Management Science

Publication details, including instructions for authors and subscription information:
http://pubsonline.informs.org

On the Nonexclusivity of Loan Contracts: An Empirical Investigation

Hans Degryse, Vasso Ioannidou, Erik von Schedvin

MANAGEMENT SCIENCE
Vol. 62, No. 12, December 2016, pp. 3510–3533
ISSN 0025-1909 (print) | ISSN 1526-5501 (online)

https://doi.org/10.1287/mnsc.2015.2277

On the Nonexclusivity of Loan Contracts: An Empirical Investigation

Hans Degryse
Department of Accounting, Finance, and Insurance, KU Leuven, 3000 Leuven, Belgium; and
Centre for Economic Policy Research (CEPR), London EC1V 0DX, United Kingdom, hans.degryse@kuleuven.be

Vasso Ioannidou
Department of Accounting and Finance, Lancaster University, Lancaster LA1 4YX, United Kingdom; and
Centre for Economic Policy Research (CEPR), London, EC1V 0DX, United Kingdom, v.ioannidou@lancaster.ac.uk

Erik von Schedvin
Research Department, Sveriges Riksbank, SE-103 37 Stockholm, Sweden, erik.vonschedvin@riksbank.se

We study how a bank's willingness to lend to a previously exclusive firm changes once the firm obtains a loan from another bank ("outside loan") and breaks an exclusive relationship. Using a difference-in-difference analysis and a setting where outside loans are observable, we document that an outside loan triggers a decrease in the initial bank's willingness to lend to the firm, i.e., outside loans are strategic substitutes. Consistent with concerns about coordination problems and higher indebtedness, we find that this reaction is more pronounced the larger the outside loan and it is muted if the initial bank's existing and future loans retain seniority and are protected with valuable collateral. Our results give a benevolent role to transparency enabling banks to mitigate adverse effects from outside loans. The resulting substitute behavior may also act as a stabilizing force in credit markets limiting positive comovements between lenders, decreasing the possibility of credit freezes and financial crises.

Keywords: coordination failures; credit freezes; credit rationing; credit supply; debt seniority; floating charge; negative externalities; nonexclusivity; transparency
History: Received June 27, 2013; accepted April 26, 2015, by Itay Goldstein, finance. Published online in *Articles in Advance* January 6, 2016.

1. Introduction

In most countries, firms gradually move from exclusive bank-lending relationships to borrowing from multiple banks. Engaging multiple banks may allow firms to reduce hold-up problems (e.g., Sharpe 1990, Rajan 1992, von Thadden 2004, Ioannidou and Ongena 2010) and dampen shocks impacting the liquidity of their banks (e.g., Detragiache et al. 2000). A loan from another bank, however, can also alter other banks willingness to lend to the firm, including the firm's initial bank. For example, the willingness of a bank to lend to the firm could be perceived as a positive signal about the firm and thus increase others banks' willingness to lend to the firm. More broadly, theories emphasizing the role of strategic complementarities in lending predict that one bank's behavior induces a similar behavior from other banks. For example, if one bank cuts its lending to a firm, it induces other banks to also claw back, and vice versa. This dependency can be a source of instability in the financial system because it gives rise to "credit freeze" equilibria and "creditor runs" (e.g., Bebchuk and Goldstein 2011, Vives 2014). Loans from other banks, however, can also decrease a bank's willingness to lend to a firm. For example, concerns about possible coordination problems with other banks (e.g., Bolton and Scharfstein 1996, Bris and Welch 2005) could trigger a decrease in the initial bank's willingness to lend to the firm. Similar predictions are obtained if firms have limited debt capacity and if higher indebtedness increases the firm's probability of default (e.g., Bizer and DeMarzo 1992, Parlour and Rajan 2001).[1] Limited debt capacity and negative externalities from outside loans may thus act as a stabilizing force in credit markets limiting positive

[1] Many seminal contributions in banking and corporate finance more generally assume that firms have limited debt capacity and that higher debt levels increase borrowers' moral hazard incentives (see, among others, Myers 1977, Holmström and Tirole 1997, Tirole 2006). As highlighted by the literature on nonexclusivity, under such settings loans from other banks impose negative externalities on existing or other banks (see, among others, Bizer and DeMarzo 1992; Kahn and Mookherjee 1998; Parlour and Rajan 2001; Bisin and Guaitoli 2004; Attar et al. 2010, 2011; and Bennardo et al. 2015 for a theoretical analysis of nonexclusivity in different game-theoretic settings covering moral hazard and adverse selection).

comovements between lenders. We bring this prediction to the data.

Using a difference-in-difference analysis, this paper studies how a bank's willingness to lend to a previously exclusive firm changes once the firm obtains a loan from another bank (which we refer to as an "outside loan") and how this change varies with the size of the outside loan and the degree to which the bank may be insulated from coordination problems and other negative externalities from such loans. Understanding how a bank's willingness to lend to a firm may change when another bank begins lending to the firm can provide important insights on factors that influence interdependencies and fragility in financial markets, and it could be informative about the role of transparency in such settings.

The analysis employs a unique data set containing information on a bank's internal limit to each firm in an institutional setting that allows for a meaningful test. This is obtained using internal data from one of the largest banks in Sweden between April 2002 and December 2008. The internal limit indicates the maximum amount that a bank is willing to lend to a firm. It represents the amount for which the bank's loan supply curve becomes vertical. Hence, changes in the internal limit represent changes in loan supply. Using this information, we investigate how the bank's internal limit to a previously exclusive firm changes once the firm acquires a loan from another bank. This would not be possible using data on outstanding loan amounts as these are equilibrium outcomes that may be driven by both demand and supply factors. The institutional setting in Sweden provides a meaningful ground for the analysis: there is a credit bureau and a collateral registry in place that allow banks to observe and react to outside loans and employ contractual features (e.g., collateral and other covenants) to mitigate any negative externalities.

To control for changes in macroeconomic and banking conditions during the event window, we benchmark the changes in the internal limit of firms that obtain loans from other banks (the treatment group) with the contemporaneous changes in the internal limit of otherwise very similar firms that do not obtain loans from other banks (the control group). The data availability allows us to match firms on a rich set of firm characteristics—both public and internal—to obtain treatment and control pairs that are similar with respect to many dimensions and are on "parallel trends" with respect to the outcome variable. As explained later, the internal variables play a pivotal role in this.

Overall, we find that loans from other banks are strategic substitutes, i.e., the initial bank wants to lend less when other banks are lending to a firm. Our estimates suggest an average treatment effect for the treatment firms of 6.2% to 8.9% (i.e., the initial bank's internal limit to total assets ratio drops on average by 6.2 to 8.9 percentage points more for the treatment firms than for similar control firms). Additional tests suggest that it is unlikely that endogeneity and selection biases are affecting the estimated treatment effect. For example, we obtain similar results if we reestimate our models for the subsample of treatment firms' whose condition was either stable or improving in the immediate period following the outside loan, suggesting that reverse causality is unlikely to be driving the bank's reaction. Following firms over time also reveals that the vast majority of the treatment firms are high-quality, growing firms that may be turning to other banks to finance some of their existing or new projects, consistent with evidence in Ioannidou and Ongena (2010). As indicated by their control counterparts, their internal limits with the bank should have increased in the absence of the outside loan. They decreased instead.

Consistent with concerns about coordination problems and higher indebtedness, we find that the decrease in the initial bank's willingness to lend is more pronounced the larger the outside loan and it is muted if the initial bank has a floating charge on the firm. A floating charge on the firm allows the initial bank's existing and future loans to maintain a senior claim on the firm's current and future assets and gives the bank strong control rights in bankruptcy (see, among others, Franks and Sussman 2005, Gennaioli and Rossi 2013, Cerqueiro et al. 2014).[2] Having a floating charge on the firm may thus insulate the initial bank from coordination and other negative externalities from higher indebtedness, which could explain why it does not adjust its willingness to lend to the firm. Additional tests holding indebtedness constant indicate that the initial bank's concerns go beyond a simple debt capacity story to one that involves coordination problems and other negative externalities.

Finally, we also study whether the initial bank further protects itself not only by changing its willingness to lend to these firms, but also by adjusting other loan contract characteristics on existing and new loans. We find that the initial bank increases its collateral requirements after the treatment firm takes an outside loan. We do not find any statistically significant changes in loan interest rates. Intensified competition between the initial and the new bank may be putting a downward pressure on interest rates. Ioannidou and Ongena (2010), for example, find that

[2] Djankov et al. (2008) find that debt contracts secured with a floating charge are enforced more efficiently: they have higher recovery rates and shorter enforcement times.

banks compete for customers by offering low interest rates to new customers to attract them in.[3]

Our paper contributes to an ongoing debate about the role of transparency in financial markets (see, e.g., Simon 1989, Musto 2004, Greenstone et al. 2006, Leuz and Wysocki 2008). Transparency—mechanisms that facilitate information transmission between market participants—may have differential effects depending on whether loans are strategic complements or substitutes. Hertzberg et al. (2011), for example, identify a "dark side" to transparency in an environment where loans from other banks are strategic complements. In a setting where a firm's repayment ability depends positively on loans from other banks, the authors show that banks decrease their lending to the firm when they expect other banks that are currently lending to the firm to crawl back. Exploiting an increase in information sharing about some firms, the authors find that banks possessing private negative information cut their lending in anticipation of this information becoming public. Transparency in their setting reinforces reactions to bad news and gives rise to credit freezes and creditor runs. Our findings suggest that firms' limited debt capacity and negative externalities from other banks may act as a stabilizing force in credit markets limiting positive comovements between different lenders. This result depends crucially on the initial bank not being insulated from the increased risk and being able to observe loans at other banks as to take actions. Our results thus give a more benevolent role to transparency allowing banks to mitigate risk exposures. Importantly, our results are not necessarily informative about how a bank may react in a setting where existing lenders crawl back.

The remainder of the paper is organized as follows. Section 2 reviews the literature and develops three testable hypotheses. Section 3 presents the data and the institutional setting. Section 4 describes our identification strategy. Section 5 discusses our results and various robustness checks. Section 6 concludes.

2. Literature Review and Testable Hypotheses

To structure our empirical analysis, drawing from the extant theoretical literature, we first discuss why a loan from another bank could alter the initial bank's willingness to lend to a firm, and we summarize the key insights from this discussion into three testable hypotheses.[4]

Theories emphasizing the role of strategic complementarities in lending argue that, all else equal, a loan from another bank could increase a bank's willingness to lend to a firm. There could be several reasons for this. A loan from another bank could be perceived as a positive signal about a firm's quality and thus increase the initial bank's willingness to lend to the firm (e.g., James 1987, Biais and Gollier 1997). An outside loan could also facilitate a worthwhile project that the initial bank cannot (or does not want to) finance alone (e.g., due to lack of sufficient liquidity as in Detragiache et al. 2000 or a too large exposure to the firm).[5] The involvement of another bank could also increase the firm's prospects. The other bank, for example, could have complementary expertise on the firm's project or a new area of expansion. Outside loans in this case would be *strategic complements* (i.e., the willingness of one bank to provide credit to a firm, reinforces another bank's willingness to lend to the firm) with the size of the complementarities increasing in the size of the other bank's involvement.

Theories emphasizing the role of strategic complementarities in lending are often referred to as theories of "credit freezes" or "creditor runs." Such theories are behind mechanisms of financial crises and fragility in financial markets (see Bebchuk and Goldstein 2011, Goldstein 2012). The source of instability in these theories arises from the interdependence in the firm's ability to repay its loans on the actions of other banks toward the firm or toward other parties that interact with the firm. In our context, when a firm engages another bank, its repayment ability to the initial bank becomes dependent on the actions of the other bank toward the firm. The prospect of coordination problems arising from this codependence could actually induce the initial bank to decrease its willingness to lend to the firm.[6] Loans

[3] This paper and Ioannidou and Ongena (2010) study firms that break an exclusive lending relationship by turning to another bank but focus on different aspects. Whereas this paper focuses on what happens to the firms' credit conditions at their initial bank once they break an exclusive relationship, Ioannidou and Ongena (2010) study the loan interest rates that these firms receive from their new banks (vis-à-vis their initial banks) and how they develop over time.

[4] Because of data availability and the institutional setting in Sweden, our analysis concerns only bank loans to firms. The theory, however, is more general and applies to any type of borrower and lender. To match the empirical analysis that follows, our discussion in this section is framed in terms of banks and firms.

[5] We believe that this possibility is rather unlikely in our setting. As discussed later on, the bank in our data set is one of the largest banks in Sweden, whereas the vast majority of the firms in our sample are small and medium size enterprises. Moreover, until the end of the sample period (2008), Sweden was relatively unaffected by the financial crisis. Hence, bank liquidity constraints seem unlikely in our sample.

[6] Several seminal contributions in banking and corporate finance highlight such coordination problems. Bolton and Scharfstein (1996), for example, point to higher costs of debt renegotiation and reduced expected liquidation values potentially leading to asset grabbing and creditor runs (see also von Thadden et al. 2010). Brunner and Krahnen (2008) study how banks in Germany aim

from another bank will be *strategic substitutes* in this case. The degree of substitutability may increase in the size of the outside loan as larger outside loans could give rise to more coordination problems.

Loans from other banks could be strategic substitutes for additional reasons. If firms have a certain "debt capacity" (i.e., a maximum amount of debt they can support given their assets), a loan from another bank should trigger a decrease in the initial bank's willingness to lend as the outside loans use up some of the "slack" on the firm's debt capacity.[7] In the absence of other positive or negative externalities from outside loans, if firms have a limited debt capacity, an outside loan should trigger an equal decrease in the initial bank's willingness to lend to the firm, whereas a similar size loan from the initial bank should trigger no reaction as the firm is merely moving along its supply curve with the initial bank.

If debt levels matter, however, outside loans are likely to generate additional negative externalities to the initial bank and thus trigger a larger negative reaction. As highlighted by the literature on the nonexclusivity of financial contracts, the higher total indebtedness could increase the firm's probability of default, inducing a devaluation on the initial bank's existing loans to the firm that is not taken into account by the new bank. This is in sharp contrast to a one-bank environment where all effects of additional loans are internalized. The specific channel through which the probability of default increases varies across models. In Bizer and DeMarzo (1992) and Bennardo et al. (2015), the outside loans exacerbate moral hazard: the higher total indebtedness reduces the firm's work effort resulting in a higher probability of default as in Holmström and Tirole (1997). In Parlour and Rajan (2001) and Bennardo et al. (2015), the nonexclusivity creates incentives for strategic default. These incentives increase in the total amount borrowed and the fraction of assets that firms can exempt from bankruptcy proceedings.[8]

Overall, it is theoretically unclear whether an outside loan should trigger an increase or decrease in the initial bank's willingness to lend to a firm. This is ultimately an empirical question, which motivates our first two hypotheses:

Hypothesis 1 (H1). *If outside loans are strategic complements, an outside loan should trigger an increase in the initial bank's willingness to lend to a firm. Everything else equal, a larger outside loan should trigger a larger positive reaction in the initial bank's willingness to lend.*

Hypothesis 2 (H2). *If outside loans are strategic substitutes, an outside loan should trigger a decrease in the initial bank's willingness to lend to a firm. Everything else equal, a larger outside loan should trigger a larger negative reaction in the initial bank's willingness to lend.*

Next, we discuss how contractual features that banks could employ can mitigate coordination problems and negative contractual externalities from outside loans. In principle, banks could use *covenants* that make loan terms contingent on future borrowing from other sources. It has been argued, however, that such covenants are not widely used because they introduce other inefficiencies and are difficult to apply in practice.[9] Moreover, the ability of covenants to mitigate the increased risk is also bounded by limited liability and in some cases they may even aggravate problems by creating incentives for opportunistic lending (see Attar et al. 2010).

Another approach, first discussed in Fama and Miller (1972), is to *prioritize debt* (i.e., allow the initial bank's existing debt to the firm to retain seniority over new loans). Although prioritization avoids dilution of prior debt, as Bizer and DeMarzo (1992) point out, this will not solve externalities from sequential

to mitigate coordination problems by forming "bank pools" (i.e., lender coordination through private contracting) when firms are in distress. Bris and Welch (2005) argue that more valuable firms may prefer to limit the number of creditors to discipline them, and Ongena et al. (2012) suggest that it may be better for a debtor to deal with a relationship lender that has lower monitoring costs. Finally, Perotti and Spier (1993) argue that increased leverage (e.g., through equity for debt swaps) may be a tool to induce concessions from senior creditors.

[7] The concept of debt capacity is referred to in theoretical and empirical works in corporate finance. Myers (1977), for example, defines debt capacity as the point at which an increase in the use of debt reduces the total market value of the firm's debt. Others define it as "sufficiently high" debt ratios such that the cost of financial distress curtails further debt issues. In theoretical models such as Holmström and Tirole (1997) or Tirole (2006), debt capacity is determined by the pledgeable income to outside investors.

[8] Because new banks do not pay for the externality on the existing loans, they can offer loans with more attractive terms and create incentives for opportunistic lending (see Bennardo et al. 2015).

[9] For example, using debt covenants, creditors could permit future borrowing only with the approval of existing creditors. This, however, would give veto power to existing creditors and open the door to hold-up problems (see Smith and Warner 1979 and Bizer and DeMarzo 1992). Although hold-up problems could be mitigated if contracts could specify ex ante the exact circumstances under which borrowing would be allowed, designing fully state-contingent contracts is very difficult in practice and often prohibitively expensive. Making debt callable is an alternative mechanism. As pointed out in Bizer and DeMarzo (1992), this would solve the problem only if the call price equals the fair market value of debt in the absence of further borrowing. For this to be true, the contract would either have to specify the fair market value ex ante, which is as complex as writing a fully state-contingent contract, or base the call price on the ex post market price of debt, which again gives rise to hold-up problems.

contracting if the higher levels of debt increase incentives for moral hazard.[10] Asking firms to pledge *collateral* could mitigate the increased risk by reducing the lenders losses in the event of default and/or mitigating the firms' likelihood of default. Collateral, for example, could help lenders select better quality firms (e.g., Bester 1985, 1987). It could also help induce better behavior ex post. In Holmström and Tirole (1997), for example, the fear of losing the pledged assets mitigates incentives for moral hazard by inducing firms to exert higher effort. Similarly, in Parlour and Rajan (2001), collateral provides a credible commitment not to engage in strategic default since it is by definition a nonexempt asset.[11]

A *floating charge* on the firm's assets—a special form of collateral that carries over to future loans—could be an effective way to mitigate the extra risk from outside loans. In addition to the functions of the collateral highlighted above, taking a floating charge on the firm allows the initial bank's current and future loans to maintain a senior claim on the firm's current and future assets, preventing subsequent lenders from undermining their seniority.[12] Other types of collateral such as fixed charges, pledges, and liens may be less effective in preserving the seniority of the existing lender because they do not extend beyond the specified assets and loans.[13] A floating charge on a firm could also help mitigate coordination failures and premature liquidations. Recent work by Gennaioli and Rossi (2013) argues that a floating charge gives its holder strong control rights over the reorganization versus liquidation decision of a firm and an "equity-like" stake on the reorganization proceeds, preventing premature liquidations and coordinations failures between multiple creditors. This leads us to our third testable hypothesis:

Hypothesis 3 (H3). *If a floating charge on a firm mitigates coordination problems and other negative externalities from outside loans, everything else equal, a loan from another bank should lead to a smaller decrease in the initial bank's willingness to lend.*

3. Data and Institutional Setting

The paper makes use of a unique data set containing information on all corporate clients of one of the four largest banks in Sweden.[14] In particular, the data contain detailed information on the contract and performance characteristics of all commercial loans between April 2002 and December 2008 as well as information about the borrowing firm. For each loan, we observe the origination and maturity dates, type of credit, loan amount, interest rate, fees, and collateral, as well as its subsequent performance. For each firm, we observe its industry, ownership structure, credit history, and credit scores, as well as the bank's internal limit to the firm—our key dependent variable.

A bank's internal limit to a firm indicates the maximum amount that the bank is willing to lend to the firm. It is based on the firm's perceived creditworthiness and repayment ability and is set by a credit committee during the so called "limit review meetings." It is an internal variable that is not communicated to the firm and does not involve any commitment to the firm.[15] Loans to a firm cannot exceed this limit. Changes in the internal limit are determined by the credit committee during a review meeting where the firm's repayment capacity is reevaluated. From an economic point of view, the internal limit can be thought of as the point where a bank's loan supply becomes vertical. Hence, changes in the internal limit capture changes in loan supply. Economic theory motivates the existence of such limits on the inability of price and other mechanisms to clear credit markets (Stiglitz and Weiss 1981).[16] From an internal organizational point of view, internal limits alleviate the need

[10] Ayotte and Skeel (2013) discuss how prioritization influences coordination problems in an environment where creditors need to decide on new loans or to renegotiate outstanding loans in bankruptcy. They argue that different layers of seniority imply that creditors will benefit differentially, aggravating coordination problems.

[11] In the context of Attar et al. (2010), valuable collateral could be viewed as a way to sidestep limited liability (i.e., an alternative to using courts to enforce unlimited liability).

[12] A floating charge is a general security interest that pertains to a broad pool of a company's assets including intangibles or circulating capital, e.g., cash, receivables, and future cash flows and assets (see, e.g., Franks and Sussman 2005, Cerqueiro et al. 2014). These assets are not individually identified. The property underlying the floating charge can constantly change as part of the firm's normal course of business. The floating charge extents automatically to any underlying property acquired by the firm while the debt is outstanding and can be reused once the debt is repaid.

[13] Fixed charges, pledges, and liens are security interests on specified assets and loans and as such do not carry over to any unspecified current or future assets and loans.

[14] The Swedish banking market is rather concentrated with the four largest banking groups accounting for around 80% of total banking assets. At the end of 2003, there was a total of 125 banks established in Sweden.

[15] This is in sharp contrast to credit lines that are communicated and typically committed (see, e.g., Jiménez et al. 2009, Sufi 2009, and Norden and Weber 2010 on the role of credit lines in alleviating financial and liquidity constraints). Although internal limits are not directly communicated, firms could indirectly learn their internal limits when they become binding. We return to this point when we discuss our methodology.

[16] The seminal work of Stiglitz and Weiss (1981) highlights that the higher risks associated with higher amounts of borrowing cannot always be priced through higher interest rate as the interest rate itself may affect the riskiness of the pool of borrowers they attract through adverse selection, moral hazard or both. Hence, above a certain level of borrowing, a bank is unwilling to lend more no matter what the interest rate is. The internal limit represents this

for a committee review meeting for the approval of every loan and grant some degree of autonomy to loan officers.

To determine a firm's internal limit, the credit committee makes use of both internal proprietary information (e.g., the loan officer's report) as well as external public information on the firm's repayment ability. Anticipated demand from the firm is not a factor that is taken into account. The loan officer's report to the committee does not include information on anticipated demand from the firm.[17] Important information about a firm's repayment ability and exposures with other counterparties is obtained from the main Swedish credit bureau, Upplysningscentralen (UC), and the firm. Through UC, the bank can observe whether the firm had recent repayment problems with other bank and/or other nonbank counterparties. It can also observe the firm's external rating, and the value of collateral on all outstanding bank loans. The information is updated monthly and at any point in time the bank can obtain a report for the past 12 months. Through UC, the bank can also observe whether other banks or creditors have requested information about one of their clients, which can give a signal to the bank that their firm may be "shopping" around for credit. The identities of the other banks or creditors cannot be observed through the registry. The bank, however, can and typically follows up by requesting additional information directly from the firm either at the firm's annual reevaluation meeting or earlier if deemed necessary.[18]

A firm's internal limit is typically reevaluated once a year during a limit review meeting scheduled a year earlier at the end of the previous meeting. However, if the firm's condition changes substantially, the meeting could be moved to an earlier date. Such interim meetings may be associated with more intense monitoring (Cerqueiro et al. 2014). One potential concern, however, is that interim meetings may be triggered by increased demand from the firm, which would make the internal limits dependent on firms' loan demand. To investigate this possibility, we examine adjustments in the internal limits following interim versus predetermined meetings. If increased loan demand is driving interim meetings and the resulting adjustments in the limits, we would observe that a larger fraction of interim meetings, as opposed to meetings that take place on time, are followed by upward revisions of the internal limits. We find that this is not the case. About 42% of meetings take place earlier than their predetermined annual date, 7% of which are followed by an increase in the limit. The corresponding number for meetings that take place on time is 6%, which is very similar, suggesting that demand is unlikely to be driving these adjustments in the interim meetings.

In sum, the Swedish institutional setting is such that it allows banks to learn whether one of their customers obtains loans from another bank and thus take measures to mitigate some of the resulting externalities—an important prerequisite for our analysis. Moreover, Swedish firms have few bank-lending relationships (see, e.g., Ongena and Smith 2000), which implies that nonexclusivity events are an integral part of this institutional setting. This is not the case in Italy, for example, where most firms have multiple bank lending relationships. Hence, the combination of institutional features and data availability provides a unique opportunity to examine how banks view outside loans by studying how the internal limit changes following the origination of loans from another bank.

To obtain additional information about the firm, the bank data set is merged with accounting data from the main credit bureau, UC, and information from the Swedish registration office, Bolagsverket. Accounting data are only available for corporations. This implies that our sample consists only of limited liability firms. To determine a firm's age, the firm's date of registration is obtained from Bolagsverket. The available information from Bolagsverket allows us (as well as current or prospective lenders) to also determine whether the firm has posted a floating charge on any of its outstanding loans. This type of information has been found to facilitate a more efficient use of collateral in debt contracts (Haselmann et al. 2010). Data on the value and volatility of the fixed and floating charge assets are obtained from the bank data set and the firm's accounting statements.[19]

Loan covenants are rarely used in Swedish business lending. Although the bank data set does not include any indicators on this, a recent survey by Hansson and Lennartsson (2011) shows that the five

amount. Other nonprice mechanisms such as collateral and other covenants could expand the bank's willingness to lend to a firm (see, among others, Bester 1985, 1987; Boot et al. 1991; Boot and Thakor 1994), but do not fully eliminate ex post frictions as borrower moral hazard and other frictions may kick in. We highlighted some of these frictions in our earlier discussion on the role of collateral and other covenants.

[17] In subsequent robustness analysis, we further study the potential role of anticipated demand in our analysis.

[18] The information that loan officers collect from the firm includes items such as revenues, ongoing expenses, as well as detailed information on how operations are financed.

[19] The law determines the types of assets that can be pledged under a floating charge claim and the creditors' rights when a borrower defaults. As of 2004, a floating charge includes inventory; accounts receivable; equipment; real estate; financial assets such as cash, bank deposits, bonds, and stocks; and can be invoked during bankruptcy like other collateral types (see Lag 2003:528 om Företagsinteckningar and Cerqueiro et al. 2014).

largest banks in Sweden (our bank is one of them) make little use of loan covenants. Out of 155 loan officers, 17% indicated that they never used covenants and about 58% indicated that they use covenants only rarely. This is in line with what the bank told us when we asked them about their use of covenants and an earlier survey conducted by the Riksbank in 2004, where Swedish loan officers were asked about their use of covenants in loan agreements. Hansson and Lennartsson (2011) indicate that the main reason banks do not use covenants is because they favor the use of the floating charge instead.[20] Covenant violations give a bank rights to "call in" a loan prior to maturity, which they argue is less beneficial than a floating charge. Taking a floating charge on a firm allows the bank's current and future loans to maintain a senior claim on the firm's current and future assets and in combination with a negative pledge clause on the security agreement they prevent future lenders from undermining their seniority.[21]

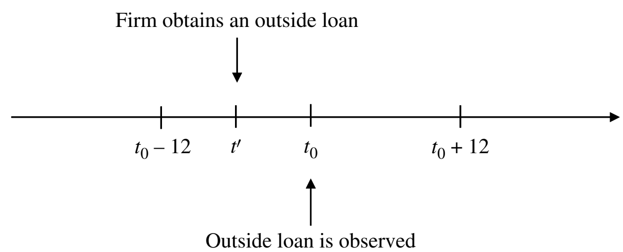
4. Methodology

Below, we describe in detail how our treatment and control groups are defined as well as the firm characteristics that we match on and why the resulting matched control firms give a reasonable proxy of the unobserved counterfactual.

4.1. Treatment and Control Groups: Definition and Descriptive Statistics

The *treatment group* consists of firms that enter the sample with an exclusive relationship with our bank and during the sample period obtain a loan from another bank.[22] We define a relationship as exclusive if the firm borrows only from our bank for at least one year and we refer to the first loan(s) from other banks as "outside loan(s)."[23] Loans from the firm's initial bank are referred to instead as "inside loans." We identify whether a firm obtains an outside loan by comparing the bank's total outstanding loans to the firm with the firm's total bank debt reported in the firm's annual accounting statements. This allows us to once a year identify whether a firm borrows from other banks.[24] To investigate how the bank responds to an outside loan, we compare the internal limits around the time of the nonexclusivity event.

Figure 1 **The Event Window**



Notes. This figure illustrates our event window. Let t' indicate when the firm obtains a loan from another bank, which we refer to as outside loan. Let t_0 indicate the time that the firm's first accounting statements following the nonexclusivity event are reported and $t_0 - 12$ to indicate the time of the firm's last accounting statements prior to the nonexclusivity event. Since the bank decides on the internal limit once a year, to evaluate how the bank reacts to the outside loan, we use a primary event window that ranges between $t_0 - 12$ and $t_0 + 12$.

Figure 1 illustrates our event window. Let t' indicate when the firm obtains a loan from another bank (i.e., when the nonexclusivity event takes place). Let t_0 indicate the time that the firm's first accounting statements following the nonexclusivity event are reported (i.e., this is when we can first observe the outside loan(s)) and $t_0 - 12$ indicate the time of the firm's last accounting statements prior to the nonexclusivity event. Since the bank decides on the internal limit once a year, there are two possibilities about the timing of any reaction following the nonexclusivity event: the meeting is held either sometime between t' and t_0 or between t_0 and $t_0 + 12$. Hence, to evaluate how the bank reacts to the nonexclusivity event, we study changes in the bank's internal limits between $t_0 - 12$ and $t_0 + 12$.[25] To further investigate the timing of these changes, we also present results for the year prior to the event window and for the two subperiods of the event window separately (between $t_0 - 12$ and t_0 and between t_0 and $t_0 + 12$).

Because of the length of the event window and the available sample period, the treatment group contains firms that obtain a loan from another bank any time during the period 2004:04 to 2007:12. Given

[20] This is also consistent with recent evidence for loans to small firms in the United States. Using a proprietary database, Minnis and Sutherland (2015) find that U.S. banks make very infrequent use of covenants when lending to small privately held firms in the United States. Covenants are present in only 1% of their 4,518 loans. They find instead that collateral and other mechanisms such as relationship length and borrower reputation are more important.

[21] Under the Swedish bankruptcy law, a fixed charge enjoys a higher priority over a floating charge even if it is subsequently issued. Hence, banks typically combine the floating charge with the negative pledge clause on the security agreement that prohibits firms from issuing a fixed charge on their floating charge assets.

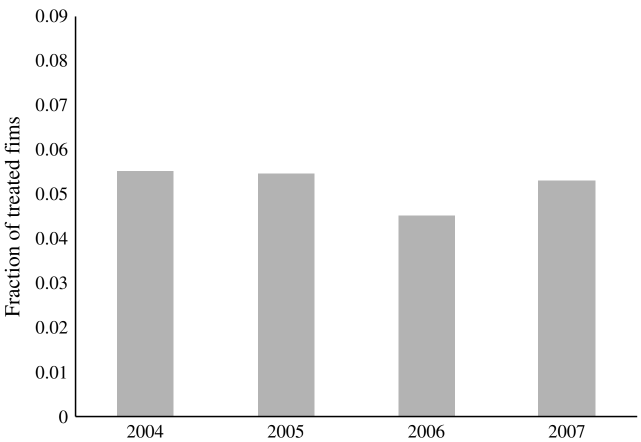
[22] About 70% of firms in the sample have a single-bank lending relationship.

[23] The results presented in the paper are robust to using two or three year cut-offs, even though the sample is then substantially reduced. Results are available from the authors upon request.

[24] This includes commercial, savings, and cooperative banks that are either domestic or foreign owned.

[25] If the firm's relationship with the bank is terminated prior to $t_0 + 12$, we use the last observed limit between t_0 and $t_0 + 12$. This involves 6% of the treatment firms. About 5% of Swedish firms have accounting periods longer than one year. We exclude those firms from our sample.

Figure 2 **Incidence of Nonexclusivity Events Each Year**



Note. This figure reports the number of treated firms in each year as a percentage of the firms with an exclusive relationship with our bank for the two prior years for which the limit is not binding.

that data are available between 2002:04 and 2008:12, this allows us to verify that all firms enter the sample period with at least a one year exclusive relationship with our bank and gives us one year after the last possible nonexclusivity event to observe the bank's limit at $t_0 + 12$. We omit firms with an internal limit lower than SEK100,000 (approximately $14,000) at time $t_0 - 12$ since such small exposures are typically determined rather "mechanically."[26] We also do not include nonexclusivity events with amounts less than 1% of the firm's internal limit at $t_0 - 12$ because these may stem from noise in combining different data sources. Since our goal is to study how the bank's loan supply reacts to the outside loan, we do not include firms whose limit at $t_0 - 12$ is binding (i.e., it is equal to their outstanding loans and unused credit lines at $t_0 - 12$) and thus could be driven by both demand and supply factors. This yields 991 treatment firms.

Figure 2 reports the number of treatment firms in each year as a percentage of the firms with an exclusive bank-lending relationship for which the internal limit is not binding. As can be observed in Figure 2, this percentage is fairly constant over time varying between 4.5% and 5.5% and is comparable to rates found in other studies (e.g., 4% in Farinha and Santos 2002 for Portugal and 4.5% in Ioannidou and Ongena 2010 for Bolivia).

To control for changes in macroeconomic and banking conditions during the event window, we employ a difference-in-difference analysis using a matching procedure. Matching methods estimate the counterfactual outcome—the treatment firms' internal limit in the absence of an outside loan—using the outcomes of a subsample of otherwise "similar" control firms. A matching technique requires that the treatment and control groups contain similar firms and that the matched firms are on "parallel trends" with respect to the outcome variable. The existing literature suggests that this is not unlikely. Although a firm may want to obtain credit from another bank, it may not always be able to— at least not instantaneously. Information asymmetries between existing and new banks may prevent firms from obtaining credit elsewhere. In Sharpe (1990), for example, when a high-quality firm tries to obtain credit from a new uninformed bank, it gets pooled with low-quality firms and is offered higher loan rates. This implies that high-quality firms are less likely to accept an offer from a new bank and that low-quality firms are more likely to accept such offers. In an amended version of the Sharpe (1990) model, von Thadden (2004) shows that because of "winner's curse" banks compete with other banks for their customers using "optimal randomization" for borrowers that are at least to them observationally identical. This implies that the treatment and control groups may contain very similar firms (i.e., there is "overlap"). It also implies that a higher proportion of treatment firms is of lower quality with respect to factors that are observable to their initial, but not to the new bank. Evidence in Ioannidou and Ongena (2010) corroborates this prediction. The authors find that although most switching firms are high-quality firms with respect to observable characteristics, a larger fraction of them is worse with respect to unobservable risk characteristics than a random draw of the population would suggest (see Figure 5 in Ioannidou and Ongena 2010). Hence, one needs to match not only on publicly observable firm characteristics, but also on factors that may be unobservable to the new bank, but are observable to the initial bank and affect its credit policies toward the firm. Our matching procedure is geared to meet this challenge.

We begin by identifying a possible set of matched control firms. This includes firms that, like the treatment firms, have an exclusive relationship with our bank at $t_0 - 12$ for at least one year, but unlike the treatment firms retain this exclusive relationship for at least until the end of the event window, $t_0 + 12$. Using information from the accounting statements, the credit bureau, and the bank data set we match these two groups with respect to several firm characteristics at the beginning of the event window, $t_0 - 12$. The set of publicly observable characteristics includes industry, age, size, asset growth, tangible assets, cash flows, indicators of leverage such as total debt to total assets and total bank debt to total assets, external credit rating, and indicators of recent repayment problems. Some of these variables are observable

[26] For example, firms may hold a company credit card with a minimum amount. Since we want to focus on strategic interactions, we do not include such automated decisions.

(to us and other banks) through the firm's accounting statements. Others are observable through the credit bureau. This yields our first set of matching variables, which we refer to as "Match 1." To control for bank proprietary information, we additionally match on the firm's internal limit, the distance to limit (i.e., the difference between the firm's internal limit and its outstanding bank debt and committed but unused credit lines), and the interest rate on the most recently originated loan at the initial bank.[27] This yields our second more preferred matching set, which we refer to as "Match 2."[28] Table 1 provides detailed definitions for all matching variables as well as all other variables used in the paper.

For our benchmark analysis, we match on each of these variables individually at $t_0 - 12$. For discrete variables, we use exact matching. For continuous variables, we use caliper matching using a 0.5 standard deviation radius for each of our matching variables. Our matching technique retains any pair that satisfies the matching criteria even if a matched control firm is also a control for another treatment firm (i.e., we allow for "replacement") or a treatment firm has more than one control firm that satisfies the matching criteria. Replacement allows for better matches and less bias, but at the expense of precision (Rosenbaum 1995). Allowing for multiple control firms allows treatment firms that are more likely to satisfy the overlap assumption to have more weight in the estimation. These are clearly subjective choices. Hence, following good practice in the literature, we report several robustness checks using different estimation methods.

Our baseline matching exercise yields 1,502 pairs corresponding to 290 treatment firms and 947 control firms for Match 1 and 302 pairs with 125 treatment firms and 260 control firms for Match 2. As can be observed in Table 2, the treatment firms for which a match can be found are of better quality than their 991 treatment counterparts, especially in Match 2. They are older, smaller firms, with lower growth, more tangible assets, higher profitability, higher leverage ratios, and lower default risk (e.g., lower default probabilities, better ratings, and perfect credit histories). Hence, by matching we retain the subsample of treatment firms that are of high quality and may be turning to another bank because of changing needs or because they were able to secure better terms at the new bank. We return to this in robustness checks later on when we follow the treatment firms over time to study how they fair relative to their matched control firms in the year or so after the outside loan. As can be observed in Table 2, the outside loan is quite sizeable. In all samples, the median outside loan is 5% of the firm's total assets. Given a median debt to total assets ratio of 21% to 33% (depending on the sample), this implies that the median outside loan is somewhere between 24% to 15% of the firm's total debt with the bank.

Next, we compare the differences between the treatment and control groups. As can be observed in Table 2, matching removes a lot of the differences between the two groups. However, important differences remain, particularly in Match 1. In Match 1 the treatment firms are slightly younger, faster growing, with lower leverage, and perfect credit histories, but with higher default probabilities and worse credit ratings. This is not the case in Match 2. The two groups of firms are much more similar and the small differences that remain suggest that the treatment firms may be of slightly better quality. Nevertheless, as can be observed at the bottom of Table 2, although in the year prior to the outside loan the two sets of firms in Match 2 are on similar paths with respect to their limits, this changes dramatically after the outside loan, with their limits being substantially and differentially reduced relative to their matched counterparts. Whereas the treatment firms' limit is reduced by 6% to 4%, the limit of their matched control firms increased by 2% to 4%.[29] Taken literally, these results suggest that while the treatment, firms' limit should have increased by 2% to 4%, it decreased by about 6% to 4%.

Although the differences in their characteristics presented above cannot explain the differential decrease in the treatment firm's internal limits, in the empirical analysis that follows we weight observations based on the quality of the match. The weights are based on a distance measure that is obtained by first standard normalizing the matching variables and then calculating the distance between each pair by summing their absolute differences. Larger weights are assigned to

[27] When a firm has more than one recently originated loan at $t_0 - 12$, we use the highest interest rate among them. Similar results are obtained if we use the average interest rate or the bank's internal rating instead. Matching on the interest rate as opposed to ratings is preferred because the ratings are sometimes missing.

[28] Matching on internal variables such as loan interest rates and internal limit also helps control for other variables that are important in shaping financial contracts such as relationship length (Ioannidou and Ongena 2010). In unreported descriptive statistics, we in fact confirm that the treatment and control firms in Match 2 are also similar with respect to relationship (both economically and statistically). For the entire sample and for Match 1 we instead find statistically significant differences between the two groups. As discussed later, additionally matching or adding relationship length as a control variable has no material effect on the paper's key results.

[29] In unreported tests, we also tested whether the changes in the internal limits reported at the bottom of Table 2 are each individually different from zero. In all cases the reported average changes were found to be statistically different from zero, at least at the 10% level.

Table 1 Variable Names and Definitions

Variable names	Definitions	Matching Sets 1	Matching Sets 2
I. Matching variables			
Calendar time			
Month-Year	Dummy variables for each of the 45 months in the sample (2003:04–2006:12)	X	X
Public firm characteristics			
Industry	Two-digit NACE codes	X	X
Age	Number of years since the date of registration	X	X
TotalAssets	Total firm assets (in SEK1,000)	X	X
AssetGrowth	Total assets at t / Total assets at $t-12$	X	X
TangibleAssets	Fixed assets, accounts receivable, and inventories to total assets	X	X
Profitability	Earnings before interest and taxes to total assets (EBIT)	X	X
Leverage	All debt obligations excluding unused credit lines and taxes to total assets	X	X
BankDebt	All bank debt obligations excluding unused credit lines to total assets	X	X
DefaultProbability	Probability of default (PD) in the next year estimated by the main Swedish credit bureau	X	X
ExternalRating	Takes values 1, 2,…,5, where 1 indicates the worst and 5 the best rating	X	X
RepaymentProblems	A dummy that equals 1 if recent repayment problems with third parties and 0 otherwise	X	X
Private firm characteristics			
LoanInterestRate	Annualized interest rate on outstanding loans at the initial bank (in %)		X
InternalLimit	Internal limit to total assets		X
DistancetoLimit	(Internal limit − Outstanding bank debt − Unused credit lines) / Internal limit		X
II. Outside loan			
OutsideLoan	A loan initiated at another bank between t_0-12 and t_0 to total assets at t_0-12		
III. Limit changes			
$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment}}$	$[(Limit_{t_0+12}-Limit_{t_0-12})/Assets_{t_0-12}]_{\text{treatment}}$		
$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{control}}$	$[(Limit_{t_0+12}-Limit_{t_0-12})/Assets_{t_0-12}]_{\text{control}}$		
$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}}$	$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment}} - (\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{control}}$		
IV. Other variables used in the analysis			
FloatingCharge	A dummy that equals 1 if initial bank's debt is secured with floating charge and 0 otherwise		
FloatingChargeValue	Value of floating charge assets (estimated by the bank)/Committed debt		
FloatingChargeVolatility	Three-year earnings volatility /three-year average assets (if floating charge = 1)		
OtherCollateral	A dummy that equals 1 if the initial bank 's debt is secured by any other type of collateral with value greater or equal than 80% of the bank's outstanding debt		
InternalRating	Takes values 1, 2,…,5, where 1 indicates the worst and 5 the best rating		
InsideLoan	New loan from the initial bank		
$(\Delta InterestRate_{t_0+12,\,t_0-12})_{\text{treatment, control}}$	$[InterestRate_{t_0+12}-InterestRate_{t_0-12}]_{\text{treatment}} - [InterestRate_{t_0+12}-InterestRate_{t_0-12}]_{\text{control}}$		
$(\Delta Collateral_{t_0+12,\,t_0-12})_{\text{treatment, control}}$	$[Collateral_{t_0+12}-Collateral_{t_0-12}]_{\text{treatment}} - [Collateral_{t_0+12}-Collateral_{t_0-12}]_{\text{control}}$, with *Collateral* being the collateral value/loan amount at origination		

Notes. This table defines all variables used in the analysis. It also indicates which matching variables are used in Match 1 and 2. For discrete variables we use exact matching, and for continuous variables we employ a 0.5 standard deviation matching window.

control firms for which the distance is smaller.[30] This allows us to more precisely estimate the treatment effects of interest and reduce possible biases arising from differences between the two groups. As can be observed in Table 3, putting more weight on the better matches helps remove any remaining differences between the matched pairs.

4.2. Empirical Specifications

Using the matched samples, we estimate the following baseline model:

$$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = \alpha + \varepsilon, \quad (1)$$

where $(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}}$ is the difference in the adjustment of the internal limit between the treatment firms and the matched control firms scaled by their respective total assets at t_0-12, which we refer to as the bank's standardized response:

$$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = \left[\frac{Limit_{t_0+12}-Limit_{t_0-12}}{TotalAssets_{t_0-12}}\right]_{\text{treatment}} - \left[\frac{Limit_{t_0+12}-Limit_{t_0-12}}{TotalAssets_{t_0-12}}\right]_{\text{control}}.$$

The dependent variable is scaled by total assets to enhance comparability across firms of different size and we use total assets prior to the outside loans to avoid endogeneity problems. The constant term is

[30] We calculate the weights as follows. Let $D_{i,j}$ be the distance between treatment firm i and control firm j. The weight for each pair is calculated as $(1/D_{i,j})/\sum_j(1/D_{i,j})$, such that the sum over each treatment firm equals one. Unreported estimations show that results are robust to alternative weighting schemes, such as, for example, equal weights.

Table 2 Descriptive Statistics

	Panel A: Prior to matching						Panel B: After matching											
	Treatment			Control			Treatment (Match 1)			Control (Match 1)			Treatment (Match 2)			Control (Match 2)		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
I. Firm characteristics																		
Public																		
Age	18.83	15.00	14.60	19.53	16.00	15.20	21.16	17.00	14.22	23.80***	19.00***	15.92	22.07	17.00	15.59	22.55	17.00	16.40
TotalAssets	389,000	3,093	7,600,000	140,000**	3,029	3,380,000	12,204	3,403	36,341	11,237	3,692	34,381	12,400	3,826	30,769	12,003	3,922	28,703
AssetGrowth	1.12	1.02	0.72	1.12	1.01	2.27	1.04	1.02	0.17	1.02**	1.00	0.12	1.04	1.03	0.12	1.01**	1.00**	0.10
TangibleAssets	0.72	0.81	0.27	0.67***	0.75***	0.28	0.84	0.89	0.14	0.84	0.88	0.13	0.86	0.89	0.12	0.86	0.89	0.12
Profitability	0.04	0.04	0.17	0.05	0.06***	0.24	0.05	0.05	0.07	0.06	0.05	0.06	0.05	0.05	0.06	0.06	0.05	0.06
Leverage	0.43	0.42	0.30	0.45	0.44***	0.31	0.49	0.47	0.22	0.52*	0.51*	0.20	0.53	0.51	0.19	0.55	0.54	0.20
BankDebt	0.27	0.21	0.25	0.28**	0.22***	0.26	0.32	0.26	0.24	0.35**	0.32**	0.25	0.35	0.33	0.22	0.41**	0.38**	0.24
DefaultProbability	3.11	1.20	5.88	2.32***	0.90***	5.43	1.82	1.20	2.64	1.41***	1.00*	1.83	1.54	1.00	2.05	1.38	1.00	2.21
ExternalRating	3.19	3.00	1.12	3.41***	3.00***	1.04	3.33	3.00	0.87	3.45**	3.00**	0.78	3.38	3.00	0.78	3.51	3.00	0.77
RepaymentProblems	0.03	0.00	0.18	0.02***	0.00***	0.13	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Private																		
InternalLimit	0.43	0.37	0.31	0.42***	0.36***	0.35	0.44	0.42	0.22	0.47*	0.44*	0.25	0.46	0.44	0.19	0.50**	0.47**	0.20
DistancetoLimit	0.13	0.05	0.19	0.16	0.07	0.22	0.11	0.04	0.15	0.08***	0.02***	0.13	0.07	0.03	0.08	0.05***	0.027***	0.06
LoanInterestRate	6.42	6.60	2.20	6.31***	6.45***	2.23	6.45	6.52	1.70	6.22*	6.30**	2.00	6.29	6.30	1.64	6.10	6.20	1.72
II. Outside loan																		
OutsideLoan	0.18	0.05	0.52	—	—	—	0.13	0.05	0.22	—	—	—	0.13	0.05	0.23	—	—	—
III. Limit changes																		
$\Delta Limit_{t_0+12,t_0-24}/Assets_{t_0-24}$	0.02	−0.01	0.26	0.02	0.00**	0.44	0.01	−0.01	0.15	0.01	−0.01	0.17	−0.01	−0.01	0.07	−0.02	−0.02	0.05
$\Delta Limit_{t_0+12,t_0-12}/Assets_{t_0-12}$	−0.06	−0.03	0.34	0.01***	0.00***	0.40	−0.06	−0.03	0.25	0.02***	−0.01***	0.29	−0.04	−0.03	0.20	0.04**	−0.01***	0.30
Number of firms		991			7,743			290			947			125			260	
Number of observations		991			18,862			290			1,187			125			287	

Notes. Panel A presents descriptive statistics for the characteristics of treatment and control (i.e., the universe of exclusive firms) groups in the sample, and panel B presents descriptive statistics for the treatment and control groups in Match 1 and Match 2. Descriptive statistics for the control group are based on the number of observations, which is different from the number of unique control firms. The same control firm can serve as a control to different treatment firms at different points in time. All variables are defined in Table 1 along with the list of matching variables for Match 1 and Match 2. Differences in means are assessed using the student's t-test. Differences in medians are assessed using the Wilcoxon-Mann-Whitney test for continuous variables and the Pearson's chi-square test for categorical variables.

***, **, and * reported next to the mean and median values of each control group indicate whether the corresponding values are statistically different with respect to the corresponding treatment group at the 1%, 5%, and 10% levels, respectively.

Table 3 **Matched Pair Differences**

	Match 1		Match 2	
	No weights	With weights	No weights	With weights
I. Firm characteristics				
Public				
Age	−0.422	−0.468	0.487	1.173
TotalAssets	−210.000	−425.902	905.000	1198.000
AssetGrowth	0.005**	0.004	0.012***	0.017
TangibleAssets	0.017***	0.011	0.012	0.014
Profitability	−0.003**	−0.001	0.000	0.001
Leverage	−0.003	0.003	−0.008	−0.005
BankDebt	−0.009	0.001	−0.022***	−0.021
DefaultProbability	−0.015	0.013	−0.118*	−0.166
ExternalRating	0.000	0.000	0.000	0.000
RepaymentProblems	0.000	0.000	0.000	0.000
Private				
InternalLimit	−0.020***	−0.010	−0.015**	−0.013
DistancetoLimit	0.024***	0.021*	0.008**	0.012
LoanInterestRate	0.012	0.146	−0.021	−0.011
II. Limit changes				
$\Delta Limit_{t_0+12,\,t_0-24}/Assets_{t_0-24}$	−0.015***	−0.008	0.000	0.002
$\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12}$	−0.062***	−0.062***	−0.041**	−0.089**
Number of observations	1,502	1,502	302	302

Notes. This table reports matched-pair differences for Match 1 and Match 2. For each matching sample, the first column reports the average differences between matched pairs using no weights (i.e., all matched pairs count equally), and the second column reports weighted average differences, where better matches have a higher weight. To measure "distance" and calculate weights, we use the sum of the absolute differences for each standard normalized matching variable between the matched pairs.

***, **, and * indicate whether the corresponding matched-pair differences are statistically different from zero at the 1%, 5%, and 10% levels, respectively.

denoted by α and the error term by ε. The constant term, α, measures the average change in the bank's willingness to lend to the treatment firms, known as the average treatment effect for the treated (ATT). A positive and statistically significant α is consistent with the net empirical dominance of theories predicting an increase in the initial bank's willingness to lend the firm, implying that outside loans are on average *strategic complements* (H1). A negative and statistically significant α is instead consistent with the net empirical dominance of theories predicting a decrease in the initial bank's willingness, implying that outside loans are on average *strategic substitutes* (H2).

To further study how the ATT varies with the outside loan, we augment Equation (1) adding the size of the outside loan scaled by total assets at $t_0 - 12$, *OutsideLoan*, as an explanatory variable:

$$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = \alpha + \beta_1 OutsideLoan + \varepsilon. \qquad (2)$$

The constant term, α, measures the ATT effect when the *OutsideLoan* is zero, and β_1 measures the degree to which the ATT effect varies with the size of the outside loan. A positive (negative) and statistically significant β_1 is consistent with H1 (H2).

To test H3, we augment Equation (2) introducing an interaction between the *OutsideLoan* and a variable Z indicating whether the initial bank's claims are protected with a floating charge:

$$(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = \alpha + \beta_1 OutsideLoan + \beta_2 OutsideLoan \times Z + \beta_3 Z + \varepsilon. \qquad (3)$$

The constant term, α, measures the ATT effect when the *OutsideLoan* is zero and the initial bank loans are not protected. The coefficient β_1 measures the degree to which the ATT effect varies with the *OutsideLoan* when its loans are not protected and β_2 measures the difference in the ATT effect when the initial bank's loans are protected. Finally, β_3 measures the ATT effect when the initial bank claims are protected and the *OutsideLoan* is zero. Hence, a negative β_1, a positive β_2, and zero or statistically insignificant β_3 are consistent with H3.

Table A.1 in the appendix provides descriptive statistics on the characteristics of treatment firms in Match 2 with and without a floating charge. The firms with and without floating charge are very similar. The only statistically significant difference between them is with respect to size and interest rates: firms with a floating charge are larger and pay lower loan interest rates. With respect to other characteristics, they are younger, faster growing, with higher profitability, higher leverage ratios, and better ratings. However, none of these differences is statistically significant as the matching exercise neutralizes some of the unconditional differences between firms with and without a floating charge.[31]

For our benchmark analysis, Equations (1)–(3) are estimated at the matched treatment and control pairs using weighted least squares, putting more weights on the better matches. Standard errors are clustered at the treatment firm level.[32] In robustness checks, we confirm that our results are robust to the use of alternative matching techniques (i.e., including distance or matching errors as controls, keeping one observation per treatment firm using the closest match, matching with propensity scores).

5. Results

The results section is structured as follows. We first document the bank's average reaction after the firm obtains a loan from another bank and the degree to which the bank's reaction depends on the size of the outside loan (H1 and H2). We then examine whether the bank's response depends on whether its existing and future loans are protected with a floating charge (H3) or other collateral. Next, we try to understand which groups of theories discussed earlier may be driving the bank's reaction to the outside loan. In subsequent analysis, we also study whether the bank adjusts other margins such as interest rates and collateral. Finally, we present results of several robustness tests: reverse-causality tests, and tests for other alternative explanations, and results using alternative matching and estimation techniques.

5.1. The Bank's Average Reaction and the Size of the Outside Loan: Test of H1 and H2

Table 4 reports our main findings with respect to H1 and H2 using our two matching samples.[33] We report results for Match 1 in panel A and corresponding results for Match 2 in panel B. Column (I) reports the average change in the bank's willingness to lend when the firm gets an outside loan (i.e., the ATT from Equation (1)). Column (II) documents how the bank's response varies with the size of the outside loan (Equation (2)). The remaining columns of Table 4 report modified specifications of Equations (1) and (2) to further investigate the timing of the bank's reaction by looking at the year prior to the event window (between $t_0 - 24$ and $t_0 - 12$) and by splitting the event window in two (between $t_0 - 12$ and t_0 and between t_0 and $t_0 + 12$).

The results in columns (I) and (II) indicate that an outside loan is followed by a decrease in the initial bank's willingness to lend to the firm and that the decrease is larger, the larger the outside loan. In particular, as can be observed in column (I), the constant term in Equation (1) is negative and statistically significant, consistent with H2. The size of the estimated coefficient in panel A indicates that the treatment firm's internal limit to total assets ratio drops on average by 6.2*** percentage points more than the ratio of similar control firms, i.e., the ATT is -0.062^{***}.[34] This amounts to a drop in the average treatment firms' limit to total assets ratio of 14%. The coefficient of the *OutsideLoan* (i.e., the outside loan to total assets ratio) in column (II) is -0.412^{***}, suggesting that a bigger outside loan triggers a larger negative reaction, consistent with H2. In terms of economic significance, our estimates indicate that \$1 from another bank is associated with a drop in the initial bank's internal limit to the firm by 41 cents.[35] Similarly, a one-standard-deviation increase in the *OutsideLoan* (by 0.22) is associated with a drop in the firm's limit to total assets ratio by 0.091 (i.e., -0.412×0.22), which amounts to a drop in the average treatment firm's limit to total assets ratio by 20.7%. The intercept is small and insignificant in column (II) suggesting that the bank does not respond to small loans. The estimates in panel B using our more conservative matching sample, Match 2, are very similar to those reported in panel A. The only difference is that some of the estimated negative reactions are slightly larger (e.g., the ATT is -8.9^{***} and the coefficient of the *OutsideLoan* is -0.42^{***}).[36] Overall, regardless of the sample used,

[31] Comparing firms with and without a floating charge prior to matching reveals that firms with a floating charge are less profitable, have higher probabilities of default, and worse ratings than firms without a floating charge. This is consistent with banks requiring a floating charge from riskier borrowers. By studying high-quality firms, we essentially neutralize this channel. Results in Cerqueiro et al. (2014) indicate that there are additional differences in industry composition between the two groups of firms emanating from different asset structures across industries. In our empirical analysis, we match on industry taking care of such differences.

[32] Results are robust to also clustering with respect to both the treatment and control firms. We do not report those as our benchmark because the procedure does not allow us to use weighted least squares.

[33] For our three hypotheses we present results using both Match 1 and Match 2. To preserve space for all robustness tests, we present results using only our second more conservative matching sample, Match 2.

[34] ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

[35] The change in the treatment firm's limit at $t_0 + 12$ following a change in outside loan at t_0 is equal to β_1. This is obtained by multiplying both sides of Equation (2) with the treatment firm's total assets at t_0 and then taking the derivative with respect to the size of the outside loan. This is possible because the scaling variable, total assets at $t_0 - 12$, is not a function of the outside loan.

[36] In unreported specifications, we control for a nonlinear relationship by including a squared term of *OutsideLoan*. For both Match 1

Table 4 **The Bank's Average Reaction and the Size of the Outside Loan: Test of H1 and H2**

	Event window		A year prior		Split event window in two			
	(t_0-12, t_0+12) +/− 0.5 SD		(t_0-24, t_0-12) +/− 0.5 SD		(t_0-12, t_0) +/− 0.5 SD		(t_0, t_0+12) +/− 0.5 SD	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
				Panel A: Match 1				
Intercept	−0.062***	−0.013	−0.008	−0.020**	−0.021*	−0.015	−0.041***	0.003
	(−3.720)	(−0.675)	(−1.036)	(−2.217)	(−1.933)	(−1.262)	(−2.842)	(0.155)
OutsideLoan		−0.412***		0.095*		−0.046		−0.365***
		(−3.770)		(1.886)		(−0.454)		(−2.906)
Number of observations (matched pairs)	1,502	1,502	957	957	1,502	1,502	1,502	1,502
Number of treatment firms	290	290	193	193	290	290	290	290
				Panel B: Match 2				
Intercept	−0.089***	−0.037	0.001	0.002	−0.032	−0.022	−0.057*	−0.015
	(−2.679)	(−0.947)	(0.158)	(0.239)	(−1.644)	(−1.197)	(−1.894)	(−0.419)
OutsideLoan		−0.423**		−0.006		−0.082		−0.342**
		(−2.257)		(−0.147)		(−0.728)		(−1.995)
Number of observations (matched pairs)	302	302	194	194	302	302	302	302
Number of treatment firms	125	125	79	79	125	125	125	125

Notes. This table reports estimates from matched regressions relating the standardized response in the internal limit to a constant term (columns (I), (III), (V), and (VII)) and to *OutsideLoan* (i.e., the size of a loan at another bank divided by the firm's total assets at t_0-12; columns (II), (IV), (VI), and (VIII)). Panel A reports results for Match 1, and panel B reports results for Match 2. In columns (I) and (II), the dependent variable is the standardized change in the limit over the event window $(t_0-12,\ t_0+12)$ (i.e., $(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{treatment}} - [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{control}}$). In columns (III) and (IV), the dependent variable is redefined as the standardized change in the limit between t_0-24 and t_0-12 (i.e., the year prior to the event window). In columns (V)–(VIII), the event window is split in two using the standardized change in the limit between t_0-12 and t_0 (columns (V) and (VI)) and between t_0 and t_0+12 (columns (VII) and (VIII)). All variables are defined in Table 1 along with the list of matching variables for Match 1 and Match 2. All models are estimated using weighted least squares, weighting the observations by the inverse distance, with distance being the cumulative absolute differences for each standard normalized matching variable between the treatment and control firms. The t-statistics calculated on robust standard errors, clustered on a treatment-firm level, are reported in parentheses.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

our results suggest that outside loans are on average strategic substitutes, not complements. A 42 cents reduction in the initial bank's willingness to lend does not necessarily imply that the treatment firms' degree of indebtedness increases as their total assets, which are held constant here, may also increase. As discussed later on, the treatment firms are experiencing a growth in assets that allows them to maintain higher levels of debt without getting more indebted. We return to this in §5.5.1.

When investigating the *timing* of the bank's reaction, we find that consistent with the parallel trends assumption there is no systematic decrease in the bank's internal limit prior to the event window, particularly when using our more conservative Match 2 sample. As can be observed in columns (III) and (IV), the constant term in Equation (1) is not statistically significant, whereas the coefficient of the *OutsideLoan* in Equation (2) has a marginally significant positive sign in panel A and is economically and statistically equal to zero in panel B. We also find that most of the banks' reaction takes places in the second half of the event window.[37] As can be observed in columns (V) to (VIII) both the constant term in Equation (1) and the coefficient of the *OutsideLoan* in Equation (2) are larger in absolute size and enjoy higher statistical significance when the model is estimated over the second half of the event window, suggesting that most of the reaction takes place between t_0 and t_0+12. In fact, the *OutsideLoan* coefficients are much smaller and statistically insignificant in the first half of the event window.[38]

and 2, the square term is not statistically significant and F tests show that the inclusion of a squared term does not significantly improve the model fit, suggesting that the relationship is linear.

[37] These models are estimated using the change in the limit in the year prior to the event window (i.e., between t_0-24 and t_0-12 in columns (III) and (IV)) or the change in the limit over the two subperiods of the event window (i.e., between t_0-12 and t_0 in columns (V) and (VI) and between t_0 and t_0+12 in columns (VII) and (VIII)). All models are estimated for the same set of observations used previously in columns (I) and (II) of Table 4.

[38] In unreported robustness checks, we examine whether the initial bank's reaction in the first half of the event window is bounded by its existing loans to the firm by reestimating the model separately for firms where the outside loan brought their total bank debt above their internal limits ("above") and for firms that remained below ("below"), but found no compelling evidence that the initial bank's reaction is bounded by its existing loans to the borrower. For both groups of firms, we again find no statistically significant changes between t_0-12 and t_0.

One potential concern is that the caliper matching introduces a selection on treatment firms that could influence our estimates. To deal with this, in an unreported specification, we apply a nearest neighbor matching with an Abadie and Imbens (2011) bias correction to estimate the ATT on the full sample of treatment firms. The obtained ATT is negative and significant, -0.062^{***}, which closely coincides with estimates in Table 3. This result mitigates the concern that our estimates are influenced by a selection on the treatment firms. In the robustness section at the end of the paper, we also discuss additional robustness checks with respect to alternative matching and estimation techniques.

To further scrutinize the sensitivity of our results to unobserved heterogeneity between treatment and control firms, we calculate Rosenbaum (2002) bounds for the estimated ATT in column (I), Table 4. We find that the ATT remains negative and significant at a 10% level for gamma values up to 1.60 for Match 1 and 1.55 for Match 2. This implies that unobserved factors (beyond those controlled by our matching exercise) would have to cause the odds ratio to be more than 55% to 60% higher for the treatment relative to the control firms for our conclusions to be altered. This is a large threshold, suggesting that it is unlikely that unobserved characteristics can overturn our findings.

5.2. Protection Through a Floating Charge: Test of H3

To investigate whether contractual features such as a floating charge allow banks to protect their claims, we estimate several specifications of Equation (3). We first estimate Equation (3) using the *FloatingCharge* dummy. As explained earlier, the floating charge is a special form of collateral that carries over to future loans and thus allows the bank's existing and future loans to maintain a senior claim on firm's current and future assets. The bank's loans are secured by the assets under the floating charge, which implies that the degree with which the initial bank is protected depends on the value of the floating charge assets and their volatility. Hence, we also estimate Equation (3) using two qualifying variables regarding the floating charge value (*FloatingChargeValue*) and the volatility of their values (*FloatingChargeVolatility*) instead of a simple dummy variable.[39] We also present results with respect to fixed charge claims, *OtherCollateral*, to better understand the role of the floating charge.

Table 5 presents our findings. All specifications are estimated for both Match 1 and Match 2. Results are qualitatively very similar between them. Hence, to conserve space we mainly discuss the economic significance of the results using Match 2, our second and more conservative matching sample. As can be observed in columns (I) and (II), the bank does not react to an outside loan when its claims are protected through a floating charge. For example, in column (II) the coefficient of the *OutsideLoan* is -0.522^{***}, whereas the coefficient of the interaction term with the *FloatingCharge* is 0.601^{***}, resulting in a combined coefficient of 0.078, which is neither economically nor statistically different from zero. Consistent with H3, we also find that the coefficient of the *FloatingCharge* is close to zero and it is not statistically significant. These findings suggest that when the initial bank's claims are protected through a floating charge, the bank does not react to the outside loan. Instead, when its claims are not protected, a \$1 from another bank triggers a drop in its internal limit to the firm by 52 cents.

Next, we examine how the bank's reaction varies with the value of the floating charge assets and the volatility of their values. As can be observed in columns (III) and (IV) of Table 5, any given outside loan triggers a smaller negative reaction, the larger the value of the floating charge assets. In column (IV) the coefficient of the *OutsideLoan* is -0.519^{***}, and the coefficients of the interaction term with value is 1.059^{***}. In terms of economic significance, this estimate implies that a one-standard-deviation increase in the *FloatingChargeValue* (0.298; see Table A.1),[40] decreases the bank's response to the *OutsideLoan* by 0.32 (1.059×0.298). With respect to volatility we do not find a statistically significant effect.[41]

To further understand the role of the floating charge, we also investigate the bank's response when its claims are protected through other types of collateral (this includes fixed charge claims, pledges, and

[39] *FloatingChargeValue* is equal to the value of the floating charge assets as reported by the bank scaled by committed bank debt (i.e., outstanding debt and unused credit lines) at $t_0 - 12$. *FloatingChargeVolatility* is equal to the volatility of earnings in the three years prior to $t_0 - 12$ divided by the firm's average assets over that period. This is expected to provide a good proxy of the volatility in the floating charge assets because the floating charge assets include inventory, accounts receivable, equipment, real estate, financial assets such as cash, bank deposits, bonds, and stocks. A lot of the variation in the underlying assets is expected to come from items such as inventory, accounts receivable, cash, and bank deposits, and thus correlate with variation in the firm's earnings.

[40] In this specification, we do not employ standard normalized values for Z so that the coefficient of the *OutsideLoan* measures the effect of the outside loan on the dependent variable in the absence of a floating charge. This makes the coefficient of the *OutsideLoan* comparable across all specifications of Table 5.

[41] Given the small number of firms with a floating charge, we tested our specifications for multicollinearity using the variance inflation factor (VIF). Multicollinearity was not found to be an issue except in column IV where the VIF for *FloatingChargeVolatility* was marginally above the commonly used threshold of 5 at 5.45.

Table 5 Protection Through a Floating Charge: Test of H3

	Floating charge				Other collateral			
	Match 1 (I)	Match 2 (II)	Match 1 (III)	Match 2 (IV)	Match 1 (V)	Match 2 (VI)	Match 1 (VII)	Match 2 (VIII)
Intercept	−0.010	−0.035	−0.013	−0.037	−0.014	−0.046	−0.010	−0.042
	(−0.533)	(−0.880)	(−0.662)	(−0.928)	(−0.583)	(−0.940)	(−0.432)	(−0.840)
OutsideLoan	−0.445***	−0.522***	−0.442***	−0.519***	−0.413***	−0.361*	−0.472***	−0.495**
	(−4.061)	(−2.954)	(−4.033)	(−2.921)	(−2.713)	(−1.746)	(−3.056)	(−2.515)
OutsideLoan × *FloatingCharge*	0.524***	0.601***					0.553**	0.585***
	(2.723)	(2.801)					(2.556)	(2.618)
FloatingCharge	−0.038	0.071					−0.039	0.070
	(−0.466)	(0.715)					(−0.485)	(0.728)
OutsideLoan × *FloatingChargeValue*			0.947***	1.059***				
			(3.018)	(2.672)				
FloatingChargeValue			0.026	0.160				
			(0.191)	(1.068)				
OutsideLoan × *FloatingChargeVolatility*			0.081	−0.067				
			(0.201)	(−0.075)				
FloatingChargeVolatility			0.004	−0.000				
			(0.049)	(−0.002)				
OutsideLoan × *OtherCollateral*					−0.000	−0.249	0.055	−0.101
					(−0.002)	(−0.626)	(0.251)	(−0.254)
OtherCollateral					0.004	0.036	0.002	0.025
					(0.100)	(0.467)	(0.056)	(0.329)
Number of observations (matched pairs)	1,502	302	1,502	302	1,502	302	1,502	302
Number of treatment firms	290	125	290	125	290	125	290	125

Notes. This table reports estimation results for Equation (3). The dependent variable is the standardized change in the internal limit over the event window (i.e., $(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{treatment}} - [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{control}}$). *OutsideLoan* equals the size of the outside loan scaled by the firm's *TotalAssets* at $t_0 - 12$. *FloatingCharge* is a dummy variable indicating whether the initial bank's loans to the firm are secured by a floating charge. *FloatingChargeValue* indicates the value of the floating charge assets over the committed amount. *FloatingChargeVolatility* indicates the average volatility in the firm's earnings over the three prior years scaled by the firm's average assets over the same period. *OtherCollateral* is a dummy variable that equals one when the bank's existing debt is secured with other types of collateral whose value relative to the outstanding loan is greater or equal to 80%, and it is equal to zero otherwise. All variables are defined in Table 1 along with the list of matching variables for Match 1 and Match 2. All models are estimated using weighted least squares, weighting the observations by the inverse distance, with distance being the cumulative absolute differences for each standard normalized matching variable between the treatment and control firms. The t-statistics calculated on robust standard errors, clustered on a treatment-firm level, are reported in parentheses.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

liens). Our indicator, *OtherCollateral*, is a dummy variable that equals one when the bank's existing debt is only secured with other types of collateral whose value relative to the outstanding loan is greater or equal to 80%, and it is equal to zero otherwise.[42] Everything else equal, these other types of collateral should be less effective because they do not necessarily allow the bank's future loans to retain seniority over outside loans. They could, nevertheless, help mitigate some of the externalities if the fear of losing the pledged assets mitigates the increased moral hazard associated with the higher levels of debt.

Results presented in columns (V) and (VI) of Table 5 suggest that this is not the case. In column (VI), for example, the coefficient of the *OutsideLoan* is −0.413**, whereas the coefficient of the interaction term is 0. Including the *FloatingCharge* and *OtherCollateral* variables in the same specification yields similar results. In particular, in column (VIII) the coefficients of *OutsideLoan* and *OutsideLoan* × *FloatingCharge* are −0.495** and 0.585***, whereas the coefficient of *OutsideLoan* × *OtherCollateral* is −0.101, suggesting that the presence of a floating charge mitigates coordination problems and other negative externalities from outside loans, whereas other collateral does not. All in all, these findings suggest that the explanatory power of the floating charge on a bank's willingness to lend to a firm may rest on its ability to protect not only the bank's current but also future loans.

5.3. Debt Capacity vs. Coordination Problems and Other Negative Externalities

So far our results suggest that outside loans are, on average, strategic substitutes rather than

[42] Similar results are found using different thresholds (e.g., 75% and 85%).

complements: a loan from another bank triggers a decrease and not an increase in the initial bank's willingness to lend to the firm. However, it is unclear whether this is only driven by a simple "debt capacity story" (i.e., the initial bank is adjusting its willingness to lend to the firm reflecting the fact that some of the firm's debt capacity is now absorbed by another bank) or whether outside loans carry additional coordination problems and other negative externalities as outlined in the theoretical literature.

In this section we develop a test to examine whether the initial bank's reaction goes beyond a simple debt capacity story. To do that, we benchmark changes in the treatment firms' internal limits to a subsample of control firms that at the time of the outside loan, received an inside loan and got indebted to a similar degree when neither the inside nor the outside loan brought the two firms' total bank debt above their internal limits with the initial bank.[43] As discussed earlier, the treatment firms are experiencing a growth in assets that allows them to maintain higher levels of debt without getting more indebted. Using a subsample of similar control firms with positive demand for loans and similar ex post indebtedness may thus provide a better counterfactual if firms have limited debt capacity and debt levels matter (i.e., it provides an approximation of what their limit would have been if they were getting indebted to a similar degree, but without breaking an exclusive relationship). Requiring that both firms remain below their initial internal limits with their initial bank reduces selection concerns that their initial bank may have never allowed the treatment firms to get indebted above the amount indicated by their initial limits. We want to abstract from cases where the initial bank's reaction may be simply driven by firms crossing that threshold (i.e., getting indebted more than what their initial bank thought they could support).

Relative to the control group, a simple debt capacity story predicts that \$1 from another bank should trigger a decrease in the initial bank's willingness to lend to the firm by \$1 (i.e., inside and outside loans are perfect substitutes). A larger than a one-to-one negative reaction instead would suggest that channels related to coordination problems and other negative externalities from outside loans matter for the bank's response. Although these two sets of theories have different predictions, the interpretation of the estimated *OutsideLoan* coefficient is complicated by the fact that it measures the net effect of all operative channels. This test can thus be informative of whether theories emphasizing the role of coordination problems and other negative externalities are at work if the estimated coefficient of the *OutsideLoan* is smaller than -1. Any other result cannot confirm or reject these theories.[44]

Results in Table 6 indicate a larger than a one-to-one reaction. Columns (I) and (II) present estimates of Equations (1) and (2) for the subsample of the Match 2 firms that satisfy the additional conditions outlined above. As can be observed in column (I), the ATT effect for this subsample is -0.077^{***} and the estimated coefficient of the *OutsideLoan* in column (II) is -1.591^{***}.[45] The coefficient of the *OutsideLoan* is statistically different from -1 at the 5% level suggesting a larger than one-to-one negative reaction. Overall, these results indicate that the estimated bank reaction goes beyond a simple debt capacity story because one dollar from another bank is not equivalent to one dollar from the inside bank, but more, suggesting that outside loans carry additional coordination problems and negative externalities.

5.4. Adjusting Other Margins

Our empirical analysis so far was geared to study how a bank's maximum willingness to lend changes when a firm becomes nonexclusive. We now study whether the bank responds by adjusting other margins as well. For example, in addition to reducing credit supply, the initial bank may further protect itself by requiring higher interest rates or increasing collateral requirements on existing and subsequent loans to the firm. To investigate this possibility, we study changes in interest rates or collateral requirements on the firm's outstanding loans with the bank

[43] Specifically we require that (1) the matched control firm got a loan from the initial bank between t_0-12 and t_0 like the treatment firm, (2) at t_0+12 the matched treatment and control firms have similar ratios of total bank debt, and (3) the firms outstanding debt with the initial bank at t_0-12 plus the outside loan are lower than the initial bank's internal limit to the firm at t_0-12. To conserve observations, for this exercise we use a one-standard-deviation calibre instead of 0.5, which is used in all other exercises. Matching on indebtedness instead of just the size of the inside loan, for example, allows us to better control for possible repayments at the initial bank. Results in §5.5.1 indicate that loans from the initial bank may temporarily overlap with loans from the new bank.

[44] A coefficient that is larger than -1 does not necessarily imply that theories involving coordination problems and other negative externalities are not at work. It could be that other theories that predict a positive reaction are also at work reducing the overall average reaction. The only result that can be informative of whether theories involving coordination problems and other negative externalities are at work is a coefficient that is smaller than -1.

[45] In unreported descriptive statistics similar to those reported in Table 2, we confirm that the treatment and control firms in this subsample are similar to each other and satisfy the parallel trends assumption as the Match 2 sample. For additional robustness, we also estimated similar specifications using subsamples of Match 2 where we only match on ex post indebtedness using a 0.5-standard-deviation radius. We get similar results with an estimated coefficient for *OutsideLoan* equal to -1.281^{***} pointing to a larger than a one-to-one reaction to an outside loan.

Table 6 Debt Capacity vs. Coordination Problems and Other Negative Externalities

	(I)	(II)
Intercept	−0.077***	−0.017
	(−3.080)	(−0.679)
OutsideLoan		−1.591***
		(−6.191)
Number of observations (matched pairs)	140	140
Number of treatment firms	67	67

Notes. This table reports estimation results for Equations (1) and (2). The dependent variable is the standardized change in the internal limit over the event window (i.e., $(\Delta Limit_{t_0+12,\,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{treatment}} - [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{control}}$). *OutsideLoan* equals the size of the outside loan scaled by the firm's *TotalAssets* at t_0-12. We take a subsample of control firms of Match 2 that received during the event window an inside loan. Equations (1) and (2) are estimated using a subsample of Match 2 for which (1) the matched control firm got a loan from the initial bank between t_0-12 and t_0 like the treatment firm, and (2) at t_0+12 the matched treatment and control firms have similar ratios of total bank debt. To conserve observations, we use a one-standard-deviation calibre for this test instead of 0.5, which is used in all other tests. All other variables are defined in Table 1 along with the list of matching variables for Match 2. All models are estimated using weighted least squares, weighting the observations by the inverse distance, with distance being the cumulative absolute differences for each standard normalized matching variable between the treatment and control firms. The *t*-statistics calculated on robust standard errors, clustered on a treatment-firm level, are reported in parentheses.

*** indicates significance at the 1% level.

between t_0-12 and t_0+12 by replacing the dependent variable in Equations (1) and (2) with

$$(\Delta InterestRate_{t_0+12,\,t_0-12})_{\text{treatment, control}} = [InterestRate_{t_0+12} - InterestRate_{t_0-12}]_{\text{treatment}} - [InterestRate_{t_0+12} - InterestRate_{t_0-12}]_{\text{control}}$$

$$(\Delta Collateral_{t_0+12,\,t_0-12})_{\text{treatment, control}} = [Collateral_{t_0+12} - Collateral_{t_0-12}]_{\text{treatment}} - [Collateral_{t_0+12} - Collateral_{t_0-12}]_{\text{control}},$$

where $InterestRate_{t_0-12}$ ($Collateral_{t_0-12}$) and $Interest\,Rate_{t_0+12}$ ($Collateral_{t_0+12}$) indicate the average interest rates (collateral values scaled by the loan amount at origination) on the firm's outstanding loans with the bank at t_0-12 and t_0+12, respectively. Using similarly constructed variables, we also study changes on the firm's existing loans with the bank (i.e., loans existing at t_0-12 that are still outstanding at t_0+12).[46]

Results are reported in Table 7. Panel A reports results for all outstanding loans, and panel B reports results for existing loans. We find that interest rates

[46] We do not consider "new loans" (loans originated after t_0 and prior to t_0+12) as a separate category as we have too few cases where both the treatment and control firms obtain a new loan from our bank for meaningful analysis.

Table 7 Adjusting Other Margins

	Interest rates		Collateral	
	(I)	(II)	(III)	(IV)
Panel A: All loans				
Intercept	0.141	−0.015	0.077*	0.049
	(0.869)	(−0.077)	(1.950)	(1.066)
OutsideLoan		1.434		0.240
		(1.472)		(1.203)
Number of observations (matched pairs)	280	280	282	282
Number of treatment firms	113	113	115	115
Panel B: Existing loans				
Intercept	0.071	−0.023	0.089**	0.065
	(0.447)	(−0.125)	(2.291)	(1.437)
OutsideLoan		0.864		0.206
		(0.932)		(1.049)
Number of obs (matched pairs)	275	275	277	277
Number of treatment firms	113	113	115	115

Notes. This table reports estimation results for Equations (1) and (2), with as dependent variable, for columns (I) and (II), $(\Delta InterestRate_{t_0+12,\,t_0-12})_{\text{treatment, control}} = [InterestRate_{t_0+12} - InterestRate_{t_0-12}]_{\text{treatment}} - [InterestRate_{t_0+12} - InterestRate_{t_0-12}]_{\text{control}}$, and for columns (III) and (IV), $(\Delta Collateral_{t_0+12,\,t_0-12})_{\text{treatment, control}} = [Collateral_{t_0+12} - Collateral_{t_0-12}]_{\text{treatment}} - [Collateral_{t_0+12} - Collateral_{t_0-12}]_{\text{control}}$. *OutsideLoan* equals the size of the outside loan scaled by the firm's *TotalAssets* at t_0-12. Results are reported for Match 2. Panel A presents results for "all loans," i.e., all loans outstanding with our bank in the beginning and end of the event window. Panel B presents the results for "existing loans," i.e., loans that were outstanding in the beginning of the event window and that did not mature before the end of the event window. All other variables are defined in Table 1 along with the list of matching variables for Match 2. All models are estimated using weighted least squares, weighting the observations by the inverse distance, with distance being the cumulative absolute differences for each standard normalized matching variable between the treatment and control firms. The *t*-statistics calculated on robust standard errors, clustered on a treatment-firm level, are reported in parentheses.

** and * indicate significance at the 5% and 10% levels, respectively.

on all outstanding or existing loans increase more for the treatment firms than similar control firms, but this relative increase is not statistically significant. Intensified competition between the initial and the new bank may be putting a downward pressure on interest rates. Ioannidou and Ongena (2010), for example, find that right after firms turn to another bank their immediately subsequent loans from the new bank carry even lower interest rates than the first loans, suggesting that competition between the initial and the new bank may be intensifying when a firm initiates a new lending relationship. With respect to collateral, we find that for all outstanding loans, as well as for existing loans, the collateralization rates for the treatment firms increase more than for the control firms by 7.7* to 8.1** percentage points (compared to an average increase of 3 percentage points for the control firms). This increase seems to be independent of the size of the outside loan.

In sum, these results suggest that a bank reacts to the negative externalities and coordination issues between lenders not only by adjusting its credit supply, but also by requiring more collateral.

5.5. Robustness Checks

5.5.1. Reverse Causality. The results presented in Table 4 on the timing of the banks reaction suggest that it is unlikely that our findings are driven by a prior deterioration in quality. It is possible, however, that the firm's deterioration in quality is still unobserved at $t_0 - 12$ and thus not captured by our matching variables, but materializes later and triggers a contraction in the firm's internal limit. Additional results presented below suggest that this is rather unlikely. In particular, if the reverse-causality story outlined above drives our results in Table 4, we should observe that the bank's reaction is larger or primarily present for firms whose quality deteriorates in the year or so after the outside loan (between t_0 and $t_0 + 12$). We do not find this to be the case. In particular, using firm profitability and probability of default as measures of credit quality, we split the treatment firms in Match 2 in two groups: firms whose quality in the year after the outside loan was either stable or improving and firms whose quality was deteriorating, and we reestimate our model for these subsamples. As can be observed in Table 8, we find that reactions to the outside loan are primarily driven by firms whose quality was either stable or improving. These are also the overwhelming majority of the treatment firms in our matched sample. This is also consistent with evidence in Ioannidou and Ongena (2010) who find that most firms who manage to obtain loans from an outside bank are high-quality firms. Overall, these results are not consistent with the reverse-causality story outlined above.

In additional results reported in Table A.2 of the appendix, we investigate this further by following firms over time. Studying how the treatment firms developed relative to the control firms in the year or so after the outside loan could be informative as to what may be driving these firms to turn to another bank. If deteriorating quality is driving firms to turn to another bank, we should observe that treatment firms fair worse than the control firms in terms of size and quality. If instead firms are turning to another bank for other reasons, we should *not* observe that treatment firms fair worse than their matched control firms. As can be observed in Table A.2, we find that treatment firms increase in size and they increase more than their matched control firms. This growth takes place between $t_0 - 12$ and t_0. When looking at their credit quality, we observe that it is either stable or slightly improving (i.e., there is no change in their profitability over the event window and their probability of default is slightly lower albeit to a lower degree than their matched control firms). There is a temporary increase in their leverage ratios in the year of the outside loan, but this does not persist till the end of the event window as their debt at the initial bank decreases in the following year. Over the entire event window the treatment firms total bank debt increases by 5.4%, though not significantly so. Their bank debt to total assets ratio, however, remained fairly constant given the increase in their total assets.[47] Both demand and supply driven explanations could be driving this result. For example, the treatment firms may be turning to another bank to refinance some of their existing loans and the two overlap temporarily. However, it is also possible that concerned with coordination problems and other negative externalities arising from the higher indebtedness, the initial bank is decreasing its lending to these firms.

Nevertheless, these patterns are not consistent with the reverse-causality story outlined above. If deteriorating quality was driving the treatment firms to turn to another bank and the initial bank's decrease in their internal limit, we would observe that these firms' quality deteriorates significantly after the outside loan, but they do not. The treatment firms retained after matching are high-quality, growing firms that may be turning to other banks to finance some of their existing or new projects.

5.5.2. Lower Anticipated Demand. To investigate whether the observed decreases in the internal limits are driven by lower *anticipated demand* from the firm, we reestimate our benchmark models for the subsample of firms that are more likely to be "adding" rather than "switching" away. Everything else equal, drops in anticipated demand for loans should be larger for firms switching away rather than firms adding a relationship. If the observed decreases in the limits are driven by lower anticipated demand from such firms, dropping the switching firms from the sample should lead to smaller estimated reactions.

To distinguish between "switchers" and "adders," we classify as adders any treatment firm that continues to have a lending relationship with the initial bank till the end of the event window ("Adders 1").

[47] The debt ratios in Table A.2 are scaled by total assets at $t_0 - 12$ as to be able to observe the changes in the firm's debt (and its composition) over the relevant period, holding constant contemporaneous changes in the firm's total assets. This information together with the firms' average debt ratios at $t_0 - 12$ reported in Table 2 allow to further calculate the treatment and control firms' debt ratios at t_0 and at $t_0 + 12$ (i.e., allowing both numerators and denominators to change). Over the entire event window, the treatment firms' bank debt to total assets ratios increased by 1.8 percentage points. The corresponding figure for the control firms is 0.4 percentage points. Both changes are not statistically different from zero.

Table 8 Reverse Causality

	Profitability				Default probability			
	Stable or improving		Deteriorating		Stable or improving		Deteriorating	
	(I)	(II))	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Intercept	−0.077*	−0.024	−0.122**	−0.074	−0.116***	−0.067**	0.017	0.034
	(−1.920)	(−0.496)	(−2.102)	(−1.149)	(−3.985)	(−2.393)	(0.145)	(0.253)
OutsideLoan		−0.480**		−0.315		−0.350***		−0.100
		(−2.266)		(−0.933)		(−5.920)		(−0.623)
Number of observations (matched pairs)	225	225	77	77	243	243	59	59
Number of treatment firms	92	92	33	33	100	100	25	25

Notes. This table reports estimation results for Equations (1) and (2) for Match 2. In all cases, the dependent variable is the standardized change in the internal limit over the event window (i.e., $(\Delta Limit_{t_0+12,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{treatment}} - [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{control}}$). *OutsideLoan* indicates the size of the outside loan scaled by the firm's *TotalAssets* at t_0*-12*. The left (right) panel presents results where we split the sample according to changes in profitability (default probability) over the event window. Columns (I) and (II) ((V) and (VI)) focus on treatment firms with stable or improving profitability (default probability). Columns (III) and (IV) ((VII) and (VIII)) present results for treatment firms with deteriorating profitability (default probability). All variables are defined in Table 1 along with the list of matching variables for Match 2. All models are estimated using weighted least squares, weighting the observations by the inverse distance, weighting the observations by the inverse distance, with distance being the cumulative absolute differences for each standard normalized matching variable between the treatment and control firms, clustered on a treatment-firm level, are reported in parentheses.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

All other firms are classified as switchers. Using this definition, 113 of the 125 treatment firms in Match 2 are classified as adders. As can be observed in columns (I) and (II) of Table 9, reestimating Equations (1) and (2) using Adders 1 yields results similar to those presented in Table4. If anything, dropping the small number of switching firms from the sample, gives a larger estimated effect (i.e., the *OutsideLoan* coefficient is −0.487*** instead of −0.423***). One potential concern with the definition of adders above is that it relies on ex post information, making the assumption that the initial bank could already foresee which firms intended to switch and which ones were adding a relationship. This assumption may not necessarily hold. Moreover, ex post variables may also be affected by the way the bank reacted to the outside loan. However, to the extent that the termination of a relationship is due to a large contraction in their internal limits, selecting on the treatment firms that remain with the bank should cut against us, resulting in smaller estimated negative reactions to an outside. Hence, we also reestimate our specifications using a more conservative definition of adders that relies more on ex ante rather than ex post information. Our second definitions of adders consist only of firms that during the event window got a new loan from the initial bank ("Adders 2"). Only 26 out of the 125 treatment firms are classified as adders in this case. Results presented in columns (III) and (IV) of Table 5 confirm our previous findings and yield an even larger estimated negative reactions for this subsample, suggesting that lower anticipated demand is unlikely to be driving the bank's estimated reaction to the outside loan.[48]

5.5.3. Alternative Matching and Estimation Techniques. Finally, we also investigate the robustness of our findings in Table 4 to different matching and estimation choices in addition to those discussed earlier. Results are presented in Table A.3 of the appendix, indicating that our estimates are robust to many commonly used alternative matching techniques (i.e., including distance and matching errors as controls, keeping one observation per treatment firm using the closest match, matching using a propensity score instead of individual variables).

[48] In unreported robustness checks, we also investigate whether the bank's reaction is partly driven by credit constraints at the bank or regional level (using unused credit capacity and the crisis period), concerns about too large exposures to the firm (using outstanding loans to a firm to total loans), positive signals and reduced hold-up possibilities (using firm and relationship characteristics such as firm size, age, relationship length, and "all-in costs" on outstanding loans), but found no support. Dropping the crisis period from the sample yields very similar results. Measures of unused limit capacity, bank exposure to the firm, firm size, age, relationship length, and "all-in costs" introduced in specifications similar to Equation (3) were not found to systematically correlate with the bank's reaction neither in levels nor when interacted with the *OutsideLoan*, except for relationship length that had a negative and statistically significant coefficient in one of the specifications. However, even in that case the estimated coefficient of *OutsideLoan* was materially unchanged at −0.424***. Similar results are obtained if we instead additionally match on relationship length. These results are available upon request.

Table 9 **Lower Anticipated Demand**

	Adders 1		Adders 2	
	(I)	(II)	(III)	(IV)
Intercept	−0.076**	−0.023	−0.034	0.096*
	(−2.174)	(−0.566)	(−0.512)	(2.027)
OutsideLoan		−0.487**		−1.027***
		(−2.096)		(−5.404)
Number of observations (matched pairs)	279	279	85	85
Number of treatment firms	113	113	26	26

Notes. This table reports estimation results for Equations (1) and (2). In all cases, the dependent variable is the standardized change in the internal limit over the event window (i.e., $(\Delta Limit_{t_0+12,\, t0-12}/Assets_{t_0-12})_{\text{treatment, control}} = [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{treatment}} - [(Limit_{t_0+12} - Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{control}}$). *OutsideLoan* indicates the size of the outside loan scaled by the firm's *TotalAssets* at $t_0 - 12$. Starting from Match 2, Adders 1 requires that treatment firms continue to have a lending relationship with the initial bank until the end of the event window. Adders 2 requires that treatment firms got a new loan from the initial bank during $t_0 - 12$ and t_0. All models are estimated using weighted least squares, weighting the observations by the inverse distance, with distance being the cumulative absolute differences for each standard normalized matching variable between the treatment and control firms. The t-statistics calculated on robust standard errors, clustered on a treatment-firm level, are reported in parentheses.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

6. Conclusions

We study how a bank's willingness to lend to a previously exclusive firm changes once the firm obtains an outside loan from another bank and how this varies with the size of the outside loan and the degree to which the bank may be insulated from coordination problems and other negative externalities from such loans. The answer to this question is theoretically unclear as different theories and mechanisms pull in different directions. Theories emphasizing the role of complementarities in lending indicate that the initial bank's willingness to lend may increase. Other theories emphasizing firm's limited debt capacity and negative externalities from outside loans predict a decrease.

Overall, we find that loans from other banks are strategic substitutes, i.e., the initial bank wants to lend less when other banks are lending to a firm. Several robustness tests suggest that this result is unlikely to be driven by reverse causality. The vast majority of the treatment firms are high-quality, growing firms that may be turning to other banks to finance some of their existing or new projects. As indicated by their control counterparts they should have been experiencing an increase in their internal limits with the bank, but they experience a decrease instead. Consistent with concerns about coordination problems and higher indebtedness, the decrease in the initial bank's willingness to lend is more pronounced the larger the outside loan and it is muted if the initial bank is insulated from coordination and other negative externalities from higher indebtedness. Additional tests holding debt levels constant between treatment and control firms indicate that a dollar from the initial bank is not equivalent to a dollar from another bank, suggesting that the bank's reaction may go beyond a simple debt capacity story to a story that involves coordination problems and other negative externalities.

Overall, our findings suggest that firms' limited debt capacity and negative externalities from outside loans may act as a stabilizing force in credit markets limiting positive comovements between lenders. They also give a benevolent role to transparency allowing banks to mitigate risk exposures. Although our analysis focuses on credit markets, the insights drawn extend to other markets such as the insurance and credit default swaps markets, where coordination problems and the externalities resulting from the nonexclusivity of financial contracts have played a pivotal role in the global financial crisis of 2007–2009 (see, e.g., Acharya and Bisin 2014). The collapse of AIG and Lehman Brothers has only highlighted the pressing need for an improved institutional framework with augmented transparency that could help the involved parties to better evaluate and internalize the externalities. Consistent with the theoretical literature, our findings suggest that that coordination problems from nonexclusivity and the negative externalities stemming from firms' limited debt capacity are a concern for lenders and undermine their willingness to lend. Our results also highlight that information on counterparty exposures combined with contractual features, such as general collateral that extends to future exposures, could mitigate coordination problems and externalities from counterparty risk.

Acknowledgments

The authors thank two anonymous referees, an anonymous associate editor, Viral Acharya, Andrea Attar, Bo Becker, Alberto Bennardo, Bruno Biais, Dion Bongaerts, Fabio Braggion, Catherine Casamatta, Geraldo Cerqueiro, Peter DeMarzo, Sonia Falconieri, Carlo Favero, Mark Flannery, Itay Goldstein (department editor), Andrey Golubov, Abe de Jong, Niklas Körling, Jose Liberti, Laurent Linnemer, Lars Norden, Steven Ongena, Marco Pagano, Christine Parlour, Mitchell Petersen, Kasper Roszbach, Francesco Saita, Bogdan Stacescu, Philip Strahan, Greg Udell, and Hannes Wagner; and conference and seminar participants at the 2013 Banque de France Conference on Risk Taking in Financial Institutions, Regulation and the Real Economy, 2012 Financial Intermediation Research Society Meetings (Minneapolis), 2012 CESIfo Applied Micro Conference (Munich), 2012 American Finance Association

Meetings (Chicago), 2012 HIT-TDB-RIETI workshop on the "Economics of Interfirm Networks" (Tokyo), 2011 European Finance Association Meetings (Stockholm), 2011 CEPR-ESSFM Meeting (Gerzensee), Norges Bank Workshop on Financial Intermediation (Oslo), 3L Finance Workshop (Brussels), Aarhus University, Banco de Portugal, Bocconi University, Cass Business School, De Nederlandsche Bank, European Central Bank, ESMT-Humboldt, Federal Reserve Bank of Philadelphia, Goethe University Frankfurt, Lancaster University, RSM Rotterdam, Tilburg University, Toulouse School of Economics, University of Cyprus, University of Maynooth, University of Naples Federico II, and Goethe University Frankfurt for helpful comments. The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the executive board of the Sveriges Riksbank. The first two authors acknowledge financial support from FWO-Flanders [Contract G.0719.13].

Appendix

Table A.1 Characteristics of Firms With and Without a Floating Charge

	Treatment firms			Floating charge			No floating charge		
Variables	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
I. Firm characteristics									
Public									
Age	22.072	17.000	15.591	15.286	12.000	9.810	22.475	17.000	15.804
TotalAssets	12,400	3,826	30,769	12,798	9,674	10,388	12,376	3,724*	31,588
AssetGrowth	1.037	1.028	0.124	1.067	1.070	0.076	1.035	1.028	0.126
TangibleAssets	0.859	0.892	0.120	0.808	0.812	0.131	0.862	0.896	0.119
Profitability	0.054	0.050	0.065	0.069	0.072	0.074	0.053	0.050	0.064
Leverage	0.532	0.512	0.189	0.576	0.539	0.192	0.530	0.509	0.190
BankDebt	0.354	0.325	0.222	0.385	0.169	0.334	0.352	0.328	0.215
DefaultProbability	1.539	1.000	2.053	1.243	1.200	0.885	1.557	1.000	2.103
ExternalRating	3.384	3.000	0.781	3.571	3.000	0.787	3.373	3.000	0.782
RepaymentProblems	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Private									
InternalLimit	0.457	0.440	0.190	0.468	0.319	0.288	0.457	0.442	0.184
DistancetoLimit	0.068	0.034	0.080	0.071	0.020	0.113	0.068	0.035	0.078
LoanInterestRate	6.292	6.300	1.636	5.070	5.150	0.804	6.364	6.495**	1.646
II. Outside loan									
OutsideLoan	0.133	0.051	0.232	0.254	0.075	0.458	0.125	0.049	0.213
III. Limit changes									
$\Delta Limit_{t_0-24,t_0-12}/Assets_{t_0-24}$	−0.012	−0.015	0.067	−0.031	−0.044	0.027	−0.010	−0.014	0.070
$\Delta Limit_{t_0+12,t_0-12}/Assets_{t_0-12}$	−0.041	−0.030	0.301	−0.005	−0.003	0.358	−0.043	−0.031	0.299
IV. Collateral information									
FloatingCharge	0.056	0.000	0.231	1.000	1.000	0.000	0.000	0.000	0.000
FloatingChargeValue	0.630	0.687	0.256	0.630	0.687	0.256	—	—	—
FloatingChargeVolatility	0.042	0.037	0.022	0.042	0.037	0.022	—	—	—
OtherCollateral	0.224	0.000	0.419	0.143	0.000	0.378	0.229	0.000	0.422

Notes. This table reports the mean, median, and standard deviation of the characteristics of treatment firms, treatment firms with floating charge, and treatment firms without floating charge in Match 2. Variable definitions are in Table 1. Differences in means are assessed using the student's t-test. Differences in medians are assessed using the Wilcoxon-Mann-Whitney test for continuous variables and the Pearson's chi-square test for categorical variables.

** and * reported next to the mean and median values of the no floating charge group indicate whether the corresponding values are statistically different relative to the floating charge group at the 5% and 10% levels, respectively.

Table A.2 Reverse Causality: Following Firms Over Time

		Treatment			Control			
Firm characteristics		Mean	SD	Median	Mean	SD	Median	Pair diff.
		Panel A: Changes between t_0-12 and t_0+12						
TotalAssets	$\text{Ln}[(Assets_{t_0+12}/Assets_{t_0-12})]$	0.097***	0.268	0.027	0.040*	0.338	0.007	0.035
DefaultProbability	$PD_{t_0+12} - PD_{t_0-12}$	−0.003**	0.017	−0.002	−0.006***	0.012	−0.004	0.302*
Profitability	$[(EBITDA_{t_0+12} - EBITDA_{t_0-12})/Assets_{t_0-12}]$	0.009	0.069	0.001	0.009	0.110	0.000	−0.007
Leverage	$(TotalDebt_{t_0+12} - TotalDebt_{t_0-12})/Assets_{t_0-12}$	0.023	0.441	−0.016	0.020	0.330	−0.028	−0.035
BankDebt	$(BankDebt_{t_0+12} - BankDebt_{t_0-12})/Assets_{t_0-12}$	0.054	0.375	−0.012	0.021	0.275	−0.025	−0.009
DebtatInitialBank	$(InsideLoans_{t_0+12} - InsideLoans_{t_0-12})/Assets_{t_0-12}$	−0.024	0.351	−0.036	0.021	0.275	−0.024	−0.100***

Table A.2 (Continued)

		Treatment			Control			
Firm characteristics		Mean	SD	Median	Mean	SD	Median	Pair diff.
		Panel B: Changes between t_0-12 and t_0						
TotalAssets	$\text{Ln}[(Assets_{t_0}/Assets_{t_0-12})]$	0.085***	0.222	0.043	0.027**	0.225	0.001	0.067**
DefaultProbability	$PD_{t_0}-PD_{t_0-12}$	0.001	0.016	−0.001	−0.003***	0.010	−0.002	0.409**
Profitability	$[(EBITDA_{t_0}-EBITDA_{t_0-12})/Assets_{t_0-12}]$	0.040***	0.076	0.043	0.042***	0.099	0.037	−0.002
Leverage	$(TotalDebt_{t_0}-TotalDebt_{t_0-12})/Assets_{t_0-12}$	0.101***	0.292	0.033	−0.001	0.232	−0.019	0.114***
BankDebt	$(BankDebt_{t_0}-BankDebt_{t_0-12})/Assets_{t_0-12}$	0.090***	0.267	0.000	0.002	0.171	−0.016	0.078***
DebtatInitialBank	$(InsideLoans_{t_0}-InsideLoans_{t_0-12})/Assets_{t_0-12}$	−0.001	0.231	−0.023	0.003	0.170	−0.015	−0.034*
		Panel C: Changes between t_0 and t_0+12						
TotalAssets	$\text{Ln}[(Assets_{t_0+12}/Assets_{t_0})]$	0.013	0.203	−0.011	0.012	0.260	−0.006	−0.032
DefaultProbability	$PD_{t_0+12}-PD_{t_0}$	−0.001	0.021	−0.001	−0.006***	0.012	−0.004	0.184
Profitability	$[(EBITDA_{t_0+12}-EBITDA_{t_0})/Assets_{t_0}]$	−0.025***	0.076	−0.036	−0.028	0.125	−0.038	−0.006
Leverage	$(TotalDebt_{t_0+12}-TotalDebt_{t_0})/Assets_{t_0}$	−0.070**	0.343	−0.044	0.023	0.242	−0.020	−0.152***
BankDebt	$(BankDebt_{t_0+12}-BankDebt_{t_0})/Assets_{t_0}$	−0.032	0.278	−0.027	0.016	0.196	−0.014	−0.086**
DebtatInitialBank	$(InsideLoans_{t_0+12}-InsideLoans_{t_0})/Assets_{t_0}$	−0.023	0.276	−0.024	0.019	0.213	−0.016	−0.065**

Notes. This table reports how key characteristics of treatment and control firms in Match 2 have evolved over the event window. Panel A displays the evolution over the entire event window, whereas Panels B and C report the first and second year of the event window, respectively. The table reports the mean, standard deviation, and median. The column next to the mean values reports whether the mean is different from zero. The last two columns report the differences between the matched pairs and whether they are statistically different from zero (pair diff.). All variables are defined in Table 1.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.3 Additional Robustness Checks: Alternative Estimation Choices

	Distance		Matching errors		Closest match		Propensity score	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
	Panel A: Match 1							
Intercept	−0.047	−0.005	−0.059***	−0.013	−0.053***	−0.016	−0.070***	−0.011
	(−1.152)	(−0.116)	(−3.577)	(−0.681)	(−3.133)	(−0.881)	(−3.228)	(−0.415)
OutsideLoan		−0.411***		−0.395***		−0.287***		−0.529***
		(−3.733)		(−3.703)		(−2.787)		(−3.481)
Number of observations (matched pairs)	1,502	1,502	1,502	1,502	290	290	860	860
Number of treatment firms	290	290	290	290	290	290	215	215
	Panel B: Match 2							
Intercept	−0.074	−0.006	−0.076**	−0.015	−0.084***	−0.048	−0.090**	−0.032
	(−0.997)	(−0.083)	(−2.091)	(−0.356)	(−3.180)	(−1.554)	(−2.457)	(−0.732)
OutsideLoan		−0.427**		−0.497**		−0.272*		−0.493**
		(−2.276)		(−2.476)		(−1.780)		(−2.157)
Number of observations (matched pairs)	302	302	302	302	125	125	235	235
Number of treatment firms	125	125	125	125	125	125	112	112

Notes. This table reports estimation results for Equations (1) and (2). The dependent variable is the standardized change in the internal limit over the event window (i.e., $(\Delta Limit_{t_0+12,t_0-12}/Assets_{t_0-12})_{\text{treatment, control}} = [(Limit_{t_0+12}-Limit_{t_0-12})/TotalAssets_{t_0-12}]_{\text{treatment}} - [(Limit_{t_0+12}-Limit_{t0-12})/TotalAssets_{t_0-12}]_{\text{control}}$). *OutsideLoan* equals the size of the outside loan scaled by the firm's *TotalAssets* at t_0-12. Panel A reports weighted least square estimates using Match 1 and Panel B reports corresponding specifications for Match 2. Columns (I) and (II) report results for Equations (1) and (2), respectively, after including a measure of the "distance" between each pair by summing their absolute differences with respect to each standard normalized matching variable as a control variable. Columns (III) and (IV) report results for Equations (1) and (2), respectively, after including as control variables the differences between matched treatment and control firms with respect to each matching variable. Columns (V) and (VI) report estimates for Equations (1) and (2) using one observation per match treatment firm by using the closest match, i.e., the pair with the lowest cumulative absolute differences between the treatment and control firms. Columns (VII) and (VIII) report estimates for Equations (1) and (2) using the subsamples of matched pairs in Match 1 and Match 2 for which the difference in the propensity to become nonexclusive is smaller than one percentage point. All variables are defined in Table 1 along with the list of matching variables for Match 1 and Match 2. The t-statistics calculated on robust standard errors, clustered on a treatment-firm level, are reported in parentheses except for columns (I) and (II) where there is double clustering.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

References

Abadie A, Imbens GW (2011) Bias-corrected matching estimators for average treatment effects. *J. Bus. Econom. Statist.* 29(1):1–11.

Acharya V, Bisin A (2014) Counterparty risk externality: Centralized versus over-the-counter markets. *J. Econom. Theory* 149:153–182.

Attar A, Mariotti T, Salanié F (2011) Nonexclusive competition in the market for lemons. *Econometrica* 79(6):1869–1918.

Attar A, Casamatta C, Chassagnon A, Decamps JP (2010) Multiple lenders, strategic default and the role of debt covenants. Working paper, Université de Toulouse, Toulouse, France.

Ayotte K, Skeel DA Jr (2013) Bankruptcy law as a liquidity provider. *Univ. Chicago Law Rev.* 80:1557–1624.

Bebchuk LA, Goldstein I (2011) Self-fulfilling credit market freezes. *Rev. Financial Stud.* 24(11):3519–3555.

Bennardo A, Pagano M, Piccolo S (2015) Multiple-bank lending, creditor rights and information sharing. *Rev. Finance* 19(2): 519–570.

Bester H (1985) Screening vs. rationing in credit markets. *Amer. Econom. Rev.* 75(4):850–855.

Bester H (1987) The role of collateral in credit markets with imperfect information. *Eur. Econom. Rev.* 31(4):887–899.

Biais B, Gollier C (1997) Trade credit and credit rationing. *Rev. Financial Stud.* 10:903–937.

Bisin A, Guaitoli D (2004) Moral hazard and nonexclusive contracts. *RAND J. Econom,* 35(2):306–328.

Bizer DS, DeMarzo M (1992) Sequential banking. *J. Political Econom.* 100(1):41–61.

Bolton P, Scharfstein DS (1996) Optimal debt structure and the optimal number of creditors. *J. Political Econom.* 104(1):1–25.

Boot AWA, Thakor AV (1994) Moral hazard and secured lending in an infinitely repeated credit market game. *Internat. Econom. Rev.* 35(4):899–920.

Boot AWA, Thakor AV, Udell G (1991) Secured lending and default risk: Equilibrium analysis, policy implications and empirical results. *Econom. J.* 101(406):458–472.

Bris A, Welch I (2005) The optimal concentration of creditors. *J. Finance* 60(5):2193–2212.

Brunner A, Krahnen JP (2008) Multiple lenders and corporate distress: Evidence on debt restructuring. *Rev. Econom. Stud.* 75(2):415–442.

Cerqueiro G, Ongena S, Roszbach K (2014) Collateralization, bank loan rates, and monitoring. *J. Finance*, ePub ahead of print September 17, http://onlinelibrary.wiley.com/doi/10.1111/jofi.12214/abstract.

Detragiache E, Garella PG, Guiso L (2000) Multiple versus single banking relationships: Theory and evidence. *J. Finance* 55(3): 1133–1161.

Djankov S, Hart O, McCliesh C, Shleifer A (2008) Debt enforcement around the world. *J. Political Econom.* 116(6):1105–1149.

Fama E, Miller M (1972) *The Theory of Finance* (Dryden Press, Hinsdale, IL).

Farinha LA, Santos JA (2002) Switching from single to multiple bank lending relationships: Determinants and implications. *J. Financial Intermediation* 11(2):124–151.

Franks J, Sussman O (2005) Financial distress and bank restructuring of small to medium size UK companies. *Rev. Finance* 9(1):65–96.

Gennaioli N, Rossi S (2013) Contractual resolutions of financial distress. *Rev. Financial Stud.* 26(3):602–634.

Goldstein I (2012) Empirical literature on financial crises: Fundamentals vs. panic. Caprio G, ed. *The Evidence and Impact of Financial Globalization* (Elsevier, London), 523–534.

Greenstone M, Oyer P, Vissing-Jorgensen A (2006) Mandated disclosure, stock returns, and the 1964 Securities Acts Amendments. *Quart. J. Econom.* 121(2):399–460.

Hansson D, Lennartsson AK (2011) Covenants within Swedish Credit Granting. Mimeo, Lund University, Sweden.

Haselmann R, Pistor K, Vig V (2010) How law affects lending. *Rev. Financial Stud.* 23(2):549–580.

Hertzberg A, Liberti JM, Paravisini D (2011) Public information and coordination: Evidence from a credit registry expansion. *J. Finance* 66(2):379–412.

Holmström B, Tirole J (1997) Financial intermediation, loanable funds, and the real sector. *Quart. J. Econom.* 112(3):663–691.

Ioannidou V, Ongena S (2010) "Time for a change": Loan conditions and bank behavior when firms switch banks. *J. Finance* 65(5):1847–1878.

James C (1987) Some evidence on the uniqueness of bank loans. *J. Financial Econom.* 19(2):217–235.

Jiménez GZ, López JA, Saurina J (2009) Empirical analysis of corporate credit lines. *Rev. Financial Stud.* 22(12):5069–5098.

Kahn C, Mookherjee D (1998) Competition and incentives with nonexclusive contracts. *RAND J. Econom.* 29(3):443–465.

Leuz C, Wysocki P (2008) Economic consequences of financial reporting and disclosure regulation: A review and suggestions for future research. Mimeo, University of Chicago, Chicago.

Minnis M, Sutherland A (2015) Financial statements as monitoring mechanisms: Evidence from small commercial loans. Working paper, University of Chicago Booth School of Business, Chicago.

Musto DK (2004) What happens when information leaves a market? Evidence from postbankruptcy consumers. *J. Business* 77(4): 725–748.

Myers SC (1977) Determinants of corporate borrowing. *J. Financial Econom.* 5(2):147–175.

Norden L, Weber M (2010) Credit line usage, checking account activity, and default risk of bank borrowers. *Rev. Financial Stud.* 23(10):3665–3699.

Ongena S, Smith D (2000) What determines the number of bank relationships? Cross-country evidence. *J. Financial Intermediation* 9(1):26–56.

Ongena S, Tümer-Alkan G, von Westernhagen N (2012) Creditor concentration: An empirical investigation. *Eur. Econom. Rev.* 56(4):830–847.

Parlour C, Rajan U (2001) Competition in loan contracts. *Amer. Econom. Rev.* 91(5):1311–1328.

Perotti EC, Spier KE (1993) Capital structure as a bargaining tool: The role of leverage in contract renegotiation. *Amer. Econom. Rev.* 83(5):1131–1141.

Rajan R (1992) Insiders and outsiders: The choice between informed and arm's-length debt. *J. Finance* 47(4):1367–1400.

Rosenbaum PR (1995) *Observational Studies* (Springer-Verlag, New York).

Rosenbaum PR (2002) Attributing effects to treatment in matched observational studies. *J. Amer. Statist. Assoc.* 97(457):183–192.

Sharpe SA (1990) Asymmetric information, bank lending and implicit contracts: A stylized model of customer relationships. *J. Finance* 45(4):1069–1087.

Simon CJ (1989) The effect of the 1933 Securities Act on investor information and the performance of new issues. *Amer. Econom. Rev.* 79(3):295–318.

Smith CW Jr, Warner JB (1979) On financial contracting: An analysis of bond covenants. *J. Financial Econom.* 7(2):117–161.

Stiglitz J, Weiss A (1981) Credit rationing in markets with imperfect information. *Amer. Econom. Rev.* 71(3):393–410.

Sufi A (2009) Bank lines of credit in corporate finance: An empirical analysis. *Rev. Financial Stud.* 22(3):1057–1088.

Tirole J (2006) *The Theory of Corporate Finance* (Princeton University Press, New York).

Vives X (2014) Strategic complementarities, fragility and regulation. *Rev. Financial Stud.* 27(12):3547–3592.

von Thadden E-L (2004) Asymmetric information, bank lending, and implicit contracts: The winner's curse. *Finance Res. Lett.* 1(1):11–23.

von Thadden E-L, Berglöf E, Roland G (2010) The design of corporate debt structure and bankruptcy. *Rev. Financial Stud.* 23(7):2648–2679.