82]	[-1.64]	[-2.96]
Size	0.008***	0.013***	-0.047***	-0.077***
	[11.72]	[13.46]	[-15.46]	[-26.82]
Profitability	-0.049***	-0.127***	0.096***	0.095***
	[-15.99]	[-32.73]	[6.84]	[7.60]
Age	-0.003**	0.002	0.075***	-0.044***
	[-2.07]	[1.00]	[11.14]	[-7.94]
R-squared	0.059	0.007	0.221	0.006
Observations	226,800	226,800	226,800	226,800

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

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; *LoanSpread*), the loan time-to-maturity, defined as the log of the difference between the loan's maturity date and its initiation date (in months; *LoanMaturity*), or the log of the number of lenders involved in a loan (*NumberLenders*). *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. *LoanAmount* 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 fixed effects and year fixed effects, respectively. Industry fixed effects are defined based on 2-digit SIC codes. $\varepsilon_{i,j,t}$ is the residual. T-statistics (in parentheses) are calculated from standard errors clustered at the firm-level.

	Dependent Variable					
	Loan Spread		Loan Maturity		Number of Lenders	
Post × *Treated*^{Secured}	-1.303***	-1.275***	0.803***	0.879***	0.350***	0.380***
	(-4.17)	(-5.33)	(6.45)	(6.79)	(4.70)	(3.81)
Size		0.022		-0.020		0.072
		(0.87)		(-1.28)		(0.94)
Age		-0.042		0.035***		0.106***
		(-1.62)		(4.08)		(4.33)
Profitability		0.252		-0.082		0.064
		(0.61)		(-0.37)		(0.13)
TotalLeverage		-0.054		0.100		-0.089
		(-0.35)		(0.73)		(-0.39)
Rating		-0.022		0.165*		-0.105
		(-0.09)		(1.94)		(-1.13)
LoanAmount	-0.080***	-0.095***	-0.010	0.001	0.186***	0.156***
	(-4.17)	(-7.07)	(-0.55)	(0.08)	(7.84)	(3.42)
LoanType	0.323***	0.334***	0.018	0.020	-0.240***	-0.243***
	(11.50)	(9.94)	(0.54)	(0.58)	(-4.86)	(-3.63)

(continued on next page)

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

	Dependent Variable					
	Loan Spread		Loan Maturity		Number of Lenders	
LoanMaturity	0.258***	0.330***			-0.126***	-0.034
	(5.45)	(6.08)			(-2.98)	(-0.82)
NumberLenders	0.107***	0.113**	-0.060***	-0.015		
	(2.92)	(2.23)	(-3.33)	(-0.74)		
LoanSpread			0.212***	0.240***	0.185***	0.183**
			(3.84)	(4.72)	(2.72)	(2.38)
$Treated^{Secured}$	1.614***	1.589***	-0.476***	-0.594***	-0.853***	-0.710***
	(16.71)	(8.69)	(-3.92)	(-3.16)	(-8.26)	(-5.93)
R-squared	0.481	0.514	0.333	0.429	0.403	0.419
Observations	454	407	454	407	454	407

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 8
Comparison of New Borrowers and Non-Borrowers

The table compares several firm characteristics across the average firm, firms that strongly benefitted from the reform, and firms that did not strongly benefit from the reform. In column (1), we consider the whole sample. From the firms that never hold any long-term debt before the reform, "new borrowers" are those that have a positive long-term leverage in every year after the reform (column (2)), while "non-borrowers" are those that continue to never hold any long-term debt after the reform (column (3)). The difference between new borrowers and the average firm (non-borrowers) is given under "Diff (2)–(1)" ("Diff (2)–(3)"). 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.

	All Companies (1)	Companies without Long-term Debt Before 2006 But Positive Long-term Debt in All Post-Reform Years ("New Borrowers") (2)	Companies without Long-term Debt Before 2006 and No Long-term Debt in All Post-Reform Years ("Non-Borrowers") (3)	Diff. (2)–(1)	Diff. (2)–(3)
Assets (in million $)	28.70	25.12	28.96	-3.58***	-3.83***
Firm Age (in years)	23.03	23.93	22.08	0.90***	1.85***
Number of Employees	146.90	142.75	123.20	-4.15	19.55
Operating Profit	0.102	0.111	0.097	0.009***	0.015***
Operating Profit Volatility	0.045	0.039	0.050	-0.006***	-0.012***
Loss Dummy	0.109	0.066	0.139	-0.043***	-0.073***
Cash Reserve	0.115	0.107	0.137	-0.008***	-0.030***
Capital-to-Labor	2.50	2.73	2.16	0.24***	0.57***
Tangible Assets	0.134	0.165	0.095	0.032***	0.070***
Fixed Assets	0.233	0.270	0.179	0.037***	0.091***
Distance to Top 5 City	75.74	89.71	58.07	13.97***	31.65***
Distance to Top 10 City	42.39	51.04	31.81	8.64***	19.22***
Distance to Capital Market	167.38	190.63	128.04	23.25***	62.59***

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 9
Effect of the Reform on the Leverage of Start-Ups: PS-Matched ANOVA Tests
The table gives descriptive statistics on the long-term leverage of start-ups in their year of incorporation. The table considers the whole sample ("All") and the treated- and control-firm subsamples. 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 has a fixed assets-to-total assets ratio in the top quartile of that asset distribution. As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with the one firm belonging to the other quartiles that has the closest fitted probability. The table reports averages over the annual pre-reform period (2001–2005) averages, averages over the annual post-reform period (2006–2009) averages, and their differences. The final column shows the differences in attributes across the high and low fixed assets group. Standard errors are clustered at the firm level (whenever possible).

		Fixed Assets Group		Diff.
Year	All	High	Low	High–Low
Panel A: Mean Long-term Leverage				
Mean 2001–2005 (1)	0.026	0.037	0.016	
Mean 2006–2009 (2)	0.041	0.060	0.021	
Diff. (2)–(1)	0.015**	0.023**	0.005	0.018
Panel B: Proportion No Long-term Leverage Firms				
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***

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 10
Effect of the Reform on Firm Performance: DID Regressions
The table shows the results from the following regression:

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

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. *AverageWage* 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. *ProfitVolatility* is the standard deviation of *Profitability* over the most recent four fiscal years, including the most recent one. We set *ProfitVolatility* 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. $Treated^{HighFixedAssets}$ 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. T-statistics (in parentheses) are calculated from standard errors clustered at the firm-level.

	Dependent Variable					
	Growth	Employees	Average Wage	Sales	Profitability	Profit Volatility
Post × $Treated^{HighFixedAssets}$	0.006***	0.051***	-0.017***	0.041***	0.005***	-0.002***
	(6.92)	(7.04)	(-6.16)	(5.68)	(4.42)	(-4.12)
Size	0.046***	0.503***	0.020***	0.780***	0.012***	-0.010***
	(49.20)	(59.63)	(6.56)	(102.63)	(12.47)	(-16.94)
TotalLeverage	-0.021***	-0.011	-0.009	-0.224***	-0.106***	0.012***
	(-7.70)	(-0.66)	(-1.30)	(-13.67)	(-36.46)	(8.22)
Age	-0.045***	0.171***	0.014***	0.212***	0.021***	-0.018***
	(-22.00)	(18.62)	(3.45)	(24.64)	(14.01)	(-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

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table 11
Effect of the Reform on Failure Rates: DID Regressions
The table shows the results from the following proportional Cox hazard model:

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

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. $Treated^{HighFixedAssets}$ 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. T-statistics (in parentheses) are calculated from standard errors clustered at the firm-level.

Dependent Variable = Time-to-Failure			
$Post \times Treated^{HighFixedAssets}$	-0.338***	-0.451***	-0.204**
	(-4.36)	(-5.62)	(-2.13)
TotalLeverage		1.196***	0.490***
		(7.70)	(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

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

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 $ValuedAddedGrowth_{k,t}$ 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: PS-Matched ANOVA Tests

The table shows the mean long-term leverage ratio of all firms ("All"), high fixed assets-, and low fixed-assets firms, separately for those with a high or low dependence on external financing. 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 proxy for 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 has a fixed assets-to-total assets ratio in the top quartile of that asset distribution. We estimate the model within each industry using only data from the year directly preceding the reform (2005). As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with the one firm belonging to the other quartiles that has the closest fitted probability. 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 all with a low external financial dependence. Standard errors are clustered at the firm level.

	External Financial Dependence						High–Low External Financial Dependence Across	
	High			Low				
Year	All (1)	Treated (2)	Controls (3)	All (4)	Treated (5)	Controls (6)	All (1)–(4)	Treated (2)–(5)
2001	0.019	0.024	0.015	0.019	0.025	0.016	0.000	-0.001
2002	0.018	0.022	0.014	0.018	0.024	0.015	0.000	-0.003
2003	0.017	0.021	0.014	0.017	0.023	0.015	0.000	-0.002
2004	0.017	0.019	0.015	0.017	0.024	0.013	0.000	-0.005**
2005	0.021	0.024	0.018	0.018	0.024	0.016	0.003**	0.000
2006	0.069	0.082	0.056	0.047	0.061	0.039	0.023***	0.020***
2007	0.074	0.089	0.058	0.049	0.066	0.041	0.025***	0.023***
2008	0.089	0.105	0.071	0.059	0.074	0.051	0.030***	0.031***
2009	0.098	0.116	0.077	0.064	0.083	0.055	0.034***	0.033***
Mean 2001–2005 (1)	0.019	0.022	0.015	0.018	0.024	0.015	0.001	-0.002**
Mean 2006–2009 (2)	0.082	0.097	0.065	0.054	0.070	0.046	0.028***	0.027***
Diff. (2)–(1)	0.063***	0.075***	0.050***	0.036***	0.046***	0.031***	0.027***	0.029***

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

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: PS-Matched 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 has a fixed assets-to-total assets ratio in the top quartile of that asset distribution. We estimate the model using only data from the year directly preceding the reform (2005). As covariates, we use firm size, profitability, and leverage. We match each firm belonging to the top fixed assets-to-total assets ratio quartile with the one firm belonging to the other quartiles that has the closest fitted probability. 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. Standard errors are clustered at the firm level.

		Fixed Assets Group		Diff.
Year	All	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)	0.015	-0.053	0.044*	-0.097**

***, **, and * indicate statistical significance at the 99%, 95%, and 90% confidence levels, respectively.