Do Banks Compete on Non-Price Terms?

Evidence from Loan Covenants

Rustam Abuzov[†]   Christoph Herpfer[‡]   Roberto Steri[§]

December 2022

Abstract

We investigate the link between competition and non-price loan terms by exploiting a regulatory shock to banks' ability to offer covenant-lite loans. As regulated banks demand more covenants, borrowers trade off staying with banks and receiving lower interest rates, but with covenants, against switching to other lenders with covenant-lite loans. The least covenant-sensitive borrowers stay with regulated lenders and enjoy improvements in other loan terms, whereas weaker borrowers switch to the shadow banking segment, with the most marginal borrowers dropping out of the market completely. On aggregate, borrowers switch to unregulated lenders, leading to a decline in banks' market share.

Keywords: non-price competition, shadow banks, leveraged lending, covenants, syndicated loans, relationship lending.

[†]Darden School of Business, University of Virginia. E-mail: abuzovr@darden.virginia.edu
[‡]Goizueta Business School, Emory University. E-mail: christoph.herpfer@emory.edu
[§]Department of Finance, University of Luxembourg. E-mail: roberto.steri@uni.lu

1 Introduction

Loan contracts include price terms, such as interest rates and fees, alongside non-price terms. Among the most prevalent non-price terms are covenants, contractual provisions that restrict corporate policies through performance triggers based on thresholds for a variety of company financials. In a seminal contribution, Smith & Warner (1979) observe that "the costs of restrictive covenants are positive" and "there is a unique optimal set of financial contracts which maximize the value of the firm", implying that firms trade off limitations to valuable financial flexibility (Graham, 2022) caused by covenants with the availability and pricing of credit.[1] As covenants can be used to tailor loan contracts to firms' demands, they potentially constitute a source of non-price competition in credit markets.

In this paper, we use an exogenous shock to the ability of some lenders to design loan covenants to document that covenants are indeed an economically relevant dimension of competition among lenders. To uncover this mechanism, we focus on the competition between regulated banks and un-regulated non-bank lenders. The literature and regulators acknowledge the importance of studying the competitive dynamics of credit markets. As Adrian & Shin (2009) emphasize, a deep under-standing of how lenders compete is essential for the effective macroprudential regulation of modern economies, in which commercial banks increasingly interact with a "shadow banking" system of non-depository institutions. Existing studies focus on price competition via interest rates, while non-price terms have received little attention, with the notable exception of Murfin (2012) who studies the supply side determinants of covenants. In this paper, we take a step toward filling this gap by investigating the substitution between price and non-price terms.

To highlight the key features of our empirical analysis, we begin by laying out a stylized model in the spirit of Matvos (2013). In our model, a continuum of competitive lenders compete for a loan to a borrower. The loan specifies a certain amount and includes both price and non-price terms,

---

[1]An extensive literature highlights the role of covenants to alleviate agency concerns by setting minimum standards for borrower behavior and by acting as trip wires against deteriorating borrower performance (e.g., Jensen & Meckling (1976); Aghion & Bolton (1992); Dichev & Skinner (2002); Tirole (2006); Chava & Roberts (2008)).

namely interest rates and loan covenants. Borrowers maximize the surplus they obtain from the loan. As in Matvos (2013), covenants can be beneficial or detrimental for borrowers. On one hand, covenants allow lenders to increase their debt capacity and operate at a larger scale. On the other hand, they restrict firms' actions not necessarily in the best interest of the borrower. In addition, borrowers have a preference for a specific relationship lender. We then investigate how borrowing choices change when the relationship lender's ability to offer covenant-lite loans is restricted.

The model offers three key predictions. First, borrowers will select into one of two options. Either, they remain with the treated lender, accepting more covenants, but being compensated by relatively lower interest rates and larger amounts; alternatively, they switch to the unrestricted lender, keeping low levels of covenants, but accepting (relatively) higher interest rates and lower loan amounts compared to "stayers". Second, borrowers that gain the most from financial flexibility will switch to preserve lower covenant burdens, while those that gain less will stay.

In the data, testing the interplay between loan price- and non-price terms is inherently challenging. A simple correlation between bank's market share and lax lending standards could be due to endogeneity, for example a setting where the most over-optimistic banks both expand the provision of credit and reduce covenant protections at the same time.

To overcome this endogeneity challenge, we exploit variation in the ability of lenders to compete on non-price terms caused by the Leveraged Lending Clarification ("the Clarification") issued by U.S. regulators in November 2014. The Clarification creates heterogeneous variation in lenders' capacity to offer covenant-lite loans because it targets regulated banking and financial institutions, but not shadow banks. Regulators were concerned about competition on non-price terms triggering a "race to the bottom" driving down covenant protection, as modeled in Lee & Mann (2021). To counteract this race to the bottom, the Clarification promotes both qualitative and quantitative measures of covenant protection to tighten lending standards in the $1.4 trillion leveraged loan segment of the syndicated loan market.[2] In this setting, we trace individual corporate loans and their covenant protections using S&P's Leveraged Commentary and Data (LCD), which provides

---

[2]See, for example, S&P Global Market Intelligence report, January 2022.

comprehensive data on U.S. issued leveraged loans.[3] Importantly for us, LCD carefully tracks the covenant structure of loans and assigns a "Covenant-Lite" flag to loans without meaningful covenants.

In effect, the clarification provides a shock to one specific non-price loan contract dimension, covenant design. As regulated lenders are forced to abandon the "optimal set of financial contracts" (Smith & Warner, 1979), we can trace out the response by lenders and borrower to this shock. Do borrowers seek funding from shadow banks, which then gain market share at the expense of regulated banks? To what extend can banks modify loan terms to limit this migration, and if so, is there a market price of covenant-lite loans? Or, are borrowers rather inelastic in their response to increased covenants and the provisions that promote the use of covenants result in loans with stronger covenant protections? Which of these scenarios prevails is ultimately an empirical question.

We exploit the Clarification in a difference in differences estimation, with firms that had previously borrowed from a regulated bank our treatment group, and firms that borrowed in the shadow banking sector before the Clarification our control group. One challenge is that loan offers are per se unobservable, particularly for borrowers that end up declining an offer. Our first-stage specification estimates the effect of the Clarification on the probability that a borrower has received non-covenant-lite loan offers from the borrower's existing bank lenders when taking out a new loan, proxying for the unobservable offer by the relationship lender with the observed average contract of said lender in the relevant quarter.

Using this setup, we find that firms who had previously borrowed from bank lenders are less likely to receive a covenant lite loan offer after the clarification. We then investigate if other loan terms were affected by the clarification. Anecdotal evidence shows that practitioners and commentators indeed perceived the Clarification predominantly as a way of addressing the waning use of covenants in loan agreements, described in detail in section 3. Consistent with this practitioner view, we find no evidence of aggregate changes in other loan terms, including interest rate, loan size, maturity or collateral requirements caused by the Clarification.

---

[3]The widely used DealScan data covering the wider syndicated loan market provides poor coverage of the leveraged lending segment, particularly covenants (Becker & Ivashina, 2016).

In our model, and the hypothetical ideal experiment, two identical banks compete for a borrower. While the regulatory shock we exploit is arguably close to the exogenous shock to the ability of one lender to offer attractive non-price terms, both the assumption of identical lenders and borrowers is likely not true in our setting. Non-banks are very different from banks in their business model, size and capitalization, and non-bank borrowers are different from borrowers of banks. To assuage this concern, we take advantage of our ability to observe lenders originate several loans in a given quarter, as well as borrowers repeatedly receive loans during our sample period. This data structure allows us to saturate our empirical specifications with both lender × time and borrower fixed effects, following prior research (e.g., Khwaja & Mian (2008), Jiménez, Ongena, Peydró & Saurina (2012) and Jiménez, Ongena, Peydró & Saurina (2014)). Effectively, our tests draw inference comparing the same borrower with itself making borrowing decisions over time, as well as with other borrowers of the same lender at the same point of time.

We then turn to the reaction of borrowers and banks in response to being pushed off the optimal contract. We find that borrowers value covenant-lite loan structures, consistent with the ex-ante optimality of price-non-price terms. The Clarification affected the lending standards of treated banks by reducing their probability of offering covenant-lite loans by 19.2% relative to unaffected lenders. As a result, borrowers who originally borrowed from regulated banks have a 36% higher probability of switching to other lenders as a response to a covenant-lite offer. Importantly, we show that this switching was heavily concentrated in borrowers that did not just face more covenants offered by their previous lenders, but simultaneously were offered covenant lite loans from their new lender.

The absence of aggregate trends in interest rates and loan supply masks interesting heterogeneity on the micro level. Our theoretical model predicts that borrowers which stay with their existing lender should obtain loans with relatively more covenants, yet relatively lower interest rates compared to borrowers that switch to non-banks. When we test this prediction on firms that borrowed from banks prior to the clarification and remain with regulated lenders, we indeed find that these "stayers" receive new loans that are relatively cheaper, larger, longer lasting and less likely to require

collateral, yet have more covenant protections. These results capture a "market price of covenants" - firms face a choice between staying with regulated lenders in exchange for superior alternative loan terms, or chase covenant lite loan structures.

The logical next question is which borrowers choose to stay, and which choose to chase covenant lite loans. We investigate differences in behavior bot at the extensive and intensive margin. Bank borrowers face three options following the Clarification. Either remain with a regulated bank, accepting more covenants (worse non-price-terms) in exchange for cheaper, larger loans. Second, obtaining covenant-lite, but (relatively) smaller, more expensive loans that require more collateral in the shadow banking sector. Finally, firms could exit the leveraged loan market, either substituting these loans with bonds or foregoing raising capital all together. We would expect that those borrowers that benefit the most from covenant lite loan features will be more sensitive to an increase in covenants.

We explore two potential sources that could drive use for covenant lite loans. First, we hypothesis that weaker borrowers with worse credit ratings and more reliance on the leveraged loan market are in more need of financial flexibility, and will react more sensitively. Second, we hypothesize that borrowers with a revealed historic preference for covenant lite loans, and those borrowers specializing in covenant lie loans, should react more sensitively. Both of these predictions are borne out in the data.

While banks seem able to retain some borrowers by making up for their lack of covenant-liteness with favorable other loan terms, on aggregate, they lose market share. We find that, in total, regulated lenders who require tighter covenant structures lose roughly $30 billion in market share to the advantage of the non-banking institutions.

We then test for a number of alternative channels other than covenants that could explain our results. Regulated banks might have responded to the Clarification with a reduction of overall credit supply in the leveraged loan sector, regardless of the presence of covenants. Alleviating this concern, we find that the risk profile of borrowers served by treated lenders does not change and regulated banks do not reduce their overall loan supply. We also find no change in the composition

of borrowers, neither in terms of the average credit rating of the borrower pool nor in the presence of highly leveraged firms. These findings suggests that the effect is in fact attributable to one specific non-price term, namely loan covenants. Our findings are also robust to variations in the event window, placebo exercises counterfactually assuming treatment in any of the four years prior to the actual decision, excluding the treatment quarter, varying the sample event window, fully interacting controls, different definitions of relationship lenders, and excluding loans with split control rights (Berlin, Nini & Edison, 2020).

Our findings show that lenders compete on non-price terms, and that they can gain market share by offering laxer covenants. Our results do not just highlight the important interplay between various price- and non-price loan terms, but also affirm the necessity to "internalize the externalities that are generated in the shadow banking system" (Adrian & Shin, 2009) and account for competition on non-price terms between the regulated and the non-regulated sectors in regulation. The advantage of being exempt from regulation might be one potential explanation for the growth of shadow banking (Buchak, Matvos, Piskorski & Seru, 2018; Chernenko, Erel & Prilmeier, 2020; Prilmeier & Stulz, 2020) despite banks' advantage of benefiting from deposit insurance (Jiang, Matvos, Piskorski & Seru, 2021). Finally, our findings relate to the ongoing regulatory debate on leveraged lending. The U.S. administration is considering the re-introduction of a leveraged lending guidance as it lapsed after 2016 Presidential elections. Remarkably, administration officials intend to extend the scope of the guidance to non-banks. In this context, non-price competition between banks and non-banks can play a significant role in regulatory design.[4]

Related Literature. Our paper relates to the recent empirical literature on the role of the shadow banking system in providing credit to firms. Irani, Iyer, Meisenzahl & Peydro (2021) study the impact of non-bank participation on credit availability in the economy during a crisis. They find

---

[4]See House financial services committee letter from December 2020, which recommends the reinstatement of leveraged lending guidance. In separate remarks, Secretary of the Treasury Yellen stated that "[w]e need to change the structure of the Financial Stability Oversight Council (FSOC) and build up its powers to be able to deal more effectively with all the problems that exist in the shadow banking sector. I think the structure is inherently flawed. I think the agencies need a definite financial stability mandate." This comment was made during *Brooking's Institute Webinar: A Decade of Dodd-Frank* Link to recording

that less capitalized banks reduce loan retention to the advantage of non-banks. Murfin (2012) shows that banks that suffer losses increase covenant tightness in new contracts. Other recent papers highlight that non-banks increasingly act as lead arrangers in the syndicated loan market, and they are particularly relevant in providing credit to middle-market firms (Chernenko et al., 2020; Davydiuk, Marchuk & Rosen, 2020). We add to this growing literature on shadow banking by focusing on non-banks acting as lead arrangers in loan syndicates. In this context, our study examines the role that non-price terms play in the coexistence of regulated banks and non-bank lenders, as well as the competition between them.[5]

Our results are also linked to recent studies that examine the costs and benefits of the use of loan covenants for corporations. Matvos (2013) quantifies the benefits that covenants provide to firms as a tool to complete debt contracts. Prilmeier (2017) examines the effect of lending relationships on covenant choice and on borrowers' economic trade-offs. Demiroglu & James (2010) relate firms' risks and investment opportunities to covenant tightness. Adler (2020) considers the ex ante effect of uncertainty about bank's reaction to covenant breaches on corporate investment decisions. Our results highlight the role of non-price competition and the organization of lending markets in covenant choices.[6]

More broadly, our analysis builds on recent contributions in empirical banking, such as Ivashina & Scharfstein (2010), Berg, Saunders & Steffen (2016), Berg, Saunders, Steffen & Streitz (2017), Schwert (2017), Acharya, Berger & Roman (2018), Acharya, Eisert, Eufinger & Hirsch (2018), Giannetti & Saidi (2019), Grosse-Rueschkamp, Steffen & Streitz (2019), and Heider, Saidi & Schepens (2019). Our empirical strategy also builds on findings from the large existing literature on relationship lending. Recent seminal studies include Degryse & Van Cayseele (2000), Degryse & Ongena

---

[5]Other studies investigate how non-banks participate in corporate loans arranged by regulated banks. Jiang, Li & Shao (2010) focus on non-banks' holding of both equity and debt of the same company. Massoud, Nandy, Saunders & Song (2011) study the involvement of hedge funds in the syndicated loan market. Neuhann & Saidi (2016) study the role of banking deregulation on the participation of non-banks in syndicated loan markets. Biswas, Ozkan & Yin (2019) find that non-banks act as lenders of last resort. Berlin et al. (2020) stress that non-bank lenders benefit from the monitoring activity of banks.

[6]A large and broadly related literature has instead focused on firms' responses to covenant violations. Key recent contributions are Chava & Roberts (2008), Roberts & Sufi (2009), Nini, Smith & Sufi (2012), and Falato & Liang (2016), among others.

(2005), and Chodorow-Reich (2013). We contribute to this extensive literature by exploiting the Clarification as a source of variation in loan covenants to study non-price competition between regulated and shadow banks.

2 Theoretical framework

In this section, we lay out a stylized model based on Matvos (2013). The model largely relies on reduced-form specifications of the costs and benefits of covenant use rather than micro-founding them. Nevertheless, it serves as a benchmark to highlight key features of our empirical setting and provides additional testable predictions.

Borrowers and Loan Contracts. Consider a firm described by a vector of characteristics ξ. The firm is seeking a loan with face value equal to one from external lenders. The loan includes a covenant as a non-price state-contingent term. Denote as $\phi \in \Omega_\phi \subseteq [0, \infty)$ the strictness of the covenant, where $\phi = 0$ indicates the absence of this covenant.

Lenders and Loan Supply. Lenders are described by a vector of characteristics $\lambda \in \Omega_\lambda$. We assume at least two perfectly competitive lenders, which compete to supply the loan amount $e(\phi, \xi)$ to a firm with characteristics ξ and covenant ϕ.[7] Note that the amount $e(\phi, \xi)$ implicitly defines the market interest rate $y(\phi, \xi)$ to firm ξ as

$$e(\phi, \xi) = \frac{1}{1 + y(\phi, \xi)} \simeq 1 - y(\phi, \xi).$$

$y(\phi, \xi)$ is the ex-ante promised interest rate and may differ from the interest payments on the loan ex-post. For example, the firm might not be able to service the payments or the interest rate might be renegotiated. Going forward, we assume that $y(\phi, \xi)$ is decreasing in ϕ, i.e., stricter covenants are associated with lower interest rates and larger loans. In other words, all else equal, covenants tightness increases the lender's expected income from the loan.

Lender and Contract Choice. Let $v(\phi, \xi, \lambda)$ be the expected income generated by firm ξ when

---

[7]Observe that, albeit lender are allowed to differ in λ, interest rates and loan amounts do not depend on λ because of perfectly competitive credit markets.

borrowing from lender λ with covenants ϕ. Assume the payoff $v(\phi, \xi, \lambda)$ can be decomposed as $v_0(\xi, \lambda) + v_L(\phi, \xi)$, where $v_0(\xi, \lambda)$ captures lender preference and $v_L(\phi, \xi)$ the payoff from the loan contract. We do not impose restrictions on how $v_L(\phi, \xi)$ varies with covenant strictness. As Matvos (2013) discusses, covenants can provide benefits of contractual completeness by relaxing financial constraints and let the firm operate at a larger scale due to larger borrowed amounts (Smith & Warner, 1979).[8] On the other hand, covenants reduce financial flexibility of borrowers, a key concern of executives (Graham, 2022), and in extreme cases allow the lender to restrict firms' actions even if not in the borrower's best interest.

The firm maximizes its expected income net of expected loan repayments. As intermediaries break even in expectation, the expected repayment coincides with the borrowed amount $e(\phi, \xi)$. Thus, the firm chooses the lender-covenant pair (λ^*, ϕ^*) such that

$$(\lambda^*, \phi^*) = \arg \max_{\lambda \in \Omega_\lambda, \phi \in \Omega_\phi} v_0(\xi, \lambda) + v_L(\phi, \xi) - e(\phi, \xi)$$

For simplicity, we assume that $v_L(\phi, \xi)$ and $e(\phi, \xi)$ are differentiable and that their difference is concave. The firm chooses its "relationship lender" λ^* with the higher value of $v_0(\xi, \lambda)$. Then, the sufficient condition for covenant choice from λ^* is

$$\frac{\partial v_L(\phi, \xi)}{\partial \phi} = \frac{\partial e(\phi, \xi)}{\partial \phi} \simeq -\frac{\partial y(\phi, \xi)}{\partial \phi}.$$

At the margin, the additional payoff that stricter covenants generate equals the additional amount the relationship lender is willing to lend. Equivalently, this additional payoff from tightening covenants equals the reduction in the interest rate on the loan with a face value of one. Symmetrically, if stricter covenants reduce a firm's payoff, the optimal strictness equalizes the marginal reductions in payoff and credit supply. The surplus accruing to firms is the amount of total income

---

[8]As we do not impose structure on $v(\phi, \xi, \lambda)$, it can include several costs and benefits such as verifiable and unverifiable cash flows, private benefits, or effort unobserved by the lender.

$\mathcal{S}(\xi)$ generated by the contract the firm chooses, $v(\phi^*, \xi, \lambda^*)$ minus the funds lent $e(\phi^*, \xi)$, i.e.,

$$\begin{aligned}
\mathcal{S}(\xi, \lambda^*, \phi^*) &= v_0(\xi, \lambda^*) + v_L(\phi^*, \xi) - e(\phi^*, \xi) \\
&\simeq v_0(\xi, \lambda^*) + v_L(\phi^*, \xi) - y(\phi^*, \xi) + 1
\end{aligned}$$

Observe that, as in Matvos (2013), the interest rate enters the surplus with a positive sign. Although this seems counter-intuitive at a first glance, a higher interest rate implies fewer resources were lent and, possibly, a lower surplus for the firm.

Restrictions to Non-Price Terms. Our empirical analysis exploits exogenous variation in some lenders' ability to offer covenant-lite loans. To make predictions about firms' covenant choices, assume there are two types of lenders $\{R, U\}$. Lenders of type R are restricted to offer *stricter* non-price terms in $\Omega_\phi^R = [\tau, \infty) \subseteq \Omega_\phi$, $\tau > 0$. For example, restricted lenders cannot supply covenant-line loans, for which $\phi = 0$. Lenders of type U are unrestricted and can offer all non-price terms in Ω_ϕ.

Consider now the case in which the relationship lender is restricted, i.e., $\lambda^* \in \Omega_\lambda^R$. The surplus reduction that a firm would suffer from restricted covenant choice if the firm chooses to borrow from lender λ^* compared to the case in which λ^* is unrestricted is

$$\mathcal{S}(\xi, \lambda^*, \phi_R^*) - \mathcal{S}(\xi, \lambda^*, \phi^*) \simeq \overbrace{(v_L(\phi_R^*, \xi) - v_L(\phi^*, \xi))}^{\text{payoff component}} + \overbrace{(y(\phi_R^*, \xi) - y(\phi^*, \xi))}^{\text{interest rate component}},$$

where ϕ_R^* denotes the optimal choice of covenant tightness if $\phi \in \Omega_\phi^R$. As ϕ^* is optimal in the unrestricted problem, stricter covenants cannot increase the payoff component, i.e., $v_L(\phi_R^*, \xi) - v_L(\phi^*, \xi) < 0$. In addition, stricter covenants result is larger amounts borrowed and lower interest rates. This reduces the loss through the interest rate component $y(\phi_R^*, \xi) - y(\phi^*, \xi)$.[9]

Instead of "staying" with its relationship lender λ^* and lose surplus, the firm can choose to take

---

[9]Notice that, if the firm optimally chooses $\phi^* = \phi_R^*$, i.e., the optimal covenant bundle is one with strictness $\phi \geq \tau$, then the surplus loss is zero.

the loan from a new unrestricted lender $\lambda^N \in \Omega_\lambda \backslash \Omega_\lambda^R$. The firm "switches" lenders if $\mathcal{S}(\xi, \lambda^*, \phi_R^*) - \mathcal{S}(\xi, \lambda^N, \phi^*) < 0$, where

$$\mathcal{S}(\xi, \lambda^*, \phi_R^*) - \mathcal{S}(\xi, \lambda^N, \phi^*) = \overbrace{v_0(\xi, \lambda^*) - v_0(\xi, \lambda^N)}^{\text{relationship component}} + \overbrace{v_L(\phi_R^*, \xi) - v_L(\phi^*, \xi)}^{\text{payoff component}} + \overbrace{y(\phi_R^*, \xi) - y(\phi^*, \xi)}^{\text{interest rate component}}.[10]$$

The relationship component $v_0(\xi, \lambda^*) - v_0(\xi, \lambda^N)$ is positive because the relationship with λ^* is more valuable than the one with λ^N, for pecuniary or for non-pecuniary reasons). This is a motive for the firm to stay with its original relationship lender. The sum of the payoff component $v_L(\phi_R^*, \xi) - v_L(\phi^*, \xi)$ and of the interest rate component $y(\phi_R^*, \xi) - y(\phi^*, \xi)$ is negative. As both components do not depend on the lender's identity, if the relationship lender λ^* could offer all covenant packages in Ω_ϕ, the firm would be better off by staying and choosing ϕ^*. Their joint reduction is therefore a motive for the firm to switch to the new lender λ^N. Overall, if the relationship component is large enough to compensate the surplus reduction of operating with tighter covenants, the firm will borrow from its relationship lender. Symmetrically, the firm decides to switch lenders if the relationship component is not sufficiently valuable.

Empirical Implications. Firms' choices to seek funding from new lenders after the Clarification reveal firms' preferences over covenant-lite loans. If stricter covenants are undesirable from the borrowers' perspective, some firms will borrow from new lenders, namely shadow banks. Switching borrowers then operate in an equilibrium with covenant-lite loans and, all else equal, same interest rates and loan sizes in comparison to the period before the Clarification. In contrast, firms that stay with their relationship lenders will obtain loan contracts with higher covenant protection and, plausibly, larger loans and lower interest rates. In other words, borrowers face a choice between either lower interest rates or fewer covenants.

The relationship component also plays an important role on the decision to borrow from new lenders. In the cross-section of borrowers, those with stronger relationships with regulated banks are less likely to switch lenders after the Clarification. Similarly, firms that lose the most surplus by

---

[10]Because the payoff $v_L(\phi, \xi)$ does not depend on the lender, the firm optimally chooses covenant tightness ϕ^* also when borrowing from the new lender λ^N.

operating with tighter covenants (payoff component and interest rate component) are more likely to switch lenders after the Clarification.

3   Data and background

3.1   Institutional background: the leveraged lending clarification

To investigate the effect of loan covenants on competition in credit markets, we exploit the introduction of a clarification ("the Clarification") of the regulatory guidelines on leveraged lending for U.S. borrowers issued on November 7, 2014. This Clarification was issued by the three largest regulators of the U.S. banking sector, namely, the Office of the Comptroller of the Currency (OCC), the Board of Governors of the Federal Reserve System ("Board") and the Federal Deposit Insurance Corporation (FDIC). We refer to these collectively as *the Agencies.*

In our empirical analyses, we exploit the fact that the Clarification applies to regulated banking and financial institutions, but not the shadow banking system.[11]   The process that led to the introduction of the Clarification began on March 22, 2013. The Agencies initially issued an initial Leveraged Lending Guidance ("the Guidance") in response to what they perceived as a race to the bottom of increasingly lax lending standards driven by competition (Lee & Mann, 2021), especially in the segment of high-risk leveraged lending. Broadly speaking, this Guidance called for improved underwriting standards for leveraged loans. To this end, the Guidance formalized the definition of *leveraged lending* (as in Table A.2 of the Appendix), and it specified minimum loan underwriting standards.

Importantly, there was considerable uncertainty among market participants as to its scope and the its applicability for different market participants. For this reason, compliance was initially low, to which the Agencies reacted by issuing the industry-wide Clarification in November 2014. To

---

[11]Specifically, the Clarification applies to "national banks, federal savings associations, and federal branches and agencies supervised by the OCC; state member banks, bank holding companies, savings and loan holding companies, and all other institutions for which the Federal Reserve is the primary federal supervisor; and state nonmember banks, foreign banks having an insured branch, state savings associations, and all other institutions for which the FDIC is the primary federal supervisor."

the extent that the Clarification was not completely anticipated, it serves as a shock to the loan covenant designs of regulated banks. Our interpretation is reinforced by the fact that the Agencies sent out the so-called Matters Requiring Immediate Attention (MRIA) letters to several banks in late summer 2014. Market participants widely considered these letters to be the turning point where lenders started implementing the regulations. [12] Based on these institutional details, as well as the evidence from Kim, Plosser & Santos (2018), we use the introduction of the Clarification as a starting point for the treatment period

The Clarification points to both qualitative and quantitative measures of covenant protection ("few or weak covenants"), and it was perceived by participants as predominantly addressing the waning use of covenants in loan agreements. News articles and industry professionals who discuss the Clarification typically refer to covenants as the primary target of the regulators. An article in *The Wall Street Journal* (Tracy, 2015) describes how "[r]egulators were explicit about loan characteristics that would grab their attention, such as lax repayment time lines and the absence of loan covenants." Similarly, Tan & Tracy (2015) focus on the MRIA letter sent to Credit Suisse and explain that

> They [the Agencies] also told banks to limit borrowing agreements that stretch out payment timelines or don't contain lender protections known as covenants.

In addition, regulators themselves emphasized covenants when discussing or analyzing the impact of the Clarification. For example, the 2015 Shared National Credits (SNC) Review underscore the relevance of covenants and link them directly to aggressive competition in the market.

> The most frequently cited underwriting deficiencies identified during the 2015 SNC review were *minimal or no loan covenants*, liberal repayment terms, repayment dependent on refinancing, and inadequate collateral valuations. The weak under-

---

[12]In July 2014, Credit Suisse received a first MRIA letter expressing regulatory concerns about its underwriting standards, and other banks received MRIA letters regarding leveraged lending in the third quarter of 2014. Failure to respond promptly to MRIA letters may be subject to formal enforcement actions, which "include cease and desist orders, formal written agreements under U.S. federal law, and Prompt Corrective Action Directives" (Webb, 2016).

writing structures were in part attributable to *aggressive competition* and market liquidity.

These observations from the media and official press releases underline the covenant channel as the primary mechanism affecting the dynamics of lending after the regulation. This is a hypothesis that we confirm empirically by measuring how much emphasis the agencies put on various loan dimensions in the actual regulatory documents. We find that regulators talked extensively about loan covenants in both the initial guidance (7 mentions) and Clarification (4 mentions). The only other loan dimension that receives comparable attention is collateral, which is mentioned 20 times in the original Guidance, and 3 times in the Clarification. In contrast, loan amounts, maturities and interest rates are barely mentioned in either document, and if mentioned not in the context of origination loan terms. This further validates the covenant channel as the primary mechanism by which the Clarification impacted the leveraged lending market.[13]

Before the Clarification, the top lead arrangers in the covenant-lite, leveraged lending market featured only two non-banks: GE Capital and Jefferies (see Panel A of Appendix Table A.3). The top lenders were dominated by regulated lenders such as Credit Suisse or Bank of America. After the Clarification, there was a substantial increase in the market share of non-banks. Regulated banks lost market share while all non-banks gained market share, and the representation of non-banks among the top issuers (presented in Panel B of Appendix Table A.3) doubles from two to four. These results indicate a shift in market share from regulated banks to unregulated non-banks around the regulatory event, which limited the ability of banks to issue covenant-lite loans.

In an initial, timely program assessment of the regulatory actions taken by the agencies, Kim et al. (2018) document broad trends in lending around the Clarification. Our paper has a different focus, in the sense that it exploits this regulatory event to trace out the adjustments made by banks and borrowers following an isolated shock to a specific loan term, covenants. On the other hand, Kim et al. (2018) focus on the macro trends in lending surrounding the regulations, without taking

---

[13]Since loan terms are set jointly, out main specification does not include controls for other endogenous loan terms that might change with the Clarification. We verify that all our specifications remain robust to controls for variables that proxy for other non-price loan terms mentioned in the 2015 SNC Review.

loan covenants into account. To the degree that the papers overlap, we identify loan covenants as the specific economic channel through which the Clarification acted and we provide a detailed, theoretically founded analysis of the trade offs borrowers face with respect to different price- and non-price loan terms. While some of our more side results seem to confirm their findings, others diverge. This is likely due to differences in data sets, as the LCD data allow us to actually identify specific leveraged loans a type of loans that is particularly badly covered in DealScan (Becker & Ivashina, 2016).

3.2 Data sources

Our primary source of loan data is S&P's Leveraged Commentary and Data (LCD) database, which is a comprehensive source of U.S. issued leveraged loans. Unlike the broader LPC DealScan, LCD focuses on the leveraged segment of the syndicated loan market. LCD is the most suitable data source for this study because the Clarification specifically targeted leveraged loans. We restrict our analyses to lead arrangers, since the Clarification emphasizes that the rules apply primarily to lenders who *originate* loans, even if they do not subsequently hold any stake in that loan. Eventual loan securitization or sale in the secondary loan market therefore does not exempt loans from the Clarification. Because the Clarification applies only to commercial banks, it is key to our analysis to accurately classify lenders as *banks* versus *non-banks* to determine their treatment status. We therefore manually identify affected banks based on the list of commercial banks provided by the Federal Reserve as well as the list of FDIC-insured banking institutions.

LCD is a critical resource for our analysis since, as Becker & Ivashina (2016) and Billett, Elkamhi, Popov & Pungaliya (2016) discuss, Dealscan has a poor reporting quality in the leveraged lending segment, which results in a widespread misclassification of covenant-lite loans. Both LCD and Dealscan link the definition of leveraged loans to the loan rating and interest rate over LIBOR. However, only LCD allows us to verify the classification of loans, as this database reports both loan ratings and interest rates. Covenant-lite loans are defined in LCD as those "that have bond-like financial incurrence covenants rather than traditional maintenance covenants that are normally part

and parcel of a loan agreement."[14]

In analyses at the lender level, we use Thomson Reuters' LPC DealScan, which has a significantly longer sample available (coverage is comprehensive since 1996) and covers the syndicated loan market more broadly. In these analyses, we aggregate data to the package level, and we consider only loans that contain information on covenants, following Berlin et al. (2020). In these tests, we identify leveraged loans as those with a primary purpose recorded as *Acquisition Line*, *Takeover*, *Merger*, *LBO*, *MBO*, *SBO*, *Dividend Recap*, or *Stock Buyback*. This definition corresponds to the first feature of leveraged loans outlined in the Clarification (see Table A.2), and we additionally require that loans have an all-in-drawn spread over LIBOR of more than 150 basis points, which is another widely used filter in the empirical literature on leveraged lending. We link borrower characteristics to the Dealscan data via the Compustat linking file provided by Michael Roberts (Chava & Roberts, 2008).

3.3 Summary statistics

Table 1 presents descriptive statistics from the LCD dataset from 2012 to 2018. Panel A refers to the entire sample, Panel B contrasts banks and non-banks, while Panel C contrasts covenant-lite loans and the remaining loans. Panel A shows that the average leveraged loan is $640 mn, bears an interest rate of 4.08%, and has a maturity of almost 6 years. Only 2% of loans are secured by collateral, and approximately 96% of borrowers have a credit rating at the time of loan origination. We find that 28% of lending relationships are new (i.e., the borrower never borrowed from the same lender after our sample begins).[15] Roughly half of loans are covenant-lite, and the vast majority of lenders have an existing banking relationship (with at least one bank) at the time of loan origination.

Panel B shows that, compared to banks, non-banks on average have smaller deals, charge higher interest rates, and lend more to un-rated borrowers. This suggests that the shadow banking system likely picks up riskier borrowers in the leveraged lending market. Perhaps not surprisingly, borrowing

---

[14]Maintenance covenants are more restrictive than incurrence covenants because they require the borrower to meet a requirement every quarter to avoid violation. Incurrence tests instead simply require that the borrower is in compliance when taking a specific action (e.g., paying a dividend, issuing new debt).

[15]Since our data are left censored, we designate relationships as *new* starting from each firm's second loan.

relationships with non-banks tend to be classified as *new* more often, namely 38% of the time versus 27% for regulated banks. This indicates their increasing involvement in the leveraged loan segment over time. Finally, only 37% of loans originated by non-bank lead arrangers involve a borrower who has a previous lending relationship with a regulated bank, while the figure rises to 96% for bank-originated loans. Banks are significantly more active than non-banks in the 2012–2018 period in the leveraged loan segment, as the number of observations for the two groups suggests. Overall, Panel B suggests that the behavior of our treated and control lenders is not comparable in the leveraged loan market along a number of different dimensions. This underscores the importance of saturating the model with lender × time fixed effects to control for these different behaviors.

The statistics in Panel C highlight that the average covenant-lite loan is larger, bears a higher interest rate, has a longer maturity, is less likely to have collateral, and is more likely to involve rated borrowers. Appendix Table A.3 adds to this by reporting the top lead agents in the covenant-lite segment. After the Clarification, prominent non-bank lenders (e.g., Jefferies Finance, the General Electric Capital Corp.) increased their participation. This is in contrast to banks, which in most cases decreased the covenant-lite segment of their leveraged lending. The number of non-bank lenders in the Top-15 doubles from two to four.

4 Loan covenants and lender choice: empirical strategy and results

4.1 Non-price terms and lender choice

We begin our analysis by presenting a number of results from non-parametric tests in Table 2. Panel A offers a transition matrix representation of our main findings, by focusing on the frequencies at which borrowers move from banks to non-banks, and vice versa, both before and after the Clarification. The two leftmost sub-panels indicate that, after the Clarification, the chance that a borrower will switch from a bank to a non-bank lender increases from 3.3% to 5.1%, a relative increase of almost 60%, while the probability of switching from a non-bank lender to a bank drops from 47.3% to 35.8%, a relative drop of about 25%. The comparison of frequencies in the rightmost

17

sub-panel shows that all changes are statistically significant at the 1% level. The facts in Panel D suggest an effect of the Clarification on lender–borrower relationships. Panel B additionally tabulates the fraction of covenant-lite lending for the same four groups as in Panel A of Table 2, while focusing on firms that switch lenders. We find that around the Clarification, the increase in the prevalence of covenant-lite lending by non-banks is higher than the increase in covenant-lite lending by banks. These facts are suggestive of borrowers switching from banks to non-banks to receive loans with less covenant protection. In the remainder of this section, our analysis hones in on covenant-lite loan terms as the specific economic channel driving the change in switching behavior for bank- and non-bank borrowers around the Clarification.

Relationship Lenders and Treated Loans. Our empirical setup relies on inferring unobservable loan offers made to former bank borrowers through the observable, realized loan terms of their relationship lenders. This setup relies on two assumptions. First, the well-documented fact that borrowers generally approach their relationship banks for new loans (Chodorow-Reich, 2013; Ivashina & Scharfstein, 2010). Second, that the realized loan terms at any point of time represent a good proxy for the unobservable, offered loan terms. This second assumption is by definition un-testable without access to private loan offers. However, we note that the realized loan offered terms will, if anything, be more attractive than the unobservable offers, effectively a winner's curse. If we find a deterioration of realized loan terms by regulated banks, this represents a lower bound for the deterioration of unobservable offers.

We identify a loan in our data with a borrower index b, a lender index l, and a quarter index t. The tuple (b, t) generally suffices to uniquely identify a loan in our sample, except for the cases in which the same borrower receives two leveraged loans in same quarter with different lead arrangers. Because these occurrences are rare (about 50 loans, or fewer than 2%) and are potentially the result of data errors, we exclude them from our main analyses.[16]

The main idea behind our analysis is that borrowers who seek a loan after the Clarification receive loan offers from regulated institutions that include relatively more covenants than the offers

---

[16]Our results are unchanged when including these observations.

from non-bank institutions. As a result, these borrowers migrate to the non-bank sector. A key challenge in the broader banking literature is that we observe only *realized* loan terms, while our ideal variable of interest would be the *offered* loan terms, which are unobservable. To cope with this data limitation, we build on the broad consensus in the literature about the importance and stickiness of banking relationships, which implies a cost to borrowers who switch lenders (Chodorow-Reich, 2013). Our identifying assumption is that borrowers approach their relationship banks first when shopping for a new loan. For each loan b at time t, we define the *relationship lender* as the last lender to which borrower b was linked in our sample before quarter t. Similar to Chodorow-Reich (2013), we consider only the most recent loan to determine the relationship status.[17]

Our first set of tests investigates how loan offers changed following the Clarification. The dependent variable in our specification, $NonCovliteOffer_{b,l,t}$, captures the likelihood that borrower b receives a non-covenant-lite loan offer from its current lender while accepting a loan offer from lender l in quarter t. We define $NonCovliteOffer_{b,l,t}$ as the fraction of non-covenant-lite loans from the relationship lender of loan (b,t) to all other borrowers it lends to in the same quarter t that are not covenant-lite. We define the indicator variables $BankBorrower_{b,t}$ and $Post_t$ to match treated loans and the Clarification period, respectively. Our specification is

$$New\ lender_{b,l,t} = \beta_0 + \beta_1 Bank\ borrower_{b,t} \times Post_t + \beta_2 X_{b,l,t} + \delta_b + \beta_3 Bank\ borrower_{b,t} + \eta_{l,t} + \varepsilon_{b,l,t}, \quad (1)$$

Our focus is on loan offers extended to borrowers whose previous lender was a regulated bank ($BankBorrower_{b,t} = 1$) because these lenders were affected by the Clarification. Therefore, our "treatment" group consists of borrowers who received their previous loan from a regulated lender. In contrast, loans in which the relationship lender is a non-banking institution ($BankBorrower_{b,t} = 0$) should have been unaffected by the Clarification, thus they comprise our control group. The

---

[17]Syndicated loans often exhibit a structure in which a large, regulated bank serves as the lead arranger, and the syndicate consists of a mix of regulated banks and non-banks. Importantly, the Clarification applies to loans even when only a single regulated bank served as the lead arranger and all participants were unregulated banks. We therefore assign treatment based on the lead arranger being a regulated bank only.

identifying assumption behind our setup is that lending relationships are sticky, and borrowers would have returned to their previous lenders absent the regulatory intervention. This assumption is consistent with a vast body of evidence from relationship lending, including in the U.S. syndicated loan market (see, for example, Chodorow-Reich & Falato (2018)). Note that, unlike in the basic DiD setting, our "treatment" indicator *BankBorrower* is time varying. Because the same borrower b could have borrowed from either a bank or a non-bank at different times, our unit of treatment is the loan (b, t).

We then define $Post_t$ as an indicator equal to one for all quarters from 2014Q4 to 2018Q4 and equal to zero from 2012Q1 to 2014Q3. Our sample spans the 6.5 calendar years from 2012Q1 to 2018Q2, so our sample is divided into two portions that span roughly three years before and four years after the Clarification, respectively.[18] The variable $X_{b,l,t}$ is a vector of indicators for loan purpose and the presence of a credit rating.

δ_b denotes borrower fixed effects, $\eta_{l,t}$ denotes lender $\times$ quarter fixed effects, and $\varepsilon_{b,l,t}$ is an error term. The term $Post_t$ is absorbed by $\eta_{l,t}$. In all specifications, we cluster standard errors at the borrower level to account for arbitrary within-borrower correlation of errors.[19]

These highly saturated fixed-effect specifications absorb many confounding factors. *Borrower* fixed effects refers to comparing the likelihood of a borrower switching lenders over time for the same borrower to account for unobservable heterogeneity between borrowers in their likelihoods of changing their lending relationships. Even more importantly, in our most complete specification, the lender $\times$ time fixed effects rule out the proposition that our findings are driven by a particular concern, namely, that the most overoptimistic banks are the most likely to offer covenant-lite loans and, at the same time, are also the most likely to show the largest expansion of their loan portfolio. Keep in mind that this is a *current lender* fixed effect, while treatment is determined based on the

---

[18]In robustness tests presented in Appendix Table A.5, we show that our results are robust to changing this time window to a symmetric 1-, 2-, 3-, or 4-year window, respectively, with the strongest effects observed in a 1-year window.

[19]As an alternative, we verify that our results are robust to clustering at the lender level. Note that our setup features only a single event, so it is not subject to the weighting issues that arise in staggered-treatment timing two-way fixed effects settings due to variation in group-time average treatment effects (Callaway & Sant Anna, 2020).

nature of the *previous lender.*

4.1.1 Covenant-lite loans and new lending relationships

We begin by investigating how borrowers respond to the changed loan offers. Table 3 reports results from estimating regression 1 with the outcome variable being New Lender, an indicator for whether a loan is taken out from a lender that had not previously extended credit to the firm. Columns (1) to (4) in Table 3 include the same controls as in Table 4. For ease of exposition, $NonCovliteOffer_{b,l,t}$ is standardized to have a mean of 0 and a standard deviation of 1, which means that the coefficient can be interpreted as the effect of a one standard deviation increase in the explanatory variable. The variable $NonCovliteOffer_{b,l,t}$ is the proxied likelihood that a borrower receives a loan offer from the previous lender containing at least one financial covenant. The coefficient estimate on this variable is positive and statistically significant across all specifications. The estimated effect is strongest in our baseline (and most stringent) specification reported in Column (4). Its point estimate is roughly 0.36 and statistically significant at the 1% level. However, the coefficient is statistically significant at the 5% and economically meaningful at about 0.2 in all specifications.

On the aggregate level, we find that about 50 borrowers switched from traditional to shadow banks as a result of the reform. In correspondence with the mean (median) loan size of treated firms, this represents more than \$30 billion (\$20 billion) in lending that migrated to the shadow banking sector.

Parallel trends in covenants and switching Figure 1 provides parallel trends graphs for both covenant lite loan offers and the propensity of borrowers to switch lenders.

Importantly, there is no visible divergent pre-trend of increasing covenant-lite lending by non-banks in the pre-Clarification period. Instead, the fraction of lending that includes at least one covenant decreases among both banks and non-banks in lockstep, from about 80% in 2012 to 50% of lending in 2014. This deterioration in the number of covenants is consistent with the "race to the bottom" feared by regulators and market observers (Lee & Mann, 2021). After the Clarification, the fraction of leveraged non-bank lending that contained covenants plummets from 50% to about

20% in 2018. Among regulated banks, the fraction of lending tied to covenants holds steady at 50% in 2015 followed by a gradual decline to 35% in 2018. Figure 1 supports our interpretation that the Clarification impacted the propensity of regulated banks to offer non-covenant-lite loans. The Clarification was temporarily successful in stopping the race to the bottom in covenant-lite lending for regulated banks. Absent the intervention, this trend would have continued on the same trajectory as non-banks.

Panel B shows parallel trends in terms of the fraction of borrowers that switch lenders. Prior to the Clarification, borrowers switch to banks and non-banks at identical rates and move in parallel. Following the Clarification, however, the fraction of new loans made out to switchers by nonbanks drastically increases, from about 40% to 60%. At the same time the fraction of bank loans made out to switchers falls from 40% to 30%.

We provide a number of robustness tests in the online Appendix. Results are robust to variations in the event window, with the strongest effects concentrated in the closest window, the year surrounding the Clarification (Table A.5). A set of placebo estimates assigning treatment to any of the quarters prior to the Clarification consistently shows non-results Table A.6. Our results are robust to dropping the quarter with MRIA letters (Table A.7), interacting all control variables with our post indicator (Table A.8), and replacing the Clarification with the original Guidance (Table A.10 and Table A.11).

Table 4 reports the results of estimating DiD regressions similar to those from **??**, with the outcome variable an indicator for whether a borrower receives a loan offer that is covenant-lite. Column (1) shows that the coefficient of $BankBorrower_{b,t} \times Post_t$ is 0.170 and is statistically significant at the 1% level. Columns (2) and (3) introduce loan-level control variables and add time-invariant lender fixed effects. Finally, Column (4) presents our most stringent specification, in which we saturate the model with lender $\times$ quarter fixed effects which account for potential time series variation in the supply side determinants of covenants (Murfin, 2012). The coefficient increases, with a point estimate of 0.192. Economically, this coefficient indicates that borrowers who have an existing relationship with banks have roughly a 20% higher probability of facing a

loan offer that includes covenants from their relationship lenders after the Clarification compared to borrowers from non-banks.

Overall, the table shows that borrowers who historically borrowed from regulated banks are less likely to obtain covenant-lite loan offers after the Clarification. This result can be interpreted as the Clarification affecting the lending standards of affected banks and reducing their supply of covenant-lite loans.

We then zoom in on the dynamics of the treatment effect before and after the Clarification. Figure 2 plots the coefficients on a set of indicators for each quarter in the sample in place of $Post_t$. The estimates are from the most stringent specification (Column 4) of Table 4. The graph shows that, before the Clarification, borrowers who had previously borrowed from regulated banks do not show a different likelihood of having covenants in their next loan. All coefficients before the Clarification are either zero or marginally negative. After the Clarification, however, all coefficient estimates for former bank-borrowers are positive and (mostly) statistically significant for two years. The figure suggests that the drop in offered covenant-lite loans to borrowers from regulated lenders coincided with the Clarification. There also seems to be a pattern of decreasing effects over time, which coincides with a 2017 ruling by the GAO that congressional authorization was required for the Clarification, effectively rescinding the regulation.

4.1.2 Borrowers only exhibit elevated switching if the new loans are covenant-lite

One concern is that our main results only show that borrowers are more likely to switch to a different lender after the Clarification reduces their chances of obtaining a covenant-lite offer from a regulated bank relative to a non-bank. This result is silent on *what type* of lender they switch to, or what type of loan terms their new lender offers.

We further sharpen our analysis in Table 5 where we investigate the propensity to switch to different sub-categories of new lenders. To this end, we refine our second stage outcome variable for specific subgroups of new lenders. First, in Column 1, we re-print the result from our most complete specification in Table 3 with the increase in the likelihood of switching to any new lender

after receiving a non-Covenant lite loan offer being 0.36, or 36 percentage points.

We then modify our outcome variable. In Column 2, we replace *New lender* to take the value of one only if the new lender is *also* offering a covenant lite loan. In Column 3, we re-define it to include only loans that are both from a new lender and are *not* covenant lite. The key factor in explaining switching to new lenders is the offer of a covenant lite loan. The heightened propensity to switch only sows up if the new lender offers a covenant lite loan, and there is no statistically or economically significant effect of switching to new lenders that do not offer covenant lite structures.

In an additional test, Table A.13, we repeat this exercise for other dimensions of loan contracts. Columns 1 and 2 present results with the outcome variable defined as a new loan with above (bellow) median interest rates. The following columns show results for above (bellow) median maturity, collateral, and loan size. The general pattern of these tests is that former bank-borrowers switch to new lenders after the Clarification irrespective of loan spread, amount and maturity. The only relevant dimension seems to be collateral, with borrowers switching only if the new loan contains low levels of collateral. Interestingly, this finding is consistent with the anecdotal evidence in subsection 3.1 which shows that regulators were concerned mostly about covenants and collateral. As regulated banks adjust these two parameters, unregulated shadow banks capture their previous borrowers through those two dimensions. Finally, we note that, while both low collateral and covenant lite loans seem able to attract borrowers, the economically far more meaningful of the two dimensions are covenants. Only 2% of loans in our sample carry specific collateral, while about half feature covenants.

4.2   Heterogeneity in loan terms and borrower reaction

Our model predicts that banks can try to make up for the (mandated) higher covenant protections through, for example, lower interest rates or higher loan amounts. If that was the case, the "package" of loan terms offered should reveal the trade offs between these different loan characteristics. These trade offs between covenant protection and other loan terms are known to be complex. Roberts & Schwert (2020), for example, find that the relationship between covenants and interest rates varies

based on heterogeneity of banks, borrowers, and interest rate environments. In our setting, it is possible that banks are compensating for less attractive covenant structures with lower interest rates, longer maturities, less collateral and larger deal size. It is ultimately an empirical question whether banks changed other loan terms after the Clarification, and if so, in which direction. We investigate this question in Table 6, where we run our first-stage DiD regression for five loan contract dimensions.

Each column repeats our most complete specification (equation 1) with different loan dimensions as outcomes, namely loan interest rate, maturity, collateral, and size. We find results that banks indeed charge lower interest rates following the Clarification, consistent with the model's prediction that attractive price terms can act as a substitute for attractive non-price terms. We also find increased collateral requirements, consistent with the Clarification stressing the need for proper collateral, and an economically small, but statistically significant drop in loan maturity of about 2 months. The two latter results are consistent with overall more cautious lending practices by regulated banks, while the lower interest rates speak to the equilibrium adjustments undertaken by banks to compensate for their lower ability to offer covenant lite loan structures.

In Appendix Table A.12, we present a final robustness test regarding the interaction of the Clarification with other loan contract dimensions. The results show that our results hold when excluding loans with "split control rights," which includes loans that are simultaneously issued with a covenant-lite term loan and a revolver (similar to Berlin et al. (2020)).

How do different borrowers react to the changed set of loan terms on offer? In Table 7, we test this question with a sample containing a single observation per borrower, only keeping borrowers of regulated banks in their last loan pre-Clarification. In columns 1 to 3, we estimate linear probability models in which the outcome variable is an indicator for whether a borrower leaves the leveraged loan market after the Clarification, that is, does not take out any more leveraged loans post-Clarification.[20] In effect, these tests estimate the "extensive" margin of the Clarification. We find that borrowers with worse credit ratings are more likely to exit the market (Column 1), an effect

---

[20]Note that, due to the change in data structure, these models have to feature industry and time-of-last-loan fixed effects in lieu of the borrower and time-of-loan fixed effects of our main regressions.

that is nonlinear, and concentrated among the borrowers with the worst credit ratings.

Furthermore, the borrowers most likely to leave the market are those that rely most heavily on leveraged loans, measured as the number of pre-Clarification deals done (Column 2). Consistent with the most marginal borrowers getting pushed out of the market, we find that borrowers with smaller, more expensive previous loans are leaving at higher rates (Column 3).

In columns 4 to 6 of Table 7, we investigate the intensive margin of the Clarification, that is, the decision to switch from regulated lenders to shadow banks *conditional on staying in the leveraged loan market.* Conditional on staying, we find very similar patterns as on the extensive margin, with more poorly rated borrowers with a demonstrated reliance on leveraged loans and worse previous loan terms first to switch lenders. While coefficients generally go in the same direction as in the extensive margin tests, their magnitude is substantially weaker with estimates about one third of the economic magnitude of those for the extensive margin. Taken together, on the extensive margin, the most marginal borrowers leave the market altogether, while on the intensive margin the next-most-marginal borrowers switch to non-banks. These results are consistent with the anecdotal evidence presented in subsection 3.1 that the main channel through which the Clarification worked was covenants, and, to a lesser degree, collateral. The financially weakest borrowers are least likely to be able to comply with hightened covenant requirements or put up sufficient collateral, and hence react most sensitively to the change in loan terms.

4.2.1   Revealed preference for covenant-lite loans

Another hypothesis coming from the model is that the Clarification's impact should be amplified for those borrowers with a revealed preference for covenant lite loan structures. The model predicts that borrowers with a revealed preference for covenant-lite loans before the Clarification are likely gaining the most benefit from these covenant-lite loan structures. Similarly, lenders who rarely offered covenant-lite loan structures before the Clarification were less limited by the regulation and their clients should be less affected since their revealed optimal contract included covenants to begin with.

26

To test these conjectures, in Column 1 of Table 8, we investigate the cross-sectional effects of *Last loan pre-Clarification CovLite*, an indicator equal to one if the borrower's last loan prior to the clarification was covenant-lite. Effectively, this specification tests for cross-sectional heterogeneity based on demonstrated covenant-lite loan demand.[21]

In Column 2, we instead look into the cross-sectional effects of the lender's propensity to offer covenant-lite contracts, namely the indicator *High CovLiteShare*. The latter is equal to one for relationship lenders that extended an above-median fraction of covenant-lite loans in the five years before the Clarification and 0 otherwise. This specification therefore tests for cross-sectional heterogeneity based on demonstrated covenant-lite loan supply.

In Column 1, the coefficient on $NonCovliteOffer$ is 0.285 and is statistically significant. The interaction term with *Last Loan CovLite* has a coefficient of roughly 0.13, which statistically significant at the 5% level. Thus, borrowers who are more dependent on covenant-lite loans before the Clarification are almost 50% more likely to switch lenders in reaction to receiving a non-covenant-lite loan offer after the Clarification.

In Column 2, $NonCovliteOffer_{b,l,t}$ has a statistically significant coefficient of 0.380. The interaction with $HighCovLiteShare_{l,t}$ has a large and significant coefficient of approximately 0.499. These results suggest that relationship lenders who extended more covenant-lite loans before the Clarification were more likely to lose borrowers who sought funding without covenant protections.

Jointly, these results suggest that on the demand side, borrowers who are more dependent on covenant-lite loans are more likely to switch lenders as a response to the Clarification. On the supply side, lenders with a high historical presence in the covenant-lite segment are more likely to experience an outflow of borrowers to the advantage of other lenders.

---

[21]Note that, for ease of exposition, Table 8 displays only the interaction terms. The un-interacted indicator for *Last Loan CovLite* is absorbed by the borrower fixed effect .

5 Aggregate patterns and external validity

5.1 Loan covenants and lender-level market share

In this section, we switch our focus to the lender level and test whether the restrictions on issuing covenant-lite loans that regulated lenders face after the Clarification translate into a loss of market share to the advantage of the shadow banking sector. Notice that our analysis in the previous section, which shows that individual borrowers switch lenders after the Clarification, does not immediately imply an increase in the *aggregate* market share of non-banks. For example, bank borrowers might be switching to other banks, and banks might also capture borrowers that previously took loans from non-banks.

Our empirical strategy to estimate the effect of loan covenants on banks' market share is similar to the strategy we employ at the loan level in our main analysis. It relies on a DiD setting in which the Clarification acts as the break int he time dimension, and regulated banks form the treatment group. The key difference is that we aggregate data at the lender–quarter level to analyze the effect of the Clarification on lenders' market share.

Table 9 presents the estimation results for both the fraction of covenant lite loans offered by lenders (Column 1) and the total market share these lenders have. The coefficient on the interaction term $Bank_l \times Post_t$ in column 1 is -0.145 and statistically significant at the 1% level. Thus, after the Clarification, regulated banks reduced the fraction of their covenant-lite loans among their newly issued loans by roughly 15% relative to non-banks. These estimates provide a bank-level equivalent to the results shown in Table 4, and they suggest that the Clarification was effective in stopping regulated banks from engaging in a race to the bottom in terms of covenants in the leveraged loan market.

The coefficient on $Bank_l \times Post_t$ in Column 2 is -1.210, and it is statistically significant at the 10% level. This implies that regulated lenders lost about 1.2% of the total market share following the Clarification.

Overall, the decisions of individual borrowers to switch lenders leads to a drop in the aggre-

28

gate market share of affected banks, with non-banking institutions capturing a larger share of the leveraged loan segment. This result is consistent with recent work showing an increase in the participation of non-banks in the leveraged loan market more broadly (Schenck & Shi, 2022; Irani et al., 2021).

6 Alternative channels and additional tests

In this section, we investigate alternative channels that could plausibly confound the effect of the Clarification on loan covenants and lender choices. The Clarification not only refers to covenants; it also considers other aspects of loan contract design, namely the capacity of borrowers to repay loans, the sustainability of their enterprise value, borrower leverage, and their ability to reduce leverage based on cash flow projections within a reasonable period of time. In addition, on the supply side, banks might have reduced their lending in the leveraged loan sector as a whole, regardless of the presence of covenants in loan agreements. We investigate if the Clarification could have impacted lenders' choice of borrowers and the market share of banks through those other dimensions rather than loan covenants.

6.1 Changes in borrower ratings and repayment capacity around the Clarification

The first challenge to the exclusion restriction is that lenders stopped extending loans to high-risk borrowers altogether, thus shifting lending to higher quality borrowers. We estimate these regressions in Panel A of Table 10, where we consider measures of borrower quality as dependent variables. Specifically, the dependent variable in Column 1 is the borrower's numerical credit rating, encoded such that lowest value corresponds to highest ratings. The dependent variable in Column 2 is an indicator equal to one if a borrower has a non-investment-grade rating. In Column 1, the estimated coefficient on $Post_t \times BankBorrower_{b,t}$ is - 0.040 with a standard error of 0.034. Similarly, in Column 2, the estimated coefficient on the borrower having an investment-grade rating is close

to zero. These results show no indication that the Clarification led to a shift in lending of regulated institutions to higher quality borrowers. Appendix Figure A.1 provides a graphical inspection of borrowers' risk profiles before and after the Clarification. This figure corroborates the intuition that the average credit rating of bank borrowers is stable around rating B between 2010 and 2018.

Another measure of borrower quality is their ability to repay loans, measured by the ratio of loan size to EBITDA. The Clarification specifically mentions loan to EBITDA ratios above the factor 6 as a "red flag" in the evaluation of borrowers' repayment capacity. Panel B of Table 10, therefore, considers changes in the debt-to-EBITDA ratios of borrowers following the Clarification. Column 1 considers the borrower's debt-to-EBITDA ratio as the continuous outcome, while the dependent variable in Column 2 is an indicator variable equal to one if the debt-to-EBITDA ratio is larger than 6.

As in Panel A, both columns show that the coefficients are economically very small and not statistically different from zero. In particular, Column 1 shows that the point estimate of $Post_t \times BankBorrower_{b,t}$ is 0.234, which would suggest that regulated lenders extended credit to borrowers with higher debt-to-EBITDA ratios, hence they had less debt repayment ability than before. In Column 2, the coefficient on $Post_t \times BankBorrower_{b,t}$ is -0.292, which suggests that regulated banks were slightly less likely to extend credit to the most financially stressed borrowers, although the coefficient is not statistically significant. The coefficient of $BankBorrower_{b,t}$ in Column 2 is negative and statistically significant at the 1% level. This indicates that, regardless of the Clarification, regulated banks are less likely to lend to borrowers with extremely high debt-to-EBITDA ratios. Together, these results again provide no support for the potential challenge to the exclusion restriction that regulated lenders shifted funding away from borrowers who had low ability to repay their debt claims around the Clarification.

6.2   Loan supply around the Clarification

Finally, in Panel C of Table 10, we turn our attention to the overall loan supply, and in particular variations in the presence of regulated banks in the leveraged lending segment around the Clarifica-

tion. The potential challenge to the exclusion restriction from loan supply is that regulated banks reduced their *absolute* level of leveraged lending, which led to a *relative* decline in market share compared to shadow banks. To test this conjecture, we limit our sample to regulated lenders and aggregate data at the lender–quarter level. The dependent variable in Column 1 is the (log) amount of leveraged lending for bank b in quarter t. The dependent variable in Column 2 is the fraction of leveraged lending from the LCD data to total lending in the syndicated loan market from Dealscan. The resulting ratio, *Leveraged Loans to Total Lending*, captures the loan supply in the leveraged segment relative to the overall loan supply of lenders. These two outcome variables capture the absolute and relative loan supply, respectively, in the leveraged lending segment. If regulated lenders were to ration leveraged lending *per se* in reaction to the Clarification, the coefficient estimates for both these outcomes should be negative.

However, the estimates from these lender-level regressions indicate that, in both columns, the coefficient on $Post_t$ is not statistically different from zero. In particular, the coefficient on total loan supply is -0.039 with a standard error of 0.037. The coefficient on the fraction of leveraged lending is 0.567 with a standard error of 0.641. These economically small and statistically insignificant estimates provide no indication that regulated lenders rationed credit supply to leveraged borrowers after the Clarification.

6.3 External validity

The Clarification provides an interesting source of variation to study the link between loan covenants as non-price terms as well as competition between banks and non-banks. In a final set of tests, we investigate whether our findings are consistent with patterns that emerge in the broader syndicated loan market. To this end, we test whether banks in general can poach borrowers from competitors and gain market share by offering lax covenant packages. Table A.14 reports the estimation results from two analyses. In these tests, we find evidence that is consistent with a more general link between competition and lax non-price terms, as predicted in Lee & Mann (2021). On the bank level, we find that banks which reduce covenants gain market share compared to other banks at the

same point of time. Consistent with Fahlenbrach, Prilmeier & Stulz (2017) lenders relax contractual safeguards without extending credit to objectively worse borrowers. To complement these results, our last tests look into loan terms around borrowers' decision to switch lenders, similar to Ioannidou & Ongena (2010). We find that the first loan between a borrowers and a new lender exhibits fewer covenants, evidence consistent with lax non-price terms being used to gain market share.

7 Conclusion

Using the interagency regulation on leveraged lending in the U.S. as variation in banks' ability to offer covenant-lite loans, we show that loan covenants are an important dimension of lender competition. Following regulatory recommendations, regulated banking institutions slowed their issuance of covenant-lite loans relative to non-banks, effectively stopping what regulators had feared was a race to the bottom. As a result, banks subsequently lost market share to the advantage of non-bank institutions. In the aggregate, we find that the enhanced covenant protections of bank loans was crowded out by the increased participation of non-bank institutions in leveraged lending. These effects are concentrated among borrowers with low switching costs and among instances in which borrowers and lenders demonstrate a prior affinity to covenant-lite loan structures. While our empirical strategy draws inference from the $1.2 trillion leveraged loan sub-segment of the U.S. lending market, we provide reduced-form, large-sample evidence that banks gain market share by offering loans with fewer covenants.

Our results also serve as a clarion call to account for competition on non-price terms between the regulated and the non-regulated sectors when making regulatory decisions. It is also a call to "internalize the externalities that are generated in the shadow banking system" (Adrian & Shin, 2009). Thus, while the Clarification reasonably addressed a lack of covenant protections in the regulated banking sector, a portion of risky loans migrated out of the regulatory environment. We leave to future research the estimation of the overall welfare impact of the increase in shadow lending, which is one of the main consequences of the Clarification.

References

Acharya, V. V., Berger, A. N., & Roman, R. A. (2018). Lending implications of U.S. bank stress tests: Costs or benefits? *Journal of Financial Intermediation*, *34*, 58–90. (Cited on page 7.)

Acharya, V. V., Eisert, T., Eufinger, C., & Hirsch, C. (2018). Real effects of the sovereign debt crisis in europe: Evidence from syndicated loans. *The Review of Financial Studies*, *31*(8), 2855–2896. (Cited on page 7.)

Adler, K. (2020). Financial covenants, firm financing, and investment. *Firm Financing, and Investment (December 11, 2020).* (Cited on page 7.)

Adrian, T. & Shin, H. S. (2009). The Shadow Banking System: Implications For Financial Regulation. *FRB of New York Staff Report*, (382). (Cited on pages 1, 6, and 32.)

Aghion, P. & Bolton, P. (1992). An Incomplete Contracts Approach to Financial Contracting. *Review of Economic Studies*, *59*, 473–494. (Cited on page 1.)

Becker, B. & Ivashina, V. (2016). Covenant-Light Contracts and Creditor Coordination. *Working Paper.* (Cited on pages 3 and 15.)

Berg, T., Saunders, A., & Steffen, S. (2016). The total cost of corporate borrowing in the loan market: Don't ignore the fees. *Journal of Finance*, *71*(3), 1357–1392. (Cited on page 7.)

Berg, T., Saunders, A., Steffen, S., & Streitz, D. (2017). Mind the gap: The difference between US and European loan rates. *The Review of Financial Studies*, *30*(3), 948–987. (Cited on page 7.)

Berlin, M., Nini, G., & Edison, G. Y. (2020). Concentration of control rights in leveraged loan syndicates. *Journal of Financial Economics*, *137*(1), 249–271. (Cited on pages 6, 7, 16, 25, and xiv.)

Billett, M. T., Elkamhi, R., Popov, L., & Pungaliya, R. S. (2016). Bank Skin in the Game and Loan Contract Design: Evidence from Covenant-Lite Loans. *Journal of Financial and Quantitative Analysis*, *51*(3), 839–873. (Cited on page 15.)

Biswas, S., Ozkan, N., & Yin, J. (2019). Non-bank loans, corporate investment, and firm performance. *Working Paper.* (Cited on page 7.)

Buchak, G., Matvos, G., Piskorski, T., & Seru, A. (2018). Fintech, regulatory arbitrage, and the rise of shadow banks. *Journal of Financial Economics*, *130*(3), 453–483. (Cited on page 6.)

Callaway, B. & Sant Anna, P. (2020). Difference in differences with multiple time periods. *Journal of Econometrics.* (Cited on page 20.)

Chava, S. & Roberts, M. (2008). How Does Financing Impact Investment? The Role of Debt Covenants. *Journal of Finance*, *63*, 2085–2121. (Cited on pages 1, 7, and 16.)

Chernenko, S., Erel, I., & Prilmeier, R. (2020). Why do firms borrow directly from nonbanks? *Working Paper, National Bureau of Economic Research.* (Cited on pages 6 and 7.)

Chodorow-Reich, G. (2013). The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-09 Financial Crisis. *Quarterly Journal of Economics*, *129*(1), 1–59. (Cited on pages 8, 18, and 19.)

Chodorow-Reich, G. & Falato, A. (2018). The Loan Covenant Channel: How Bank Health Transmits to the Real Economy. *Working Paper*. (Cited on page 20.)

Davydiuk, T., Marchuk, T., & Rosen, S. (2020). Direct lending in the us middle market. *Working Paper*. (Cited on page 7.)

Degryse, H. & Ongena, S. (2005). Distance, Lending Relationships, and Competition. *Journal of Finance*, *60*(1), 231–266. (Cited on page 7.)

Degryse, H. & Van Cayseele, P. (2000). Relationship Lending Within a Bank-based System: Evidence from European Small Business Data. *Journal of Financial Intermediation*, *9*(1), 90–109. (Cited on page 7.)

Demiroglu, C. & James, C. M. (2010). The information content of bank loan covenants. *Review of Financial Studies*, *23*(10), 3700–3737. (Cited on page 7.)

Dichev, I. & Skinner, D. (2002). Large-Sample Evidence on the Debt Covenant Hypothesis. *Journal of Accounting Research*, *40*, 1091–1123. (Cited on page 1.)

Fahlenbrach, R., Prilmeier, R., & Stulz, R. (2017). Why Does Fast Loan Growth Predict Poor Performance for Banks? *Review of Financial Studies*, *31*, 1014–1063. (Cited on page 32.)

Falato, A. & Liang, N. (2016). Do creditor rights increase employment risk? evidence from loan covenants. *Journal of Finance*, *71*(6), 2545–2590. (Cited on page 7.)

Giannetti, M. & Saidi, F. (2019). Shock propagation and banking structure. *Review of Financial Studies*, *32*(7), 2499–2540. (Cited on page 7.)

Graham, J. (2022). Presidential Address: Corporate Finance and Reality. *Working Paper*. (Cited on pages 1 and 9.)

Grosse-Rueschkamp, B., Steffen, S., & Streitz, D. (2019). A capital structure channel of monetary policy. *Journal of Financial Economics*, *133*(2), 357–378. (Cited on page 7.)

Heider, F., Saidi, F., & Schepens, G. (2019). Life below zero: Bank lending under negative policy rates. *Review of Financial Studies*, *32*(10), 3728–3761. (Cited on page 7.)

Ioannidou, V. & Ongena, S. (2010). Time for a Change: Loan Conditions and Bank Behavior when Firms Switch Banks. *Journal of Finance*, *65*(5), 1847–1877. (Cited on page 32.)

Irani, R. M., Iyer, R., Meisenzahl, R. R., & Peydro, J.-L. (2021). The rise of shadow banking: Evidence from capital regulation. *The Review of Financial Studies*, *34*(5), 2181–2235. (Cited on pages 6 and 29.)

Ivashina, V. & Scharfstein, D. (2010). Bank Lending During the Financial Crisis of 2008. *Journal of Financial Economics*, *97*(3), 319–338. (Cited on pages 7 and 18.)

Jensen, M. & Meckling, W. (1976). Theory of The Firm: Managerial Behavior, Agency Costs and Ownership Structure. *Journal of Financial Economics*, *3*, 305–360. (Cited on page 1.)

Jiang, E., Matvos, G., Piskorski, T., & Seru, A. (2021). Banking without deposits: Evidence from shadow bank call reports. *Working Paper, National Bureau of Economic Research.* (Cited on page 6.)

Jiang, W., Li, K., & Shao, P. (2010). When shareholders are creditors: Effects of the simultaneous holding of equity and debt by non-commercial banking institutions. *Review of Financial Studies*, *23*(10), 3595–3637. (Cited on page 7.)

Jiménez, G., Ongena, S., Peydró, J.-L., & Saurina, J. (2012). Credit Supply and Monetary Policy: Identifying the Bank Balance-Sheet Channel with Loan Applications. *American Economic Review*, *102*(5), 2301–26. (Cited on page 4.)

Jiménez, G., Ongena, S., Peydró, J.-L., & Saurina, J. (2014). Hazardous Times for Monetary Policy: What Do Twenty-three Million Bank Loans Say about the Effects of Monetary Policy on Credit Risk-Taking? *Econometrica*, *82*(2), 463–505. (Cited on page 4.)

Khwaja, A. I. & Mian, A. (2008). Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market. *American Economic Review*, *98*(4), 1413–42. (Cited on page 4.)

Kim, S., Plosser, M., & Santos, J. (2018). Macroprudential Policy and the Revolving Door of Risk: Lessons from Leveraged Lending Guidance. *Journal of Financial Intermediation*, *34*, 17–31. (Cited on pages 13 and 14.)

Lee, S. & Mann, W. (2021). Competition and lending standards with multidimensional private information. *Working Paper.* (Cited on pages 2, 12, 21, and 31.)

Massoud, N., Nandy, D., Saunders, A., & Song, K. (2011). Do hedge funds trade on private information? evidence from syndicated lending and short-selling. *Journal of Financial Economics*, *99*(3), 477–499. (Cited on page 7.)

Matvos, G. (2013). Estimating the benefits of contractual completeness. *Review of Financial Studies*, *26*, 2798–2844. (Cited on pages 1, 2, 7, 8, 9, and 10.)

Murfin, J. (2012). The Supply-Side Determinants of Loan Contract Strictness. *Journal of Finance*, *67*, 1565–1601. (Cited on pages 1, 7, and 22.)

Neuhann, D. & Saidi, F. (2016). Bank deregulation and the rise of institutional lending. *University of Texas at Austin Working Paper.* (Cited on page 7.)

Nini, G., Smith, D., & Sufi, A. (2012). Creditor control rights, corporate governance, and firm value. *Review of Financial Studies*, *25*, 1713–1761. (Cited on page 7.)

Prilmeier, R. (2017). Why Do Loans Contain Covenants? Evidence From Lending Relationships. *Journal of Financial Economics*, *123*, 558–579. (Cited on page 7.)

Prilmeier, R. & Stulz, R. M. (2020). Securities laws, bank monitoring, and the choice between cov-lite loans and bonds for highly levered firms. *Fisher College of Business Working Paper.* (Cited on page 6.)

Roberts, M. & Sufi, A. (2009). Control Rights and Capital Structure: An Empirical Investigation. *Journal of Finance*, *64*, 1657–1695. (Cited on page 7.)

Roberts, M. R. & Schwert, M. (2020). Interest rates and the design of financial contracts. *Working Paper, National Bureau of Economic Research.* (Cited on page 24.)

Schenck, N. & Shi, L. (2022). Impact of leveraged lending guidance: evidence from nonbank participation in syndicated loans. *Journal of Financial Regulation and Compliance*, (ahead-of-print). (Cited on page 29.)

Schwert, M. (2017). Municipal bond liquidity and default risk. *Journal of Finance*, *72*(4), 1683–1722. (Cited on page 7.)

Smith, C. & Warner, J. (1979). On Financial Contracting: An Analysis of Bond Covenants. *Journal of Financial Economics*, *7*, 117–161. (Cited on pages 1, 3, and 9.)

Tan, G. & Tracy, R. (2015). Credit Suisse Loans Draw Fed Scrutiny. *Wall Street Journal.* (Cited on page 13.)

Tirole, J. (2006). *The Theory of Corporate Finance.* Princeton University Press, Princeton. (Cited on page 1.)

Tracy, R. (2015). Feds Win Fight Over Risky-Looking Loans. *Wall Street Journal.* (Cited on page 13.)

Webb, P. (2016). Leveraged Lending Guidance and Enforcement: Moving the Fulcrum. *NC Banking Inst.*, *20*, 91. (Cited on page 13.)

Figure 1
Parallel Trends for Non-Covenant-Lite Lending
Panel A plots the fraction of switchers for banks and non-banks over time. The vertical lines mark the quarter of the Clarification in the fourth quarter of 2014. Panel B of this figure plots dynamics of the fraction of non-covenant-lite lending originated to non-financial US borrowers. The blue line represents the fraction of non-covenant-lite lending by banks, while the gray line plots the same fraction for non-banks. Data on leveraged loans come from Thomson Reuters LPC database.
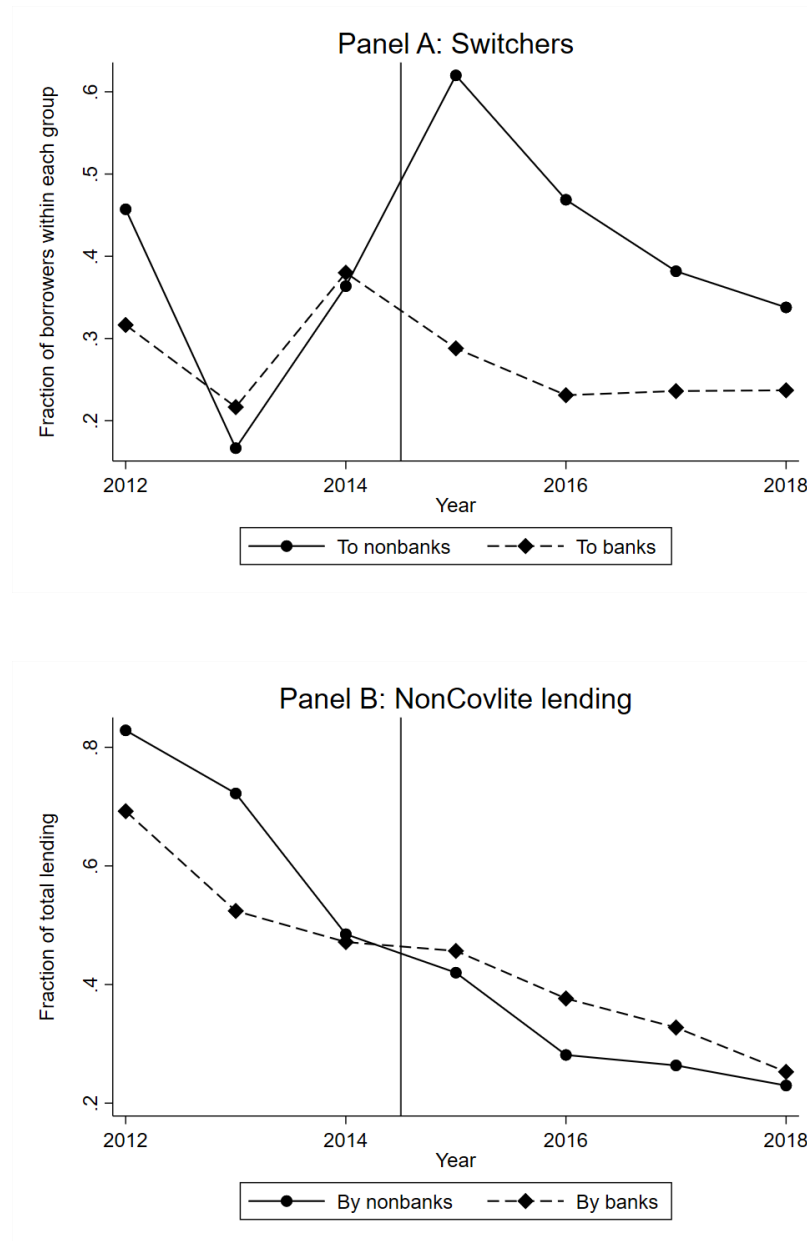
Figure 2
Treatment Effect Over Time

This figure provides a graphical illustration of the treatment dynamics in our difference in differences specification on non-covenant-lite loan offers. The graph plots the development of the coefficients on the indicator *Bank Borrower* interacted with an indicator for each quarter from 2012 to 2018. The reference quarter is 2012q1. We estimate the most stringent specification (column 4) of Table 4. The dependent variable is the likelihood of a loan offer that is not covenant-lite. The red line marks the passage of the Clarification in the fourth quarter of 2014. Vertical bars present 95% confidence intervals.
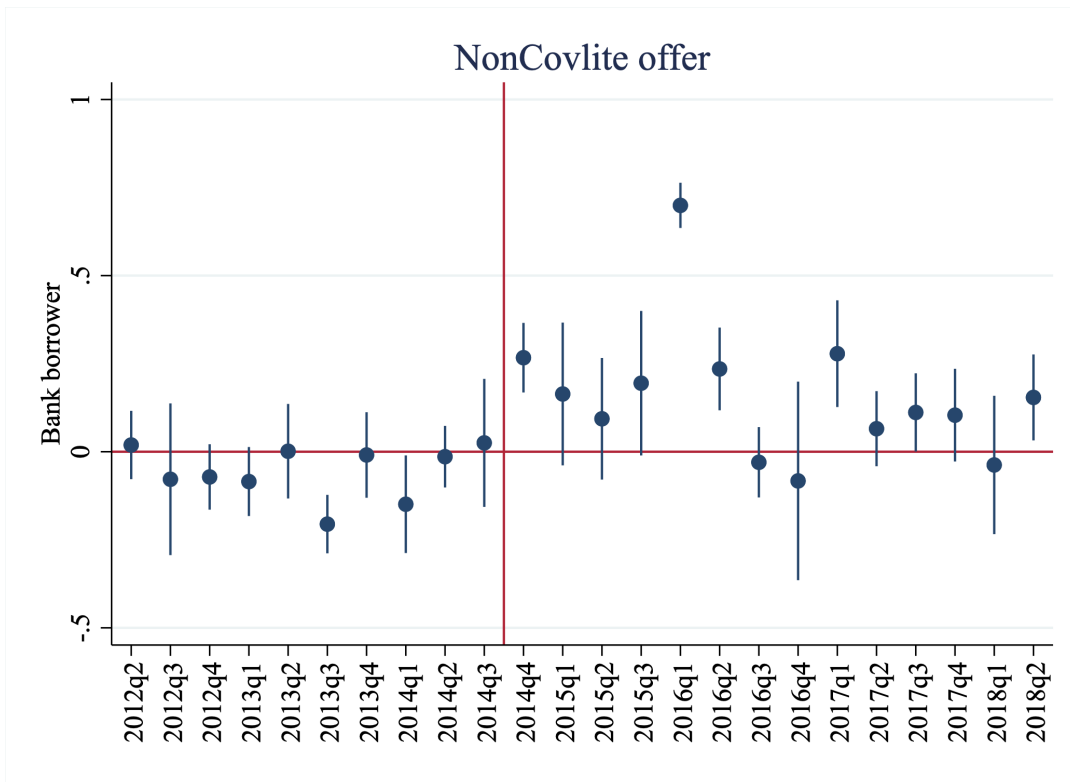
Table 1
Summary statistics

The table reports descriptive statistics for our sample of leveraged loans from LCD. The sample period spans 2012 to 2018. Panel A refers to the full sample, Panel B contrasts bank and non-bank institutions, Panel C contrasts covenant-lite and non-covenant-lite loans. Deal Size denotes the size of the loan in USD bn. Interest is the loan interest rate; Maturity the loan maturity in years; Collateral is an indicator for the loan being asset backed; Rated in an indicator for the loan being rated; New Lender is an indicator for new lenders. CovLite is an indicator variable for covenant-lite loans. Bank Relationship is an indicator for the borrower having a banking relationship at the time of loan origination. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

Panel A: Full Sample

	Mean	St. Dev.	Min	Max	NObs
Deal Size ($ bln)	0.64	0.77	0.01	12.35	4460
Interest	4.08	1.81	0.33	15.00	4460
Collateral	0.02	0.14	0.00	1.00	4460
Maturity	5.82	1.21	0.50	10.00	4460
Rated	0.96	0.20	0.00	1.00	4460
New Lender	0.28	0.45	0.00	1.00	4460
CovLite	0.56	0.50	0.00	1.00	4460
Bank Relationship	0.90	0.30	0.00	1.00	4460

Panel B: Banks vs non-banks

	Banks					non-banks				
	Mean	St. Dev.	Min	Max	NObs	Mean	St. Dev.	Min	Max	NObs
Deal Size ($ bln)	0.67	0.80	0.01	12.35	4039	0.35	0.41	0.01	2.40	421
Interest	4.00	1.78	0.33	15.00	4039	4.86	1.87	2.25	12.00	421
Collateral	0.02	0.14	0.00	1.00	4039	0.01	0.11	0.00	1.00	421
Maturity	5.81	1.20	0.70	10.00	4039	5.87	1.23	0.50	8.50	421
Rated	0.96	0.19	0.00	1.00	4039	0.93	0.25	0.00	1.00	421
New Lender	0.27	0.44	0.00	1.00	4039	0.38	0.49	0.00	1.00	421
CovLite	0.56	0.50	0.00	1.00	4039	0.58	0.49	0.00	1.00	421
Bank Relationship	0.96	0.20	0.00	1.00	4039	0.37	0.48	0.00	1.00	421

Panel C: Covenant-Lite vs Non-Covenant-Lite

	Covenant-Lite					Non-Covenant-Lite				
	Mean	St. Dev.	Min	Max	NObs	Mean	St. Dev.	Min	Max	NObs
Deal Size ($ bln)	0.71	0.75	0.01	7.60	2515	0.55	0.79	0.01	12.35	1945
Interest	4.10	1.73	1.75	12.00	2515	4.06	1.90	0.33	15.00	1945
Collateral	0.00	0.00	0.00	0.00	2515	0.05	0.21	0.00	1.00	1945
Maturity	6.18	1.14	0.70	8.50	2515	5.36	1.13	0.50	10.00	1945
Rated	0.98	0.13	0.00	1.00	2515	0.92	0.26	0.00	1.00	1945
New Lender	0.26	0.44	0.00	1.00	2515	0.30	0.46	0.00	1.00	1945
Bank Relationship	0.90	0.30	0.00	1.00	2515	0.90	0.29	0.00	1.00	1945

Table 2
Facts: non-parametric evidence

The table presents non-parametric evidence before and after the Clarification (2014Q4). Panel A reports the frequency of transitions from bank to non-bank lenders (and vice versa), Panel B tabulates the fraction of covenant-lite loans corresponding to the transitions from bank to non-bank lenders (and vice versa). Both panels report test on differences between the periods before and after the Clarification. T-test statistics are in parentheses. All statistics are based on our sample of leveraged loans from the LCD dataset. The sample period is from 2012 to 2018. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

Panel A. Frequency of Transitions

	Before the Clarification		After the Clarification		Changes in Transition Frequencies	
From	To: Bank	Non-bank	To: Bank	Non-bank	To: Bank	Non-bank
Bank	96.7%	3.3%	94.9%	5.1%		1.9%*** (-4.38)
Non-bank	47.3%	52.7%	35.8%	64.2%	-11.5%*** (3.40)	

Panel B. Fraction of CovLite Lending when switching lenders

	Before the Clarification		After the Clarification		Changes in CovLite Fraction	
From	To: Bank	Non-bank	To: Bank	Non-bank	To: Bank	Non-bank
Bank	19.8%	11.9%	56.9%	73.6%	37.1%*** (21.01)	61.7%*** (14.33)
Non-bank	35.7%	5.6%	57.2%	62.8%	21.5%*** (3.8)	57.1%*** (4.6)

41

Table 3
Probability to switch lenders after the Clarification

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching to a new lender. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

				New Lender				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Bank Borrower	0.140**	0.136**	0.216***	0.220***	0.179**	0.182**	0.235**	0.371***
	(0.071)	(0.069)	(0.082)	(0.081)	(0.086)	(0.086)	(0.092)	(0.117)
Bank Borrower	-0.174***	-0.163***	-0.182***	-0.177***	-0.134*	-0.138*	-0.150*	-0.279***
	(0.057)	(0.055)	(0.064)	(0.064)	(0.078)	(0.078)	(0.079)	(0.083)
Post	-0.159**							
	(0.068)							
Borrower FE	No	No	No	No	Yes	Yes	Yes	Yes
Time FE	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Lender FE	No	No	Yes	Yes	No	No	Yes	No
Lender × Time FE	No	No	No	No	No	No	No	Yes
Loan Purpose FE	No	No	No	No	Yes	Yes	Yes	Yes
Rating FE	No	No	No	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	No	No	No	No
Obs	4460	4460	4460	4460	4460	4460	4460	4460
R^2	0.01	0.04	0.08	0.10	0.51	0.51	0.54	0.64

Table 4
Clarification and probability of getting a non-covlite loan offer from relationship lender

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of being offered a covenant-lite loan. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. NonCovlite $offer_{b,l,t}$ estimates the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

| | | | | NonCovlite offer | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Bank Borrower	0.216***	0.214***	0.176***	0.175***	0.173***	0.170***	0.162***	0.193***
	(0.022)	(0.020)	(0.021)	(0.021)	(0.026)	(0.025)	(0.025)	(0.031)
Bank Borrower	-0.190***	-0.197***	-0.180***	-0.178***	-0.186***	-0.185***	-0.180***	-0.217***
	(0.017)	(0.015)	(0.017)	(0.017)	(0.023)	(0.022)	(0.022)	(0.021)
Post	-0.385***							
	(0.021)							
Borrower FE	No	No	No	No	Yes	Yes	Yes	Yes
Time FE	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Lender FE	No	No	Yes	Yes	No	No	Yes	No
Lender × Time FE	No	No	No	No	No	No	No	Yes
Loan Purpose FE	No	No	No	No	Yes	Yes	Yes	Yes
Rating FE	No	No	No	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	No	No	No	No
Obs	4460	4460	4460	4460	4460	4460	4460	4460
R²	0.26	0.46	0.55	0.56	0.74	0.74	0.75	0.88

Table 5

Effects for different sub-segments of leveraged lending market

The table presents difference-in-differences estimates for different sub-segments. Column 1 analyzes switching to any new lender Any, Column 2 - to lenders offering covlite loans, Column 3 - to lenders offering non-covenant-lite loans. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and quarter and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New lender defined as:		
	(1) Any new lender	(2) Only CovLite	(3) Only NonCovLite
Post × Bank Borrower	0.371***	0.589***	0.216
	(0.117)	(0.126)	(0.136)
Bank Borrower	-0.279***	-0.378***	-0.203**
	(0.083)	(0.099)	(0.101)
Borrower FE	Yes	Yes	Yes
Time FE	No	No	No
Lender FE	No	No	No
Lender × Time FE	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes
Obs	4460	3762	3718
R^2	0.64	0.64	0.62

Table 6

Changes in other loan terms offered

This table reports difference-in-differences estimates of the effect of the Leverage Lending Clarification (2014Q4) on offered loan characteristics other than covenants. Columns 1, 2, 3 and 4 represent results for interest rate, maturity, collateral and deal size, respectively. All variables are defined in Appendix Table A.1 and, as in our main specification, reflect average loan terms offered by banks. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Parentheses contain standard errors clustered by borrower and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	Interest	Maturity	Collateral	Deal Size
	(1)	(2)	(3)	(4)
Post $\times$ Bank Borrower	-0.493***	-0.199***	0.019**	-0.016
	(0.139)	(0.074)	(0.009)	(0.037)
Bank Borrower	-0.401***	0.036	0.006	0.275***
	(0.122)	(0.067)	(0.008)	(0.026)
Borrower FE	Yes	Yes	Yes	Yes
Lender $\times$ Time FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Obs	4453	4460	4460	4460
R^2	0.82	0.80	0.74	0.85

Table 7

Borrower reaction – extensive and intensive margin

The table presents results from OLS regressions of borrower reactions to changed loan terms on firm and loan characteristics. The unit of observation is a single observation per bank-borrower on their last loan prior to the Clarification. The outcome variable in columns 1 to 3 is an indicator whether borrowers that previously took out leveraged loans stop taking out any more loans after the Clarification. The outcome variable in columns 4 to 6 is an indicator whether, conditional on staying in the leveraged loan market, a borrower switches from a regulated lender to a non-bank lender. Parentheses contain standard errors clustered by industry and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	Borrowers leaving the leveraged lending market			Borrowers switching to nonbanks		
	(1)	(2)	(3)	(4)	(5)	(6)
Credit rating	0.053 ***			0.020**		
	(0.013)			(0.007)		
Number of previous loans		0.029**			0.010*	
		(0.011)			(0.006)	
Deal size			−0.052 ***			−0.011**
			(0.017)			(0.005)
Interest			0.031**			0.006
			(0.012)			(0.006)
Collateral			0.232*			−0.046**
			(0.125)			(0.021)
Maturity			0.019			−0.002
			(0.015)			(0.007)
Covlite			−0.024			0.007
			(0.046)			(0.020)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	713	713	713	665	665	665
R^2	0.08	0.07	0.11	0.07	0.06	0.07

Table 8

Historically revealed preferences for covenant lite loans

The table presents difference-in-differences estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender l since the beginning of our sample, on $Post \times Bank\ Borrower$. Column 1 investigates the cross sectional impact for borrowers who's last loan prior to the clarification was covenant lite, by interacting the $Post \times Bank\ Borrower$ with the indicator *CovLite Demand*. Column 2 does the same using *CovLite Supply*, an indicator for lenders who have issued above median fraction of Covenant Lite loans in the 5 years prior to the Clarification. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender	
	(1)	(2)
Bank Borrower	-0.263***	-0.422***
	(0.084)	(0.107)
Post × Bank Borrower	0.373***	0.459***
	(0.116)	(0.139)
Post × Covlite Demand	-0.178**	
	(0.070)	
Bank Borrower × Covlite Demand	-0.092	
	(0.062)	
Post × Bank Borrower × Covlite Demand	0.197***	
	(0.072)	
Covlite Supply		0.192***
		(0.061)
Post × Covlite Supply		-0.251***
		(0.070)
Bank Borrower × Covlite Supply		-0.157**
		(0.063)
Post × Bank Borrower × Covlite Supply		0.238***
		(0.073)
Borrower FE	Yes	Yes
Lender × Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Rating FE	Yes	Yes
Obs	4460	4460
R^2	0.64	0.64

Table 9
Effect of covenant-lite lending on bank-level market share
The table presents difference-in-difference estimates of the effect of the Leverage Lending Clarification (2014Q4) on covenant-lite loan offers and lender market share in a given quarter. *Bank* is an indicator for regulated lenders. All other variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018, aggregated on the lender-quarter level. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include lender-quarter average interest rate, maturity, loan rating, and indicators for secured loans. Standard errors clustered by lender and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	Covlite lending		Market Share	
	(1)	(2)	(3)	(4)
Post × Bank	-0.148***	-0.145***	-1.215*	-1.210*
	(0.051)	(0.050)	(0.647)	(0.637)
Lender FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Obs	626	626	626	626
R^2	0.59	0.62	0.81	0.82

Table 10

Alternative channels - changes in type of borrowers and loan supply

The table reports results on the impact of the Clarification on lending via alternative channels. Panel A studies the credit risk profiles of borrowers post-Clarification. The dependent variable in Column 1 is the numerical credit rating, where 1 corresponds to the best rating of AAA. The dependent variable in Column 2 is an indicator *1(Investment Grade)* which is equal to one if the borrower has a investment grade rating. Panel B explores changes in Debt to EBITDA ratios of borrowers, both in terms of the continuous ratios (Column 1) as well as an indicator for the most highly leveraged borrowers with Debt to EBITDA larger than 6, *1(Debt to EBITDA>6)* (column 2). The regressions in both Panel A and B are on the borrower level.

Panel C reports the leveraged lending dynamics of regulated banks on the bank level. The dependent variable in Column 1 is the log of leveraged lending defined at bank and quarter level. The dependent variable in Column 2 is the fraction of leveraged lending (LCD) to the total lending (Dealscan). All variables are defined in Appendix Table A.1. The sample period is 2012 through 2018. Parentheses contain standard errors clustered by borrower and quarter (Panels A and B) or clustered by lender (Panel C), and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	Panel A: Borrowers' Risk Profile	
	Credit Rating (continuous)	1(Investment Grade)
	(1)	(2)
Post $\times$ Bank Borrower	-0.072	0.004
	(0.103)	(0.005)
Bank Borrower	0.040	-0.006
	(0.096)	(0.005)
Borrower FE	Yes	Yes
Lender x Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Obs	4460	4460
R^2	0.84	0.85

Panel B: Debt to EBITDA ratio		
	Debt To EBITDA	1(Debt To EBITDA>6)
	(1)	(2)
Post × Bank Borrower	0.094	-0.575
	(0.233)	(0.365)
Bank Borrower	-0.122	-0.331**
	(0.122)	(0.146)
Borrower FE	Yes	Yes
Lender × Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Rating FE	Yes	Yes
Obs	1415	1415
R^2	0.88	0.86
Panel C: Leveraged Lending Dynamics of Regulated Banks		
	Leveraged supply (total)	Leveraged Loans to Total Lending (%)
	(1)	(2)
Post	-0.039	0.567
	(0.037)	(0.641)
Lender FE	Yes	Yes
Controls	Yes	Yes
Obs	498	406
R^2	0.80	0.14

Appendix

Figure A.1
Dynamics of Borrower Credit Ratings in the Leveraged Loan Market
The figure depicts the evolution of average borrower credit ratings in leveraged loan agreements originated to non-financial US borrowers. The blue line represents the average credit rating across all bank borrowers. Data on leveraged loans come from Thomson Reuters LPC database.
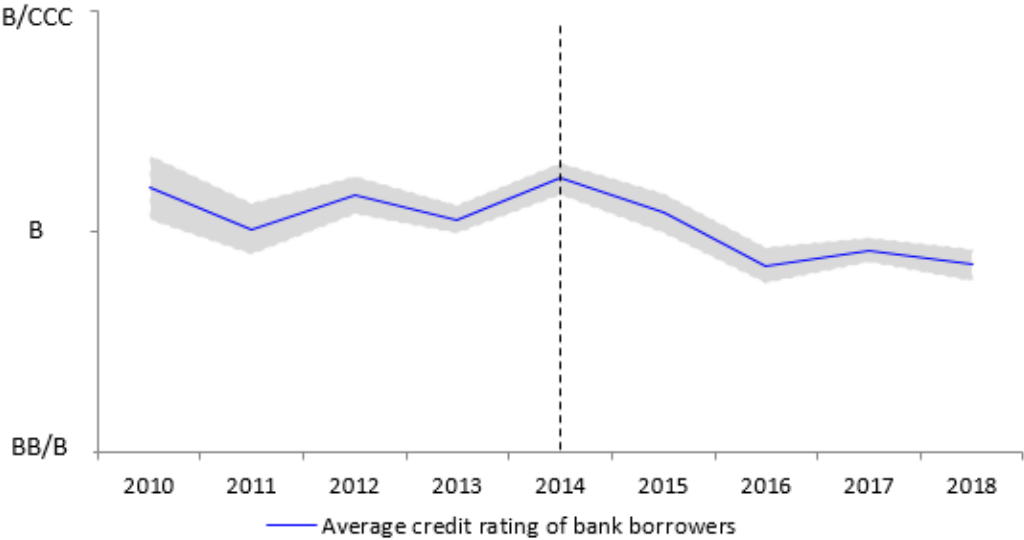
Table A.1
Variable Definitions

Variable	Definition
Bank Borrower	Indicator equal to one for borrowers who's last previous loan was issued by US and foreign banks supervised by Federal Reserve, OCC, and FDIC and zero in all other cases
Collateral	Average fraction of the deals secured with collateral and originated by lender in a given quarter weighted by the amount of loan
CovLite Lending	Dollar amount of covenant-lite loans originated by lender / Total dollar amount of covlite loans originated in the loan market in a given quarter
Covlite and NonCovlite loans	Borrower had both covenant-lite and non-covenant-lite loans as of loan origination
Covlite loans only	Borrower had only covenant-lite loans as of default date
Debt to EBITDA	Borrower debt divided by EBITDA
1(Debt to EBITDA>6)	Indicator if borrower debt divided by EBITDA exceeds 6, a level of particular attention in the Leveraged Lending Clarification
High Covlite Share	Indicator if lender issued an above median fraction of covenant lite loans prior to the Clarification
Interest	Average all-in-drawn spread of the deals originated by lender in a given quarter weighted by the amount of loan
Last loan pre-Clarification CovLite	Indicator if borrower's last loan prior to the clarification was covenant-lite

Market share	Dollar amount of loans originated by lender / Total dollar amount of loans originated in the loan market in a given quarter
Maturity	Average maturity of the deals originated by lender in a given quarter weighted by the amount of loan
Non-Covlite Offer	The likelihood that a borrower's relationship lender would have offered a non-covenant-lite loan, measured as the fraction of non-covenant-lite loans given out by this lender to all other borrowers (if any) in the year in which the borrower takes out their loan
Number of interactions	the number of loan agreements between borrower and lender prior to the Clarification
Post	Indicator equal to one following the issuance of the Clarification in 2014q4 and zero otherwise
Relationship	Indicator equal to one if a specific borrower have taken at least two loans in the past from a particular lender and zero otherwise
Relationship duration	The number of years between two loan agreements in a given borrower and lender pair

Table A.2
Excerpts from the Leveraged Lending Guidance (2013): Leveraged Loans

#	Definitions of leveraged loan
1	Proceeds used for buyouts, acquisitions, or capital distributions.
2	Transactions where the borrower's Total Debt divided by EBITDA (earnings before interest, taxes, depreciation, and amortization) or Senior Debt divided by EBITDA exceed 4.0X EBITDA or 3.0X EBITDA, respectively, or other defined levels appropriate to the industry or sector.
3	A borrower recognized in the debt markets as a highly leveraged firm, which is characterized by a high debt-to-net-worth ratio.
4	Transactions when the borrower's post-financing leverage, as measured by its leverage ratios (for example, debt-to-assets, debt-to-net-worth, debt-to-cash flow, or other similar standards common to particular industries or sectors), significantly exceeds industry norms or historical levels.

Table A.3
Top-15 lead agents in covenant-lite loan market
The table presents league tables for Top-15 lead agents in the covenant-lite loan market before (Panel A) and after (Panel B) the Clarification (2014Q4).

Panel A. Before the Clarification (2012q4-2014q3)

#	Lead Agent	N	%	mln
1	Credit Suisse	191	18.82	109.9
2	Bank of America	168	16.55	113.5
3	JP Morgan Chase	113	11.13	101.4
4	Deutsche Bank	102	10.05	71.6
5	Morgan Stanley	79	7.78	37.8
6	Barclays Bank	68	6.7	42.5
7	Goldman Sachs	60	5.91	31.7
8	Citigroup	59	5.81	44.2
9	UBS AG	44	4.33	17.5
10	**Jefferies Finance**	43	4.24	15.8
11	Royal Bank of Canada	40	3.94	9.6
12	Wells Fargo	16	1.58	7.1
13	**General Electric Capital Corp.**	8	0.79	2.7
14	SunTrust Bank	7	0.69	1.6
15	BNP Paribas Group	3	0.3	0.9

Panel B. After the Clarification (2014q4-2016q3)

#	Lead Agent	N	%	mln
1	Credit Suisse	108	15.77	68.3
2	Bank of America	83	12.12	54.1
3	JP Morgan Chase	82	11.97	81.9
4	Goldman Sachs	56	8.18	29.4
5	Morgan Stanley	54	7.88	38.4
6	Deutsche Bank	51	7.45	34.7
7	**Jefferies Finance**	45	6.57	18.2
8	Citigroup	43	6.28	45.7
9	Barclays Bank	39	5.69	30.6
10	UBS AG	32	4.67	9.1
11	Royal Bank of Canada	24	3.5	8.3
12	**General Electric Capital Corp**	15	2.19	4.5
13	Wells Fargo	11	1.61	4.0
14	**Antares Leveraged Capital Corp**	9	1.31	1.9
15	**Macquarie Capital**	6	0.88	1.1

Table A.4

Alternative estimation method: second stage instrumented non-covenant-lite loan offers and switching lenders

The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender l since the beginning of our sample, on the instrumented $\widehat{NonCovLiteOffer}_{b,l,t}$ from the first stage. For ease of exposition, NonCovLite Offer is standardized in this regression, meaning the coefficient can be interpreted as the effect of a one standard deviation increase in the explanatory variable. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	0.204**	0.212**	0.286***	0.380***
	(0.092)	(0.093)	(0.104)	(0.126)
Bank Borrower	0.059	0.060	0.111*	0.140
	(0.062)	(0.062)	(0.067)	(0.096)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
Kleibergen-Paap rk Wald F	46.07	45.86	43.08	37.62

Table A.5

Varying event windows

The table presents diff-in-diff estimates of the effect of the Leverage Lending Clarification (2014Q4) on borrowers switching lenders. We estimate the most complete specification from our main analysis (Table A.4, Column 4) using a 1, 2, 3, and 4 year symmetric windows around the Clarification. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the respective period. *Bank borrower* is an indicator for loans taken out by borrowers who's previous loan was from a regulated lender. Controls include interest rate, maturity, and indicators for secured loans. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	1y	2y	3y	4y
	(1)	(2)	(3)	(4)
Post × Bank Borrower	1.094***	0.341*	0.306**	0.322***
	(0.331)	(0.180)	(0.134)	(0.112)
Bank Borrower	-0.562***	-0.278**	-0.191**	-0.244***
	(0.214)	(0.112)	(0.091)	(0.079)
Borrower FE	Yes	Yes	Yes	Yes
Lender × Time FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Obs	1023	2288	3731	4885
R2	0.91	0.75	0.66	0.62

Table A.6

Placebo: probability to switch lenders before the Clarification

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching between lenders. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2010 to 2014. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Bank Borrower × Post 2010q4	0.060			
	(0.366)			
Bank Borrower × Post 2011q4		0.220		
		(0.169)		
Bank Borrower × Post 2012q4			-0.001	
			(0.172)	
Bank Borrower × Post 2013q4				-0.114
				(0.216)
Bank Borrower	-0.315	-0.432***	-0.256*	-0.237**
	(0.358)	(0.148)	(0.136)	(0.115)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Lender FE	No	No	Yes	Yes
Lender × Time FE	No	No	No	No
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Obs	2402	2402	2402	2402
R^2	0.70	0.70	0.70	0.70

Table A.7
Robustness: exclusion of 2014q3

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching to a new lender. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018, while leaving the 3rd quarter of 2013 out of the sample (which was marked by the issuance of the MRIA letters to Credit Suisse and other banks). Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.192**	0.194**	0.234**	0.360***
	(0.093)	(0.093)	(0.098)	(0.119)
Bank Borrower	-0.140*	-0.145*	-0.143*	-0.273***
	(0.081)	(0.081)	(0.082)	(0.087)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Obs	4300	4300	4300	4300
R^2	0.51	0.51	0.54	0.64

Table A.8

Robustness: Table 3 with controls

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching to a new lender. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.182**	0.178**	0.231**	0.367***
	(0.086)	(0.087)	(0.093)	(0.118)
Bank Borrower	-0.138*	-0.132*	-0.145*	-0.274***
	(0.078)	(0.078)	(0.079)	(0.084)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Industry	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R2	0.51	0.51	0.54	0.64

Table A.9
Robustness: Table 4 with controls

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of being offered a covenant-lite loan. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	NonCovlite offer			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.173***	0.169***	0.162***	0.193***
	(0.025)	(0.025)	(0.025)	(0.031)
Bank Borrower	-0.186***	-0.185***	-0.180***	-0.218***
	(0.023)	(0.022)	(0.022)	(0.021)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R^2	0.74	0.74	0.75	0.88

Table A.10

Timing: effect of the *Initial Guidance* on covenant-lite lending

The table presents difference-in-differences estimates of how the Leverage Lending Guidance (2013Q2) rather than the Clarification (2014Q4) affected the chance of being offered a covenant-lite loan. All variables are defined in Appendix Table A.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. $NonCovliteOffer_{b,l,t}$ estimates the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t. Controls include loan purpose, loan type and credit rating. standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	NonCovlite loans			
	(1)	(2)	(3)	(4)
Post Guidance× Bank Borrower	0.022	0.020	0.022	-0.015
	(0.029)	(0.028)	(0.029)	(0.040)
Post Clarification × Bank Borrower	0.165***	0.162***	0.154***	0.199***
	(0.029)	(0.028)	(0.028)	(0.037)
Bank Borrower	-0.200***	-0.197***	-0.194***	-0.209***
	(0.026)	(0.025)	(0.026)	(0.028)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R^2	0.74	0.74	0.75	0.88

Table A.11

Timing: effect of the *Initial Guidance* on the probability to switch lenders

The table presents second-stage 2SLS estimates of how the Leverage Lending Guidance (2013Q2) rather than the Clarification (2014Q4) affected the probability to switch lenders. All variables are defined in Appendix Table A.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. $NonCovliteOffer_{b,l,t}$ estimates the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post Guidance× Bank Borrower	-0.034	-0.029	-0.052	-0.063
	(0.114)	(0.111)	(0.115)	(0.164)
Post Clarification × Bank Borrower	0.192**	0.193**	0.254**	0.399***
	(0.097)	(0.096)	(0.102)	(0.139)
Bank Borrower	-0.113	-0.120	-0.118	-0.244**
	(0.106)	(0.105)	(0.110)	(0.122)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R^2	0.51	0.51	0.54	0.64

Table A.12
Robustness: excluding loans with split control rights

The table presents the same second-stage 2SLS estimates of the impact of the Clarification (2014Q4) as in Table A.4, excluding all loans which have an associated revolving facility that features covenants, i.e. cases of "split control rights" (Berlin et al., 2020). All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

| | New Lender | | | |
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.168***	0.163***	0.143***	0.177***
	(0.031)	(0.031)	(0.031)	(0.043)
Bank Borrower	-0.203***	-0.199***	-0.185***	-0.203***
	(0.028)	(0.027)	(0.027)	(0.029)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Industry	No	No	No	No
Obs	3366	3366	3366	3366
R^2	0.76	0.76	0.77	0.89

Table A.13

Effects for different sub-segments of leveraged lending market: other loan terms

The table presents diff-in-diff estimates for different sub-segments. Column 1 (2) analyze switching to any new lender with above (below) average interest rate, Column 3 (4) - to lenders offering loans with above (below) average maturity, Column 5 (6) - to lenders offering loans with above (below) average collateral requirement, Column 7 (8) - to lenders offering loans with above (below) average loan size. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers who's last previous loan was with a Commercial US bank. Controls include loan purpose, loan type and credit rating. Standard errors clustered by borrower and quarter and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender							
	High rate	Low rate	High maturity	Low maturity	High collateral	Low collateral	High size	Low size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Bank Borrower	0.238**	0.133*	0.272***	0.100*	-0.003	0.374***	0.200***	0.172*
	(0.100)	(0.068)	(0.098)	(0.060)	(0.018)	(0.115)	(0.066)	(0.098)
Bank Borrower	-0.149**	-0.130**	-0.233***	-0.046	-0.013	-0.266***	-0.132***	-0.146*
	(0.075)	(0.051)	(0.074)	(0.043)	(0.015)	(0.083)	(0.049)	(0.078)
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	No	No	No	No
Lender FE	No	No	No	No	No	No	No	No
Lender × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460	4460	4460	4460	4460
R^2	0.61	0.47	0.50	0.40	0.41	0.62	0.39	0.54

Table A.14

External validity: covenants, bank market share and switching lenders in the broader loan market

The table presents regressions of the relationship between loan characteristics, bank market share, and the borrower's decision to switch lenders. The dependent variable in Columns 1-2 is the bank's total market share (in percentages) in a given quarter, the dependent variable in Columns 3-4 is 1(Loan from new lenders) that takes the value of 1 for loans which are the first between a borrower and a lender. The explanatory variables in Columns 1-2 are bank-quarter averages of the number of covenants, average loan maturity, average interest rate, and average borrower rating. The explanatory variables in Columns 3-4 are various loan and borrower characteristics, such as the number of covenants in the loan contract, loan maturity, loan interest rate, and the borrower's rating. These regressions therefore present a comparison of loans made by new lenders compared to those in existing lending relationships. All variables are defined in Appendix Table A.1. The sample period is 1995 through 2017 and includes all loans to non-financial borrowers from LPC DealScan. Variables are aggregated to the lender-quarter level in columns 1 and 2, and on the loan level in columns 3 and 4. Parentheses contain standard errors clustered by lender and quarter (columns 1 and 2) and borrower and quarter (columns 3 and 4), and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	Bank Level: Market Share		Loan Level: 1(Loan from new lenders)	
	(1)	(2)	(3)	(4)
Number of covenants	−0.365 ***	−0.250 ***	−0.019**	−0.023 ***
	(0.094)	(0.093)	(0.009)	(0.009)
Maturity		0.172*		0.003 ***
		(0.094)		(0.001)
Interest		−0.105*		0.012
		(0.055)		(0.008)
Rating		−0.096 ***		0.001
		(0.019)		(0.005)
Lender FE	Yes	Yes	No	No
Borrower FE	No	No	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes
Observations	4,521	4,521	17,297	17,297
R^2	0.64	0.64	0.36	0.36