

Credit constraints and job destruction: Evidence from Denmark

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Abstract

During the financial crisis, credit was reduced for many firms, and employment decreased everywhere and in most sectors. In this paper, we test whether there is a link between the financial system and job destruction through a decline in credit availability. We construct a new data set that combines firm-level information and bank-level information, and to establish causality, we exploit the exogenous variation in banks' loan supply induced by injunctions from the financial regulator to increase solvency. Using information on almost 15,000 Danish firms over the 2010 to 2015 period, we find that injunctions have a serious negative impact on the amount of firm-level debt. We further find that firms do not substitute the lost bank financing with equity; instead, they scale down their activities and lower their wage costs. We also find that larger banks are more likely to transmit the effect of an injunction to their smaller customers (by constraining credit), whereas smaller banks are more likely to shield their corporate customers irrespective of size.

JEL classification: E51; G21; G28; G32

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1. Introduction

Credit was one of the first casualties of the Great Recession. Immediately after the subprime crisis started in mid-2007, interbank markets became strained, and in the year following the Lehman Brothers bankruptcy, bank credit around the world significantly decelerated (Aisen and Franken, 2010), with real effects on firm growth and employment. Campello, Graham, and Harvey (2010) find that firms that were credit constrained during the crisis of 2008 cut back on investments and employment. Laeven and Valencia (2013) find that constrained credit supply reduced growth in value added in the 2008-2009 period, and Chodorow-Reich (2014) finds that constrained credit supply destroyed jobs in small and medium sized firms in the same period.

These papers use variation in firm level assessments by CFOs, bank level health measurement, and differences in countries' policy interventions to restore bank capital in order to make inferences about the causal effects. In this paper, we make use of unique insights into a financial supervisory authority's bank inspections, and we contribute to the literature by using a difference-in-difference approach to provide causal evidence that changes in banks' minimum capital requirements also have real effects on employment. This is done on a panel of recent data in the context of a small open economy with a large banking sector that has significant international reach. The large overweight of small and medium sized private firms makes for another contribution to this particular research question (Akbar, Rehman, and Ormrod, 2013).

Ideally, financial systems efficiently allocate resources to productive use, i.e., to investment opportunities with positive net present values, and economic growth follows by way of capital accumulation and technological innovation (La Porta et al. 1997, 1998; Levine, 1997; Rajan and Zingales, 1998; Beck et al., 2000; Wurgler, 2000). Imperfections such as asymmetric information and transaction costs may nevertheless disturb this harmony. Financial intermediaries may not be able to overcome all of these imperfections (Stiglitz and Weiss, 1981), but they themselves may also be subject to some of them, which means that finance does not necessarily follow where enterprise leads.

Put differently, there is a supply side and a demand side that do not automatically result in the socially optimal equilibrium, and while there is evidence of a bank lending channel, the slow recovery from the crisis could also result from firms' lower demand for credit or lower-quality investment opportunities, in which case the banks would service the economy by limiting overinvestments. In this paper, we examine whether the observed real

effects on firms are due to credit constraints or a lack of good investments. We develop a robust empirical strategy based on injunctions identified in inspection reports from a financial supervisory authority that enables us to identify and isolate a potential supply-side effect independently of the demand-side effect. As it is argued further below, these injunctions represent an exogenous shock to lending and work as an instrument that is unrelated to the economic well-being of the firm prior to the injunction. First, we use an injunction dummy variable in a simple OLS setting. Second, we estimate two-stage models to verify that we do not confound the effect of the injunction with the state of the banks. In this way, we add to a number of recent papers that have worked with similar issues in different ways.

To assess the relative importance of banks' loan supply and firms' loan demand, Kremp and Sevestre (2013) use French firm-level data from 2000-2010 to estimate a disequilibrium model that accounts for the selection bias arising from including only firms with loans. They conclude that credit rationing during the financial crisis was low and that firms' loan demand decreased more than banks' loan supply. Bo, Driver, and Lin (2014) also find that demand effects dominate financial constraints in China. Gaiotti (2013) uses qualitative information on whether Italian firms' credit applications are turned down to identify a potential effect of banks' loan supply on firms' investments. They find evidence of credit rationing that reduces investments. They also conclude that the impact is time-varying and was most significant in the 2008-2009 period. Akbar, Rehman, and Ormrod (2013) use U.K. data on private firms from 2004-2009 to identify the effect of credit supply shocks. They use the recent financial crisis as an instrumental variable and find that credit supply shocks have adversely affected the total debt ratio of private firms. Neither of these analyses establishes a direct link between banks and their customers, which, from our perspective, is important insofar as banks should be expected to continuously change their capital ratios to reflect the risk in their underlying portfolio. When this direct link is missing, results are entirely driven by firm characteristics, and all banks are assumed to or at least treated as if they act according to the same opportunity set and rationale, which easily becomes a problematic assumption.

Francis and Osborne (2012), in contrast, examine the relation between a financial supervisory authority's minimum capital requirements and bank behavior. They find that U.K. banks' target capital ratios increase when capital requirements increase and that lending increases when bank capitalization (the gap between actual and estimated target capital ratio) improves. In other words, they find that changes in capitalization at the individual bank level predict subsequent lending growth. Aiyay, Calomiris, and Wieladek (2016, 2014) also make

use of the variation individual capital requirements for U.K. banks to show that banks reduce lending in response to higher minimum capital requirements. On the one hand, Francis and Osborne (2012) and Aiyay, Calomiris, and Wieladek (2016, 2014) identify a bank lending channel coming from capital requirements but do not trace the real effects of it. The source of the credit constraint is clear, but the real effect is not. On the other hand, Kremp and Sevestre (2013) and Gaiotti (2013) identify real effects of credit constraints, but then, the source of the credit constraint is not clear. Our contribution in this paper is to bridge this gap.

We construct a new data set that combines information on firms and their banks, and to overcome the fact that banks endogenously change their own capital requirements, we use variation from injunctions made by the financial supervisory authority to individual banks in order to examine the causal effects of shocks to the loan supply. We empirically examine the effects by using Danish data from 2010 to 2015 on almost 15,000 firms and 89 banks, and our results clearly indicate that higher capital requirements for banks have implications for firms' capital structure and activity and real effects on employment and defaults. Denmark is an ideal laboratory for such an analysis because in 2009, as one of the requirements that came with the second so-called bank package, Parliament decided on public access to the Danish Financial Supervisory Authority's (FSA) inspection reports. Moreover, a substantial part of the limited liability firms disclose their banking relationships together with balance sheet notes on bank debt, which allows us to match firms and banks and to look specifically at bank debt. Denmark is also an archetypical European country in the sense that firms rely heavily on debt financing from a large banking sector, where the largest bank alone has a balance sheet that is 150 percent of the national GDP.

The rest of the paper is organized as follows. Our data are presented in the next section, along with both the sample selection and some characteristics of the firms and banks in the data. In Section 3, we present our identification strategy. In Section 4, we present and discuss the main econometric results along with some heterogeneous effects. Finally, we assess the consequences of the potential endogenous selection of the sample and test the robustness of our results in Section 5. Section 6 concludes.

2. Data

We use firm-level financial data together with information on bank relations merged with bank-level data from the FSA in Denmark. The FSA oversees compliance with banking regulations and routinely conducts inspections of individual banks to assess whether the

minimum capital requirement sufficiently complies with regulation and covers the risk attached to banks' assets. The Danish setting provides the opportunity to collect and code the content of the inspection reports that are produced after each visit. Our data period starts in year 2010, which is the first year with public access to these reports, and ends in 2015. We use annual firm balance sheet and income statement data from the Danish Business Authority. There is open data access that allows us to harvest information for all firms that submit electronic annual reports to the Business Authority. Since our interest in this paper is the firm-bank relation, we focus solely on firms that disclose their banking information; this information is not legally required and exists for about one-third of firms. To minimize concerns with selection bias, we examine the main firm characteristics between firms for which we have and do not have banking information. At this point, however, this is the only selection criteria that we apply; however, we run diagnostics to identify and remove observations that do not make sense, such as when the accounting identity that total assets must equal the sum of total equity and total liabilities does not hold. We move on to exclude financial firms from the firm data set and firms with less than 50,000 DKK in total assets; this threshold value corresponds to the minimum statutory capital requirement for new limited liability firms. All accounting variables are winsorized at the 1 percent level.

The Danish FSA provides us with financial data on commercial banks and savings banks that contain information about key performance indicators used as part of their monitoring. We subsequently add a dummy variable indicating whether a bank is a large bank or a small bank. To this end, we categorize the so-called group 1 banks as large banks. The FSA operates with four different groups defined by the size of working capital, where group 1 banks are the largest, with a working capital of 65 billion DKK or more. In 2013, there were five such banks, and at the sector level, they covered more than 80 percent of deposits. These banks are also the systemically important financial institutions, and they are the country-wide banks. In addition to this data set, we use a second and unique data set based on the hand collection of all public reports from the inspections conducted by the FSA since 2010. Inspections are crucial for the most important supervisory activity: supervision of solvency. The Basel II framework allows banks to use internal models to estimate the minimum capital requirement needed to cover their risky assets; however, the second pillar of the framework ensures that supervisory authorities are obliged to review and evaluate the adequacy of these internal models, and it gives them authority to issue injunctions if the minimum capital is deemed inadequate. We look for such injunctions in the inspection reports. The FSA conducted 284 inspections from 2010 to 2015, according to our

investigation of the public material, resulting in 54 injunctions to raise minimum capital.⁴ It is important for a bank to follow such an injunction, since its banking license depends on having a capital level above the minimum requirement. All registered injunctions are similar in the sense that they are formal requirements that the bank to raise the minimum capital. In the public documents, there is no advice as to what measures ought to be taken on the asset side of the bank's balance sheet.

The firm data set and the two bank data sets are subsequently merged. Some of the firms report relationships with foreign banks that are not supervised by the FSA, and likewise, the FSA monitors banks that are not reported by any of the firms. All these firms are excluded when we merge the data. Since we need to calculate year-to-year growth rates, we retain only firms for which we have data for at least two consecutive years. We keep the data set unbalanced and end up with 33,641 firm-year observations that satisfy these additional criteria. There are 14,783 unique firms among these firms, and there are 89 unique banks. We now move on to describe our data by way of some descriptive statistics.

2.1. Variables and descriptive statistics

Panel A in Table 1 presents some standard variables for the firms, which are also the characteristics that we later use in the multivariate analyses to control for observable time-variant differences between firms: **SIZE**, the natural logarithm of total assets; **AGE**, the natural logarithm of firm age; **LEVERAGE**, total debt over total assets; **RETURN**, the ratio of earnings before interest and taxes over total assets; **LEGAL STATUS**, a dummy variable equal to one if the firm is a joint stock company; and **GROUP 1 BANK**, a dummy variable equal to one if the firm's bank is a group 1 bank. We also construct a dummy variable for the geographic region in which the firm's headquarters is located (**LOCATION**) and a dummy variable for the 2-digit NACE industry affiliation (**INDUSTRY**). Finally, we present statistics for the different firm-level outcomes that we use as our dependent variables: **DELTA DEBT**, the change in the natural logarithm of total debt from year t-1 to year t; **DELTA ASSETS** and **DELTA EMPLOYEE COSTS**, calculated similarly only with total assets and total employee

⁴ After an inspection, the FSA may issue injunctions on other matters than capital requirements such as for example management or accounting practices. None of these are however directly related to the credit supply in the same way as solvency.

costs instead of total debt; and DEFAULT, the probability that the official firm status changes from active to inactive from year $t-1$ to year t .

To obtain some univariate insights on the differences between firms, we sort them into two groups based on whether their banks received an injunction from the FSA. To obtain a cross-sectional snapshot that is unaffected by the unbalanced structure of the panel, we use the numbers for the year 2013 in the table; this year does not stand out as a particular year, in terms of the firm data or the bank data. Thus, the injunction group consists of firms with a bank that received an injunction in 2013, and the no injunction group consists of all other firms for which we have bank information. In Panel B in Table 1, we add statistics for firms with no information on the bank relation so as to check for selection biases. There are a couple of notable differences: First, the firms in our analysis (Panel A) are significantly larger than the firms for which we do not have information about the bank relationship (Panel B). The firms in our analysis are also more likely to be joint stock companies. Second, they operate with lower leverage, and they have a higher but also more volatile return on assets. The firms in our sample also grow their debt and assets to a greater extent. One could therefore argue that our data seem to be biased toward stronger firms and that our results therefore are conservative estimates of the actual effects.

Table 1

Descriptive statistics for firms. Mean values with standard deviations in parentheses. Year 2013.

	Panel A: Bank information			Panel B: No bank information
	No injunction	Injunction	Difference	N/A
Size	14.77 (1.95)	15.04 (1.79)	-0.27*** (0.04)	9.15 (1.73)
Age	2.46 (0.68)	2.56 (0.68)	-0.10*** (0.01)	2.48 (0.75)
Leverage	0.51 (0.28)	0.47 (0.28)	0.04*** (0.01)	0.57 (0.28)
Return	0.08 (0.18)	0.08 (0.18)	0.00 (0.00)	0.04 (0.11)
Legal status	0.29 (0.45)	0.31 (0.46)	-0.02** (0.01)	0.27 (0.44)
Group 1 bank	0.55 (0.50)	0.80 (0.40)	-0.25*** (0.01)	N/A (N/A)
Debt growth	-0.01 (0.67)	0.00 (0.66)	-0.01 (0.01)	-0.05 (0.49)
Equity growth	0.05 (0.54)	0.06 (0.49)	-0.02 (0.01)	0.08 (0.50)
Assets growth	0.01 (0.33)	0.02 (0.30)	-0.01** (0.01)	0.01 (0.23)
Employee costs growth	-0.00 (0.44)	-0.01 (0.42)	-0.00 (0.01)	0.01 (0.46)
Default	0.01 (0.10)	0.01 (0.07)	0.00*** (0.00)	0.03 (0.18)
Observations	4,385	5,791		17,785

We have used a T-test to test for differences in means. *** p<0.01, ** p<0.05, * p<0.1.

Returning to the firms in our analyses in Panel A in Table 1, we notice that the two groups are fairly similar in terms of size and age, although the differences are significantly different from zero. Firms with banks that did not receive an injunction operate with higher leverage; their debt to assets ratio is 51 percent, which is relatively high by international standards, and the statistically significant difference to other firms is about 4 percentage points. At the same time, we also notice that the growth rate of debt is the same regardless of whether the bank received an injunction in 2013. The other growth rates are also of the same magnitude, with a 2-percentage point difference in equity growth being the largest and only

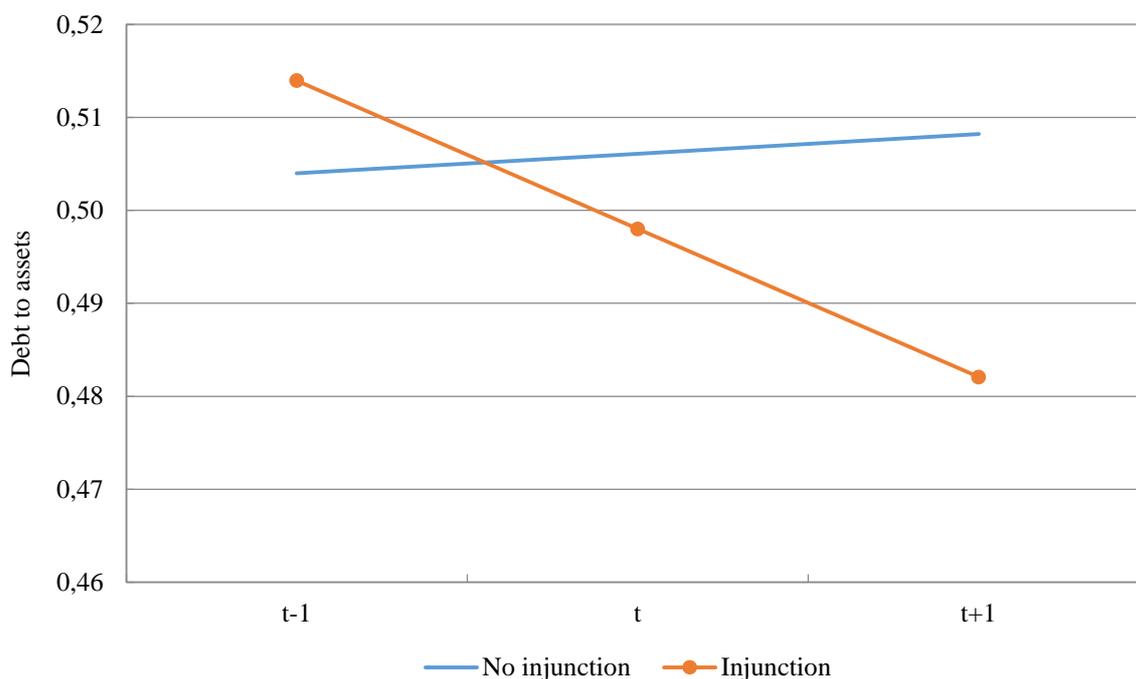
significant difference. Second, there is a notable difference in the variable indicating whether a firm's bank is a group 1 bank, where firms with banks that did receive an injunction are much more likely to be costumers of large banks (group 1 banks). This result does not necessarily mean that large banks receive an injunction more often; it could be an artifact of our sampling, or it could reflect the fact that large banks have a relatively high market share. In Table 2 below, we examine some characteristics of the banks in our data set in more detail.

One take on the firm characteristics included here is that they collectively capture some of the central elements of the risk of default, which arguably is one of the most important factors that determines a firm's access to credit. Firm size, then, has a positive relation with a firm's creditworthiness since large firms are less likely to default. Such a positive relation should also be expected for older firms and for firms with higher profitability (for a given level of risk). Debt level may be the single most important variable in this respect, as more leverage typically increases default risk.

In Figure 1, we graph the change in the debt to asset ratios for firms with banks that did and did not receive an injunction in an event study-inspired manner, where time t indicates the year of the injunction. For each year in our sample, we calculate the average debt ratio for all firms for each year from $t-1$ to $t+1$ and group them by the banks' injunction status. The figure then shows the pooled average across years by group. The first observation is that debt ratios differ by less than one percentage point in time $t-1$, which is reassuring, since it indicates that firms are indeed *ex-ante* similar. Second, in the year after the injunction, leverage is almost 3 percentage points lower for firms with banks that received an injunction, and this difference is statistically different from zero. Thus, what we see here is a first indication of exposed firms starting to rely less on debt concurrently with injunctions.

Figure 1

Leverage by injunction status. All firms. Years 2010-2015.



We sort banks in the same way as we did firms. Table 2 presents some descriptive statistics for the banks in our data set. As mentioned earlier, there are 54 injunctions in total in our data. These are spread throughout the 2010-2015 sample period, with most observations in the middle of the period. For any given year, relatively few observations go into the descriptive statistics; note that this leaves us with low power in the statistical tests for differences in means. Table 2 presents the statistics for year 2012 based on banks' injunction status in 2013. In addition to GROUP 1 BANK, which we have already explained, we have the following bank specific variables: SOLVENCY RATIO, which is core capital plus supplementary capital as a percentage of risk-weighted assets; EXCESS COVERAGE, which is excess coverage after fulfillment of the statutory minimum liquidity requirements as a percentage of the liquidity requirement; LARGE EXPOSURES, which is the sum of large exposures as a percentage of core capital plus supplementary capital, where large exposures are defined as the sum of assets and off-balance-sheet items that after a reduction for secured exposures, exceeds 10 percent of the sum of core capital and supplementary capital; GROWTH IN LOANS, which is growth in loans for the period over total loans at the beginning of the period; INTEREST RATE RISK, which is the percentage of core capital that would be lost owing to a one percent increase in the interest rate; and IMPAIRMENT RATIO, which is impairment losses as a percentage of loans and guarantees.

There are notable differences between the two groups of banks on almost all accounts, although these differences are statistically different from zero in only two cases. The low power in these tests calls for caution, but it should be noted that prior to an injunction, a forthcoming injunction bank is no different than other banks, which suggests that the injunction is indeed an exogenous surprise (more on the identification strategy in Section 3). Now, turning to the observable but rarely statistically significant differences, we observe that large banks (group 1 banks) are underrepresented in the injunction group. This could be related to their loan portfolios; however, the FSA also clearly states that larger banks are visited more frequently, and an inspection visit is obviously a necessary condition for an injunction. Unsurprisingly, banks that receive injunctions have lower solvency ratios on average and also less excess coverage. Banks that receive injunctions are significantly more exposed to large accounts and have grown the volume of their loans at lower rates. Finally, these banks have also had to take significantly more impairment losses onto their books.

As a closing note, it should be restated that the Danish banking sector comprises a few large banks (5) that accounted for almost 80 percent of total lending in 2014 and for approximately the same share of total assets. By international standards, this is a relatively concentrated market. Another important part of the Danish setting is the high level of merger activity in the financial sector in the last decade; the number of Danish commercial and savings banks more than halved between 2000 and 2014. This significant wave of bank mergers likely reshaped the sector and the relations with corporate customers. One implication hereof is that there is no clear segmentation in the market where, for instance, large banks primarily cater to large firms and small banks cater to small firms. In terms of the size of their corporate customers, the distributions are very similar for the two groups of banks.

Table 2

Descriptive statistics for banks. Mean values with standard deviations in parentheses. Year 2012 based on injunction status in 2013.

	No injunction	Injunction	Difference
Group 1 bank	0.06 (0.24)	0.00 (0.00)	0.06 (0.08)
Solvency ratio	23.80 (18.20)	14.71 (4.36)	9.09 (5.83)
Excess coverage	327.92 (248.23)	210.12 (110.86)	117.79 (80.51)
Large exposures	39,62 (36.45)	76.65 (102.38)	-37.04** (18.27)
Growth in loans	-0.93 (11.24)	-1.50 (32.38)	0.57 (5.72)
Interest rate risk	2.26 (4.11)	0.41 (4.52)	1.85 (1.45)
Impairment ratio	1.89 (2.97)	4.31 (2.74)	-2.41** (1.02)
Observations (bank-years)	49	10	

For the purpose of a meaningful comparison, in this particular table, we have excluded banks with injunctions in years prior to 2013. Without this restriction, we could be treating a bank with a 2012 injunction but no 2013 injunction similar to a bank with no injunction at all. We have used a T-test to test for differences in means. *** p<0.01, ** p<0.05, * p<0.1.

3. Identification strategy

If a firm's bank receives an injunction and if the bank does not have sufficient excess capital to cover the required increase in capital, it will eventually resort to credit rationing. If the firm is unable to fully substitute a decrease in credit supplied from its primary lender, either from other forms of financing or bank financing, which have (perhaps) not received an injunction, the credit cut will affect the firm's capital structure and its ability to engage in new investment projects, among other things.

The main empirical challenge lies in simultaneously estimating the effect of the bank lending channel and the firm borrowing channel. The bank lending channel relates to banks' supply of debt, and the firm borrowing channel relates to firms' demand for debt. Without a proper identification strategy, we cannot know whether bank lending actually causes changes

in firm financing and operations. We pool our data and estimate models along the lines of the following equation by using the OLS estimator.

$$\Delta \ln(\text{Debt})_{it} = \alpha_0 + \alpha_1 \text{Injunction}_{ibt-1} + \beta \mathbf{X}_{it-1} + \gamma \mathbf{Y}_{it} + \delta \mathbf{Z}_{bt-1} + \lambda_t + \varepsilon_{ibt} \quad (1)$$

The dependent variable in equation (1), $\Delta \ln(\text{Debt})_{it}$, is the annual growth rate in debt (in percentages) for firm i in year t . We also estimate models with the annual growth rate in equity, assets, and employee costs as the dependent variable, as well as a model with a binary variable for whether a firm defaults or not as the dependent variable, in which case we use a Probit model for the estimation. Our primary variable of interest, $\text{Injunction}_{ibt-1}$, is a dummy variable equal to one if bank b of firm i received an injunction in year $t - 1$ and zero otherwise. \mathbf{X}_{it-1} is a vector of observable firm characteristics with values lagged one year (SIZE, LEVERAGE, RETURN, AGE, LEGAL STATUS, and GROUP 1 BANK); \mathbf{Y}_t is a vector of other observable and non-lagged firm characteristics (LOCATION and INDUSTRY); \mathbf{Z}_{bt-1} is a vector of observable bank characteristics (EXCESS COVERAGE, LARGE EXPOSURES, GROWTH IN LOANS, INTEREST RATE RISK, and IMPAIRMENT RATIO), with their values also lagged one period; λ_t is a year dummy; and ε_{ibt} is the error term. Errors are clustered at the bank level to accommodate for the fact that injunctions occur at the bank level and that outcomes therefore may be correlated across banks.

We obtain unbiased estimates when the observed measures are orthogonal on unobserved measures. The identifying assumption is therefore that conditional on all the observable characteristics, there is no correlation between borrowing from a bank that receives an injunction and unobserved characteristics of the firm that affect the dependent variable. This is not an unrealistic assumption. The FSA states that a bank is examined every fourth year *on average* with more frequent visits to larger and riskier banks. In a given year, a bank therefore does not know whether there will be an examination, which is to say that such an examination is at least quasi-random. We cannot rule out that examinations are expected, and while they may not be randomly assigned to banks, they seem exogenous to the individual firm. The set of observable firm characteristics and bank characteristics that we have described above further limits the concern of unobserved, confounding factors.

Before we make any inference regarding the causal effect of the injunctions, we must address the concern of injunctions being truly exogenous to the borrowing firms. The question is whether firms that borrow from banks that receive injunctions are different along unobserved dimensions than firms that borrow from banks that do not receive injunctions. If an injunction is anticipated, a bank may seek to raise its solvency by adjusting its lending prior to the examination, which would lead to a biased estimate of the effect of the bank lending channel. Unanticipated shocks remove such concerns. In section 6, we attempt to accommodate this concern and test the robustness of our results with another estimation approach. More specifically, we seek to isolate the effect of an injunction by explicitly accounting for a bank's probability of receiving an injunction in a two-stage regression model.

At this point, we rely on the quasi-random nature of the injunction dummy and the control variables. Each of these variables may influence borrower outcomes regardless of whether the bank receives an injunction, causing them to reduce the residual variance of the dependent variable and limit the concern that borrowers of different banks also differ along unobserved dimensions. A discussion of the results follows in the next section.

4. Results

In Model 1 in Table 4, where the change in debt is the dependent variable, there is a negative effect of an injunction. The point estimate is -0.03. Economically, this result indicates that firms with banks that receive injunctions increase their debt by 3 percent less than other firms in the year following the injunction. Statistically, this result is very strong and thus confirms the existence and importance of a bank lending channel fueled by shocks to the minimum capital requirement. We have included a number of firm characteristics in the specification to control for the possibility that the effect of an injunction differs between firms along these observable dimensions. In other words, we take into account that larger firms have lower growth rates for their debt, which—unsurprisingly—is also the case for more leveraged firms and older firms. On average, joint stock companies have higher growth rates for their debt than other firms, which is also not surprising given the higher statutory capital in these firms. To control for bank risk—and to exhaust the risk of unobserved variation—we have also included a number of bank characteristics, all of which are nonsignificant in this first specification.

Table 4

Estimation results.

	(1)	(2)	(3)	(4)	(5)
	Debt	Equity	Assets	Employee costs	Default
Injunction	-0.03*** (0.01)	-0.00 (0.01)	-0.02*** (0.01)	-0.02** (0.01)	0.00*** (0.00)
Size	-0.06*** (0.01)	-0.07*** (0.01)	-0.07*** (0.01)	-0.02*** (0.00)	-0.00*** (0.00)
Leverage	-0.34*** (0.02)	0.46*** (0.02)	0.01 (0.01)	-0.00 (0.03)	-0.00 (0.00)
Return	0.01 (0.03)	0.08*** (0.02)	0.11*** (0.02)	-0.26*** (0.04)	-0.01** (0.00)
Age	-0.04*** (0.01)	0.01 (0.01)	-0.01 (0.01)	0.06*** (0.01)	0.00 (0.01)
Legal status	0.08*** (0.01)	0.08*** (0.01)	0.09*** (0.01)	0.03*** (0.01)	0.00 (0.00)
Group 1 bank	0.02 (0.02)	0.03** (0.01)	0.03*** (0.01)	0.02** (0.01)	-0.00 (0.00)
Excess coverage	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)
Large exposures	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Growth in loans	-0.00 (0.00)	0.00* (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Interest rate risk	-0.00 (0.00)	-0.01*** (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Impairment ratio	0.00 (0.01)	-0.00 (0.01)	-0.00 (0.00)	-0.00 (0.01)	0.00 (0.00)
Constant	1.24*** (0.12)	0.86*** (0.11)	1.09*** (0.10)	0.07 (0.07)	N/A (N/A)
Observations	15,947	16,038	16,074	10,477	12,693
R-squared	0.03	0.05	0.04	0.02	0.08

Estimation by pooled OLS. Robust standard errors in parentheses. Standard errors are clustered at the bank level. Industry and year dummies are included in the estimations but not reported. *** p<0.01, ** p<0.05, * p<0.1.

Total debt comprises different elements, some of which are long term or short term, and some of which are bank debt or other debt. We have focused on total debt to capture reallocations of debt, primarily between banks. We are primarily interested in the real effects, i.e., the origin of credit is less of a concern than the availability of credit to this particular firm somewhere in the financial system. However, one might argue that bank debt specifically is the important part of total debt to focus on and, more specifically, that the short-term bank debt should be the focus, as this is where banks can make immediate adjustments. Reducing short-term loans directly curbs the capital shortfall. In other words, if there is a bank lending channel, we should also be able to observe an effect on short-term bank debt. In an unreported regression, using the smaller subset of data where this particular information on short-term bank debt is available, the coefficient estimate for the injunction variable is as high as -0.15, indicating that firms with banks that receive injunctions grow their short-term bank debt 15 percent less than other firms in the year following the injunction.

While banks appear to reduce their credit supply after an injunction, it does not necessarily have implications for business. First, firms can substitute bank debt with other debt. Second, they can substitute debt with equity, in which case we should observe an increase in equity when debt decreases. This would be in accordance with an assumption of efficient capital markets. To determine whether this is in fact the case, we use the change in equity as the dependent variable in Model 2. In this specification, the estimated coefficient of the injunction variable is not significantly different from zero, suggesting that firms do not make up for the credit cut with more equity. On average, corporate financing is thus limited by injunctions.

When there is a bank lending channel, what happens to banks will eventually have repercussions for their customers. As a corollary to the fact that debt is reduced and equity does not increase, business activity should go down, which is exactly what we observe in Model 3, where the change in total assets is the dependent variable. Here, the point estimate is -0.02. Economically, this result means that firms with banks that receive injunctions increase their assets by 2 percent less than other firms in the year following the injunction. When asset growth is low and activity stagnates, firms will eventually have to respond by cutting costs one way or the other. In model 4, we test what happens to employee costs, which is a good measure, since it captures both the quantity of labor (number of employees) and the price of labor (wages). As these costs are registered with negative values, a positive coefficient estimate indicates a reduction in costs, and this is what we observe. This variable

indicates the change in the natural logarithm of employee costs, meaning that employee costs grow by 2 percent less in firms with banks that receive injunctions.

By slowing down economic activity and reducing overall employee costs, injunctions appear to have real consequences for the economy. As a final attempt to understand these consequences, we estimate a Probit model (5) on whether a firm officially changes status from active to inactive in the year following the injunction. We think of this change as a default and report the estimated marginal effects, and we find that injunctions also increase the probability of default. More specifically, it increases by 0.3 percent. The average default probability in our sample is 1 percent, and with a point estimate of 0.3 percent, this result implies that for a firm with an average default probability, this probability would increase by $0.003 / 0.01 = 30$ percent if the bank receives an injunction.

4.1. Heterogeneous effects

The previous section clearly shows a significant economic impact of injunctions. This average effect may, however, conceal some heterogeneous effects. In other words, some types of firms may be more severely affected than others. As noted by Chodorow-Reich (2014, p. 5), research has examined a bank lending channel “using the insight that due to greater asymmetric information or smaller buffer savings, smaller, less transparent borrowers should exhibit greater sensitivity to credit supply constraints”. A similar argument is made in Cotugno, Monferrà, and Sampagnaro (2013). Using a long panel (28 years) of Japanese firms, Voutsinas and Werner (2011) also find that smaller firms are particularly exposed to credit constraints, especially during economic downturns.

Table 5

Heterogeneous effects. Abbreviated results. All years. Standard errors in parentheses.

	(1)	(2)	(3)	(4)	(5)
	Debt	Equity	Assets	Employee costs	Default
Panel A: Firm size					
(Injunction*SmallF)	-0.04*** (0.01)	-0.02 (0.01)	-0.03*** (0.01)	-0.03*** (0.01)	0.00*** (0.00)
(Injunction*LargeF)	0.01 (0.02)	0.05** (0.02)	0.03* (0.01)	0.01 (0.02)	0.00*** (0.00)
Panel B: Firm age					
(Injunction*Young)	-0.03 (0.02)	0.03 (0.03)	-0.00 (0.02)	-0.07*** (0.01)	-0.00 (0.00)
(Injunction*Old)	-0.04*** (0.01)	-0.01 (0.01)	-0.02*** (0.01)	-0.01 (0.01)	0.00*** (0.00)
Panel C: Bank size					
(Injunction*SmallB)	-0.02 (0.03)	-0.01 (0.03)	-0.01 (0.02)	0.01 (0.02)	0.00 (0.00)
(Injunction*LargeB)	-0.03*** (0.01)	0.00 (0.01)	-0.02*** (0.00)	-0.03*** (0.01)	0.00*** (0.00)
Panel D: Firm size and bank size					
(Injunction*SmallB *SmallF)	-0.02 (0.03)	-0.00 (0.03)	-0.01 (0.02)	-0.00 (0.03)	-0.00 (0.00)
(Injunction*SmallB*LargeF)	-0.04 (0.04)	-0.03 (0.04)	-0.03 (0.03)	0.05 (0.06)	0.00 (0.01)
(Injunction*LargeB *SmallF)	-0.05*** (0.01)	-0.02*** (0.01)	-0.04*** (0.00)	-0.04*** (0.01)	0.00*** (0.00)
(Injunction*LargeB*LargeF)	0.03*** (0.01)	0.07*** (0.01)	0.04*** (0.00)	-0.01 (0.01)	0.00*** (0.00)
Controls					
Firm controls	Yes	Yes	Yes	Yes	Yes
Banks controls	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes

We use the following abbreviations for the different categories of firms: Small (Large) Bank: SmallB, (LargeB); Small (Large) Firm: SmallF (LargeF). Estimation by pooled OLS. Robust standard errors in parentheses. Standard errors are clustered at the bank level. Industry and year dummies are included in the estimations but not reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To test whether this is the case, Table 5 repeats the analyses from above; however, we have now separated the effect on debt, assets, equity, etc., by an indicator for the size and the age of the firm. As for the size, it should be recognized that larger firms have better opportunities to shift from one bank to another and thus mitigate the potential effect of an injunction. Moreover, banks may be more reluctant to cut the credit supply to large important customers and, by contrast, be more likely to tighten up relative to smaller and perhaps more marginal customers; in Kremp and Sevestre (2013), larger firms are always less credit rationed in the crisis. To operationalize this, we use the 75 percentile of total assets to separate large firms from smaller firms, and we use 5 years to separate older firms from younger firms. Across the different models in the two tables, the results lend support to the same conclusion that we arrived at above—i.e., the bank lending channel is affected by shocks to the minimum capital requirement, and there are real economic effects of this, except that the effects pertain entirely to smaller firms. Interestingly, the effects are strongest for older firms according to our categorization, and firms that are both small and young, which could be suspected to be particularly vulnerable to credit constraints, do not stand out as such. If we combine firm size and firm age, it is clear that size dominates age: small firms are always more affected, regardless of their age. We then repeat the analyses with firms separated by the size of their bank by using the group 1 variable to discriminate between them. The results are reported in panel C in Table 5. We find no significant effects for firms with small banks; it seems that the average effects reported in Table 3 are entirely due to large banks, which, apparently, pass on the consequences of the injunction to their corporate customers, whereas smaller banks somehow either manage or decide not to do this. One potential explanation for this result is that smaller banks adhere more to relationship banking than larger banks (Berger et al., 2004). In the financial history of Denmark, the “church tower principle” plays a dominant role. This principle states that a bank should be more responsible for the customers that it can see from the church tower—i.e., a form of relationship banking. Unreported numbers show that small banks have significantly higher excess coverage than large banks. Hence, these banks may also be more capable of absorbing the effect of the increased capital requirement instead of immediately transmitting it to their customers. This is consistent with the idea that banks with excess capital face fewer constraints in their ability to lend (Francis and Osborne, 2012). Moving on, it makes sense to combine the firm size dummy with the bank size dummy to examine these more granular categories and determine whether large banks treat their small and large customers equal. Results are reported in Panel D. If we combine the insights from the literature on firm size cited above with the results on

bank size, the hypothesis is that large banks are more likely to reduce the credit supply to smaller firms, and this is exactly what we find. Furthermore, small firms in large banks that receive injunctions grow their asset base at lower rates, and for the first time in our analyses, we see a segment of firms that also grow their equity at lower rates. Employee costs are reduced by 4 percent, and the probability of default increases. Interestingly, almost all of these results are opposite for large firms. This result is in line with Chodorow-Reich (2014), who finds that the bank lending channel identified in his paper exists only for small firms.

Before closing this section, we want to take a brief, descriptive look at one particular characteristic of the firms that are most likely affected by credit constraints. Smaller and younger firms are often far less real asset intensive than larger and older firms; instead, their balance sheets are relatively heavy on immaterial assets. In our data (unreported numbers), the average ratio between immaterial and real assets is highest for small firms in large banks, which is statistically larger than that of any of the other groups, and it is always higher for smaller firms than for larger firms, suggesting that the firms most affected by injunctions are small and light on investments in real assets. We view this finding as being consistent with the theoretical results in Almeida et al. (2011), where financing constraints distort the investment process and lead firms to have a preference for safer investments with more tangible and verifiable cash flows. In earlier empirical studies, similar financial market inefficiencies have been found to impede the allocation of capital (Wurgler, 2000) and economic growth in general (Beck et al., 2000).

5. Robustness

As mentioned earlier, we want to further address the concern of injunctions being truly exogenous to borrowing firms. Since the FSA states that banks are examined every fourth year on average, with more frequent visits to larger and riskier banks, there may be a positive correlation between the time since the last inspection and the likelihood of a new inspection—and with that the likelihood of an injunction, which may cause banks to make preventive adjustments and cut lending. Hence, we should perhaps group our firms into three groups instead of the traditional two groups used in the standard difference-in-difference approach (treated and untreated). If we have no recording of an injunction, it may be where the bank was not examined (Group 1) or where the bank was indeed examined, but the FSA found no ground for an injunction (Group 2). This is an important distinction if examinations

are not random. Finally, there is a group of firms with banks that were examined and did receive injunctions (Group 3).

The fact that we have implicitly kept the probability of receiving an injunction constant (equal for all) in our baseline regressions may confound the effect of the injunction with the state of the banks. When we want to evaluate the effect of injunctions and if the examinations are not random, we should not compare the change for those firms with a bank that was examined and that received an injunction to those with a bank that did not receive an injunction without explicitly recognizing the probability of the bank receiving an injunction. Failing to do so means that we treat an unhealthy bank that was not examined similar to a healthy bank that was examined and “cleared”.

As a robustness test, we therefore apply the difference-in-difference methodology developed in Faulkender and Petersen (2012), where they study the relationship between corporate investments and repatriations under the American Jobs Creation Act from 2004. The idea is to estimate two coefficients instead of one to distinguish between the three different groups instead of the usual two. In other words, we need a coefficient to distinguish Group 1 from Groups 2 and 3 and another coefficient to distinguish Group 2 from Group 3. As shown in Faulkender and Petersen (2012), this can be done with the following specification:

$$\begin{aligned}
\Delta \ln(Debt)_{it} &= \alpha_0 + \alpha_1 \Pr[Injunction]_{ibt-1} + \alpha_2 (Injunction - \Pr[Injunction])_{ibt-1} \\
&\quad + \beta \mathbf{X}_{it-1} + \gamma \mathbf{Y}_{it} + \delta \mathbf{Z}_{bt-1} + \lambda_t + \varepsilon_{ibt} \\
&= \alpha_0 + \alpha_2 Injunction_{ibt-1} + (\alpha_1 - \alpha_2) \Pr[Injunction]_{ibt-1} + \beta \mathbf{X}_{it-1} + \gamma \mathbf{Y}_{it} \\
&\quad + \delta \mathbf{Z}_{bt-1} + \lambda_t + \varepsilon_{ibt}
\end{aligned} \tag{2}$$

where

$$\Pr[Injunction]_{bt} = \alpha_0 + \delta \mathbf{Z}_{bt-1} + \varepsilon_{bt} \tag{3}$$

This is a two-stage model, where the predicted probability of receiving an injunction is calculated in the first stage (equation (3)) and then carried forward to the second stage (equation (2)). Otherwise, all variables are similar to those in equation (1). In the first stage, we estimate a Probit model with the injunction dummy as the dependent variable and the bank variables in vector \mathbf{Z} as the explanatory variables. As the rewriting of the model

highlights, it is α_2 in which we are interested. This is the coefficient of a new variable that measures the difference between the injunction dummy and the predicted probability of receiving an injunction, which we label RESIDUAL. It measures the incremental change in debt financing for firms that did receive an injunction relative to those that did not, where we hold firm characteristics and the probability of injunction constant. In other words, it measures the effect of the unexpected probability that the bank gets an injunction.

Table 6 reports the results when we use this estimation procedure. As mentioned above, it is the coefficient estimate on the residual variable that tells us about the effect of injunctions. The results are similar to those reported in Table 3, where the raw injunction variable is used. On average, debt is down by 3 percent relative to firms with banks that did not receive injunctions, when we take account of the estimated probability of receiving an injunction; equity is the same, i.e., unaffected by injunctions, and assets are reduced by 2 percent in relative terms, the same with employee costs; finally, the probability of default increases. Compared to those in Table 3, the sizes of the coefficients are close to each other in magnitude and statistically significant at the same levels, indicating that injunctions could indeed be considered randomly assigned to firms. This result is further supported by the fact that the coefficient on the probability that a firm's bank will get an injunction is never statistically different from zero. In conclusion, the baseline regressions, the heterogeneous effects regressions, and this alternative estimation of the baseline setup all produce the same, robust results.

Table 6

Robustness.

	(1) Debt	(2) Equity	(3) Assets	(4) Employee costs	(5) Default
Pr(Injunction)	-0.02 (0.06)	-0.02 (0.06)	-0.00 (0.04)	0.12 (0.08)	-0.00 (0.01)
Residual	-0.03*** (0.01)	0.00 (0.01)	-0.02** (0.01)	-0.02** (0.01)	0.00*** (0.00)
Size	-0.06*** (0.01)	-0.07*** (0.01)	-0.07*** (0.01)	-0.02*** (0.00)	-0.00*** (0.00)
Leverage	-0.33*** (0.02)	0.47*** (0.02)	0.02 (0.01)	-0.00 (0.03)	-0.00 (0.00)
Return	0.01 (0.03)	0.08*** (0.03)	0.12*** (0.02)	-0.26*** (0.05)	-0.01** (0.00)
Age	-0.04*** (0.01)	0.01 (0.01)	-0.01 (0.01)	0.06*** (0.01)	0.00 (0.00)
Legal status	0.09*** (0.01)	0.08*** (0.01)	0.10*** (0.01)	0.03** (0.01)	0.00 (0.00)
Group 1 bank	0.02 (0.01)	0.02* (0.01)	0.03*** (0.01)	0.03*** (0.01)	-0.00 (0.00)
Excess coverage	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00* (0.00)	-0.00 (0.00)
Large exposures	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Growth in loans	-0.00 (0.00)	0.00** (0.00)	0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Interest rate risk	-0.00 (0.00)	-0.01*** (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Impairment ratio	0.00 (0.01)	-0.00 (0.01)	-0.00 (0.00)	-0.01 (0.01)	0.00* (0.00)
Constant	1.29*** (0.14)	0.90*** (0.14)	1.16*** (0.12)	-0.02 (0.10)	N/A (N/A)
Observations	15,340	15,421	15,456	10,032	12,087
R-squared	0.03	0.05	0.04	0.02	0.08

Estimation by pooled OLS. Robust standard errors in parentheses. Standard errors are clustered at the bank level. Industry and year dummies are included in the estimations but not reported. *** p<0.01, ** p<0.05, * p<0.1.

6. Conclusion

Credit was reduced for many firms during the financial crisis, and employment was reduced everywhere and in most sectors. There is, however, less agreement about the linkages among the financial system in general, credit markets in particular, and job destruction as a result. One strand of the scholarly literature has sought to determine whether the loan demand of firms decreased more than the loan supply of banks, but no stable conclusion has yet emerged. This literature is either precise about the real effects but not the source of the credit constraint, or it is precise about the credit constraint but not the real effects. To our knowledge, this paper is the first to address this gap. We use exogenous variation in banks' loan supply, coming from changes in the Danish FSA's individual minimum capital requirements to local banks, to examine whether and how such shocks are transmitted to business customers. In other words, we use the instrumental properties of such injunctions to look for a capital requirement-driven bank lending channel.

We find that debt in firms with banks that receive an injunction decreases in the year following the injunction relative to firms with banks that do not receive an injunction. Firm-level regressions on changes in the amount of debt, with controls for a number of firm and bank variables, show that debt is reduced by 3 percent on average. This result is statistically significant and economically important. Using a subset of data, where we have information about short-term bank debt, we find that the corresponding number is as high as 15 percent. Furthermore, we find for the whole sample that equity does not change owing to an injunction, which is to say that, on average, firms have not compensated for the drop in bank finance by increasing equity. A corollary of less debt and the same equity is that business activity should decrease, and this is exactly what we find; the book value of total assets decreases by 2 percent in the year after the injunction relative to firms with banks that do not receive an injunction. At the same time, we find that employee costs decrease by 2 percent. This is most likely the result of laying off workers. As recently suggested by Fernandes and Ferreira (2017), this could also reflect a reorganization of the workforce from relatively expensive permanent employees to relatively cheap fixed contract employees. Such elements of the labor market outcomes could be an interesting avenue for future research.

Our results are based on observations about almost 15,000 firms that we observe for at least two consecutive years. For each firm-year, we have identified the bank relation. We have constructed a new data set with information on firm financials combined with hand-

collected information on the related banks supplemented with data from the FSA on the health of each of the banks. In addition to using the quasi-experimental setting provided by the Danish post-crisis case, our results have direct bearing on other (primarily) European economies. In 2014, the International Monetary Fund included Denmark in its network of countries with systemically important financial sectors. Danish banks are part of an international network of banks with substantial claims on one another.

Finally, our paper documents that the importance of a capital requirement-driven bank lending channel varies by firm type. Specifically, we examine whether the firm-level effects are different for firms of different size and age and for firms with a small or large bank. We find that the negative effects pertain to smaller firms. Somewhat contrary to our prior findings, firm age is completely dominated by firm size, and young firms are negatively affected only when they are also small in terms of their assets. We also find that the negative effects pertain to firms with large banks. Our final result is that the negative effects are greatest for small firms with large banks, whereas large firms do well with large banks. In summary, we have shown that there is a statistically significant and economically important transmission between the access to credit and the real economy, especially for small firms with relations to large banks.

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