

Do Banks Compete on Non-Price Terms?

Evidence from Loan Covenants

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Abstract

We study the interplay between non-price loan terms and competition in credit markets. We exploit a regulatory shock to regulated banks' ability to offer favorable non-price terms, particularly covenant-lite loans. We find that borrowers trade-off increased covenants and lower interest rates from regulated banks, with covenant-lite loans and higher rates from non-banks. This non-price competition alters market structure: less covenant-sensitive borrowers remain with regulated lenders, and financially weaker borrowers switch to shadow banks or leave the leveraged lending market. As a result, banks' market share declines. Our findings on borrower behavior and loan terms align with a stylized equilibrium model.

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

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1 Introduction

Loan contracts consist of price terms, such as interest rates and fees, and non-price terms, such as covenants and collateral requirements. While a large literature has documented the effect of competition between lenders on price terms, such as interest rates, the design of non-price terms on differentiating lending contracts remains underexplored. In an effort to gain market share, lenders may offer more favorable prices or relax non-price terms. Non-price terms are widely acknowledged as an important dimension of loan contracts (e.g., [Jensen & Meckling 1976](#); [Smith & Warner 1979](#); [Rajan & Winton 1995](#); [Matvos 2013](#)) and relaxing them can potentially give lenders an angle to beat their competition. However, studying competition between lenders with respect to non-price terms is challenging because all loan terms are jointly determined ([Roberts & Schwert, 2022](#)). Our study leverages a regulatory shock that limits lenders' ability to offer certain non-price terms, but not price terms, to gain insight into the role of non-price terms for competition in credit markets.

We exploit the 2014 Leveraged Lending Clarification (“the Clarification”) issued by U.S. regulators as a shock that restricts regulated banks, but not shadow banks, from offering lenient non-price terms ([Murfin, 2012](#)). By tracking how borrowers respond to the altered competitive environment, we can assess the role of non-price terms in competition. This empirical strategy is crucial to determine whether lenders compete on non-price terms. A simple correlation between a banks' loan growth and lax non-price terms cannot be reliably interpreted as evidence of non-price competition in credit markets. For instance, banks that hold optimistic views about the economic outlook may both increase credit and weaken covenant protection, leading to a statistical association between lax non price terms and lender growth that is not necessarily the outcome of non-price competition. Our paper exploits a shock to non-price terms that was limited to a subset of the market to study how lenders adjust other loan dimensions, and how borrowers react to this change in the loan term menu.

The Clarification places particular emphasis on one dimension of non-price terms, covenant-lite loan structures. Covenants, a salient non-price term, regulate corporate policies through triggers based on financial metrics ([Chava & Roberts, 2008](#)). Regulators were concerned about competition between lenders leading to a “race to the bottom” in covenant protection, as modeled in [Lee &](#)

Mann (2021). To counter such tendencies, the Clarification promotes both qualitative and quantitative measures to strengthen covenant protection in the \$1.4 trillion leveraged loan segment of the syndicated loan market.¹ Importantly, the regulation applies only to regulated lenders, such as commercial banks, but not shadow banks, for example, financing companies. We trace the response by lenders and borrowers to this shock using S&P’s Leveraged Commentary and Data (LCD), which offers comprehensive data on U.S. leveraged loans and their covenant protection.²

We motivate key thrusts of our empirical analysis through a stylized model along the lines of Matvos (2013). The model embeds a trade-off where strict non-price terms limit firms’ financial flexibility (e.g., Graham, 2022) but improve credit availability and pricing. We further assume that borrowers exhibit a preference for relationship lenders (e.g., Chodorow-Reich, 2013), and that borrowers are heterogeneous, valuing financial flexibility and relationships differently. Restrictions on the relationship lender’s ability to offer covenant-lite loans then lead to two outcomes. “Stayers” remain with their relationship bank, while “switchers” opt for unrestricted lenders. The model further predicts a balance between price and non-price terms following the initial shock, where “stayers” receive lower interest rates in exchange for accepting stricter non-price terms.

Motivated by this theoretical framework, we turn to the data. We first verify that borrowers’ behavior aligns with non-price competition. Following the drop in banks’ ability to offer covenant-lite loans, borrowers turn to unaffected lenders for covenant-lite loans, consistent with earlier work documenting a general shift in lending towards shadow banks surrounding the Clarification (e.g. Kim, Plosser & Santos, 2018; Calem, Correa & Lee, 2020). We then provide evidence of a direct relationship between interest rates and covenant design, consistent with the existence of a “market price of covenants”, as suggested by Smith & Warner (1979) and Bradley & Roberts (2015). Additionally, we explore how other non-price terms, such as loan size and maturity, adjust to changing market conditions. Our next set of results examines how the overall market structure is affected. We investigate which borrowers stop borrowing in the leveraged lending market (“extensive margin”), and which others keep participating but switch to non-banks (“intensive margin”), and how

¹See, for example, [S&P Global Market Intelligence report, January 2022](#).

²LCD accurately tracks the covenant structure of loans to both public and private borrowers, and categorizes loans without maintenance covenants as “covenant-lite”. Following the existing literature, we define covenant-lite loans as those with no financial maintenance covenants, while these loans can still include affirmative and negative “incurrence” covenants. The widely used DealScan data covering the wider syndicated loan market provides poor coverage of the leveraged lending segment, particularly covenants (Becker & Ivashina, 2016).

these changes in borrower behavior affect the market share of banks and non-banks.

Our empirical strategy utilizes a difference-in-differences (DiD) design around the Clarification, relying on relationship lending to identify exposure to the shock, as suggested by the theoretical framework. The treatment group consists of firms that have borrowed from regulated banks in the past, and the control group is made up of firms that have previously borrowed from the shadow banking sector. As in similar empirical setups (Chodorow-Reich, 2013), our identifying assumption is supported by the literature showing that borrowers are more likely to seek new loans from their relationship banks (e.g., Degryse & Van Cayseele 2000; Degryse & Ongena 2005; Ivashina & Scharfstein 2010).

The Clarification offers potentially useful variation to study non-price competition, but two key residual challenges remain. First, regulated banks and non-banking institutions are not comparable on both observable and unobservable aspects, which results in selection bias in standard DiD estimates. To mitigate the scope for confounds, we saturate our key empirical specifications with lender \times time and borrower fixed effects. This is feasible as lenders issue several loans per quarter and borrowers take multiple loans during the sample period. Second, loan *offers* are generally unobservable (Berg, 2018). To overcome this, for each borrower and each quarter, we proxy for unobservable offers by using the average loan terms extended by relationship lenders to other borrowers in the same quarter, controlling for fixed effects and other factors. Our analysis draws inference from the same borrower over time, and compares borrowers from the same lender but with different previous relationship banks. Through this approach, we seek to sweep up most sources of unobservable heterogeneity, and isolate the effect of a shock to a borrower's offered non-price loan terms.

We find that firms exposed to less covenant lite loan offers are roughly 37% more likely to switch lenders after the Clarification, and 19% less likely to receive covenant-lite loan offers. Borrowers switch at higher rates only to new lenders that offer covenants lite loans. Our results also support the idea that favorable price terms can compensate for less attractive non-price terms. Regulated banks offer loan rates that decrease 0.49% relative to rates offered by non-banks after the Clarification, and they set other non-price terms more cautiously. In effect, this amounts to an estimate for a "market price of covenants". Other non-price-terms also adjust to compensate for stricter covenants,

with collateral requirements increasing and loan maturity dropping slightly, although only a small fraction of loans in the sample are asset-backed.³ Overall, our findings align with the anecdotal perception that the Clarification mostly aimed to address the waning use of covenants, something we discuss in more detail in Section 3.

We then study how different borrowers react to changed loan terms. Financially weaker borrowers face difficulties meeting heightened covenant requirements, and are sensitive to changes in loan terms. The weakest borrowers stop borrowing in the leveraged loan market (extensive margin), or switch to non-banks (intensive margin). In the cross-section, borrowers with demonstrated demand for covenant-lite loans are more likely to switch lenders due to the Clarification, and lenders with a strong presence in the covenant-lite sector lose more borrowers. Banks retain some customers through offering favorable interest rates, but overall lose 1.2% of total market share to non-banking institutions. As a result shadow banks increase their presence in the leveraged loan market.

Finally, we rule out alternative explanations for our findings. First, the Clarification highlights borrower risk and repayment capacity. Lenders might engage in a flight to quality and reduce lending to high-risk borrowers as a response to the new regulation. Second, regulated banks may reduce credit supply in the leveraged loan segment as a result of the Clarification, regardless of covenants. We find no evidence to support either hypothesis. Our are supported by a wide range of additional robustness checks and placebo tests.

Our results emphasize the interplay between various price and non-price loan terms, and highlight competitive dynamics that can impact the industry structure. As [Adrian & Shin \(2009\)](#) stress, understanding lender competition is critical for effective macroprudential regulation, which must “internalize the externalities that are generated in the shadow banking system”. While we use the Clarification primarily as a laboratory to study how various loan terms adjust to a shock to a specific non-price term, and how borrowers react to this shock, our findings also have implications for the ongoing regulatory discussion on leveraged lending. As of early 2023, the US administration is considering the reintroduction of a leveraged lending guidance, as it has lapsed after 2016 Presidential elections, with a focus on extending its scope to non-banks. In this context, our finding of

³LCD classifies loans as asset-backed when they are explicitly collateralized by a specific asset, although loans can be *secured* by a general lien against the firm. For simplicity, we refer to asset-backed loans as “collateralized”.

non-price competition between banks and non-banks can inform in regulatory design.⁴

Related Literature. Our study contributes to the empirical contracting literature, particularly in its examination of the costs and benefits of loan covenants. Recent studies include [Matvos \(2013\)](#), who quantifies the benefits that covenants provide to firms as a tool to complete debt contracts and [Prilmeier \(2017\)](#), who examines the effect of lending relationships on covenant choice and on borrowers' economic trade-offs. In this context, the closest study to ours is [Murfin \(2012\)](#), who studies the supply-side determinants of covenants, and shows that banks increase covenant tightness in new contracts after suffering losses. Our results highlight the role of non-price competition on covenants and its impact on the structure of lending markets.⁵

Our paper adds to the recent empirical literature on the competitive dynamics between regulated banks and the shadow banking system in corporate lending. [Irani, Iyer, Meisenzahl & Peydro \(2021\)](#) study the impact of non-bank participation on credit availability in the economy during a crisis. They find that less capitalized banks reduce loan retention to the advantage of non-banks. Other recent papers highlight that non-banks increasingly act as lead arrangers in the syndicated loan market and are particularly relevant in providing credit to middle-market firms ([Davydiuk, Marchuk & Rosen, 2020](#); [Chernenko, Erel & Prilmeier, 2022](#)). 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.⁶

⁴See [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](#)

⁵A large and broadly related literature has instead focused on firms’ responses to covenant violations. Key recent contributions are [Chava & Roberts \(2008\)](#), [Roberts & Sufi \(2009\)](#), [Demiroglu & James \(2010\)](#), [Nini, Smith & Sufi \(2012\)](#), and [Falato & Liang \(2016\)](#), among others.

⁶Other 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, Nini & Edison \(2020\)](#) stress that non-bank lenders benefit from the monitoring activity of banks. One reason why non-traditional banks have been gaining market share against incumbents is superior technology ([Fuster, Plosser, Schnabl & Vickery, 2019](#); [Chava, Ganduri, Paradkar & Zhang, 2021](#); [Berg, Fuster & Puri, 2022](#)).

We exploit the Clarification as a theoretically-motivated laboratory to investigate non-price competition in corporate credit markets, yet some of our results also speak to the efficacy of the Clarification as a regulatory intervention. While this is not the key thrust of our paper, some of our findings add to recent work on trends in loan markets surrounding the Leveraged Lending Guidance and Clarification. [Kim et al. \(2018\)](#), [Calem et al. \(2020\)](#), and [Newton, Ongena, Xie & Zhao \(2022\)](#) emphasize the relative importance of the 2014 Clarification as the primary event affecting the leveraged lending market, and document its impact on non-bank participation in speculative-grade term loan syndicates. [Schenck & Shi \(2022\)](#) instead find that, while the 2013 Guidance led to an increase in non-bank syndicate participation, this trend entirely reversed after the 2014 Clarification. We add to this literature by analyzing the most comprehensive sample of the leveraged loan market which allows us to compare loans arranged by regulated banks with those exclusively arranged by non-banks. This is a key advantage since non-price terms are established during loan origination. Therefore, our analysis focuses explicitly on this comparison.⁷ We document that a first-order motive for borrowers to switch their lead lenders after the Clarification is covenant-lite loan structures. Our theoretical motivation and empirical findings highlight the importance of a regulatory advantage in competition on non-price terms and help explain the dynamics of how and why non-banks attract new borrowers ([Gopal & Schnabl, 2022](#)).

2 Theoretical Framework

In this section, we lay out a stylized model in the spirit of [Matvos \(2013\)](#). The model largely relies on reduced-form specifications of the costs and benefits of strict non-price terms rather than micro-founding them. Nevertheless, it serves as a benchmark to highlight key features of our empirical setting and provides testable predictions. Although the model's intuition applies to several non-price terms which can interact with each other ([Berg, Saunders & Steffen, 2016](#)), for the sake of exposition, we refer to them as covenants in this Section.

Borrowers and Loan Contracts. Consider a firm described by a vector of characteristics ξ .

⁷Unlike our study, the main results in [Calem et al. \(2020\)](#) and [Schenck & Shi \(2022\)](#) rely on data from the Shared National Credit (SNC) program, which tracks the syndicate structure for loans originated by the 18 largest regulated banks in the market, all of which are subject to the Clarification. Nonetheless, their studies provide valuable insights into the syndicate structure and non-bank participation in those loans.

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 competitive lenders, which compete to supply the loan amount $e(\phi, \xi)$ to a firm with characteristics ξ and covenant ϕ .⁸ 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., covenants have value for lenders as shown in the empirical literature (Chava & Roberts, 2008). In other words, all else equal, covenant 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 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 $v_L(\phi, \xi)$ captures 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 letting the firm operate at a larger scale due to larger borrowed amounts (Smith & Warner, 1979).⁹ On the other hand, covenants reduce borrowers' financial flexibility, 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 $e(\phi, \xi)$.

⁸Observe that, although lender are allowed to differ in λ , interest rates and loan amounts do not depend on λ because of competitive credit markets.

⁹As 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.

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.¹⁰ 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 $\mathcal{S}(\xi)$ 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 counterintuitive at first glance, a higher interest rate implies fewer resources lent and, possibly, a lower surplus for the firm.

Restrictions to Non-Price Terms. Our empirical analysis exploits 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-lite loans, for

¹⁰In practice, relationships can carry value due to private information ([Petersen & Rajan, 1994](#)) or due to lender specialization ([Paravisini, Rappaport & Schnabl, 2023](#)).

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 in larger amounts borrowed and lower interest rates. This reduces the loss through the interest rate component $y(\phi_R^*, \xi) - y(\phi^*, \xi)$.¹¹

Instead of “staying” with its relationship lender λ^* and losing surplus, the firm can choose to take the loan from a new unrestricted lender $\lambda^N \in \Omega_\lambda \setminus \Omega_\lambda^R$. The firm “switches” lenders if $\mathcal{S}(\xi, \lambda^*, \phi_R^*) - \mathcal{S}(\xi, \lambda^N, \phi^*) < 0$, where

$$\mathcal{S}(\xi, \lambda^*, \phi_R^*) - \mathcal{S}(\xi, \lambda^N, \phi^*) = \overbrace{v_0(\xi, \lambda^*) - v_0(\xi, \lambda^N)}^{\text{relationship component}} + \overbrace{v_L(\phi_R^*, \xi) - v_L(\phi^*, \xi)}^{\text{payoff component}} + \overbrace{y(\phi_R^*, \xi) - y(\phi^*, \xi)}^{\text{interest rate component}}.¹²$$

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 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 for the surplus reduction of operating with tighter covenants, the firm

¹¹Notice 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.

¹²Because 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 .

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, interest rates similar to those in the period before the Clarification. In contrast, firms that stay with their relationship lenders will obtain loan contracts with higher covenant protection and, plausibly, lower interest rates. In other words, borrowers face a choice between lower interest rates and fewer covenants.

3 Data and Background

3.1 Institutional Background: The Leveraged Lending Clarification

The Leveraged Lending Guidance and Clarification. To investigate the effect of loan covenants on competition in credit markets, we exploit the introduction of a clarification (“the Clarification”) to 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.¹³ 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

¹³Specifically, 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.”

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 specified minimum loan underwriting standards ([Guidance, 2013](#)).

Importantly, there was considerable uncertainty among market participants regarding the scope and applicability of the Guidance. For this reason, compliance was initially low, to which the Agencies reacted by issuing the industry-wide Clarification in November 2014 ([Mnuchin & Phillips, 2017](#); [Clarification, 2014](#)). To the extent that the Clarification was not completely anticipated, it serves as a shock to regulated banks' ability to offer loans with favorable non-price terms, particularly covenant-lite loans. Our interpretation is reinforced by the fact that the Agencies sent out "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.¹⁴ Based on these institutional details, we use the introduction of the Clarification as a starting point for the treatment period, and in additional robustness checks verify that the issuance of the Clarification was indeed the decisive event.¹⁵

Finally, the leveraged lending guidance was subsequently overturned by the Government Accountability Office, in 2018, on the grounds that it required congressional review. In the Appendix we show evidence consistent with our main effects reverting after the leveraged lending Clarification was overturned.

Covenants and Other Loan Terms. 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

¹⁴In 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](#)).

¹⁵Prior work on the leveraged lending Guidance and Clarification show somewhat mixed results with respect to the relative importance of these events. [Kim et al. \(2018\)](#), [Calem et al. \(2020\)](#), and [Newton et al. \(2022\)](#) emphasize the relative importance of the 2014 Clarification, while [Schenck & Shi \(2022\)](#) instead find that the 2013 Guidance had strong effects. Similarly, there is mixed evidence on the link of the regulatory action and loan terms. [Schenck & Shi \(2022\)](#) find that the impact on the presence of maintenance covenants was negative even for loans originated by regulated lenders. [Kim et al. \(2018\)](#) present mixed evidence on whether there were differences between lending policies of banks and non-banks following the Clarification. Our work, based on a different, more comprehensive data set covering the entirety of the leveraged loan market are not perfectly comparable to these papers, but imply a large role of loan covenants in explaining the rise of non-banks in this market.

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 (2014) 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 underscores the relevance of covenants and links 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 underwriting structures were in part attributable to *aggressive competition* and market liquidity.”

These observations from the media and official press releases suggest that the covenant channel is the primary mechanism affecting the dynamics of lending after the Clarification. This conjecture is consistent with how much emphasis the agencies put on various loan dimensions in the actual regulatory documents. Regulators talk extensively about loan covenants in the Clarification (mentioned 4 times). The only other loan dimension that receives comparable attention is collateral, which is mentioned 3 times in the Clarification. In contrast, loan amounts, maturities and interest rates are barely mentioned, and when they are mentioned, it is not in the context of loan origination terms.

Non-Bank Participation in Leveraged Lending. 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.2). 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 multiple non-banks gained market share, and the representation of non-banks among the top issuers (presented in Panel B of Appendix [Table A.2](#)) doubled from two to four soon after the Clarification. These results indicate an economically meaningful 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.

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 and contains both public and private borrowers. LCD is the most suitable data source for this study because the Clarification specifically targets leveraged loans, and this data addresses the prevalence of private borrowers in this segment. To determine the treatment status of firms, we identify lead arranger for each loan, since the Clarification emphasizes that the rules apply primarily to lenders that originate loans, even if they do not subsequently hold any stake in those loans. 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 or 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 poor reporting quality in the leveraged lending segment, and widespread misclassification of covenant-lite loans.

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. All variable descriptions can be found in Appendix [Table A.1](#). Panel

A shows that the average leveraged loan is \$640 million, bears an interest rate of 4.08%, and has a maturity of almost six 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 (meaning that the borrower has not borrowed from the same lender since our sample begins).¹⁶ 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 unrated borrowers with less collateral, consistent with previous work (Chernenko et al., 2022). This suggests that the shadow banking system likely picks up riskier borrowers in the leveraged lending market. Perhaps not surprisingly, borrowing 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 that has a previous lending relationship with a regulated bank, while the figure rises to 96% for bank-originated loans. In the 2012–2018, banks were significantly more active than non-banks in the leveraged loan segment, as the number of observations for the two groups suggests. Overall, Panel B indicates that our treated and control lenders are not comparable in the leveraged loan market along a number of different dimensions. This underscores the importance of saturating the model with lender \times 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, is less likely to have collateral, has a longer maturity, and is more likely to involve rated borrowers. Appendix Table A.2 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 soon after the introduction of the Clarification.

¹⁶Since our data are left censored, we designate relationships as new starting from each firm's second loan.

4 Non-Price Competition

4.1 Empirical Facts

We begin our analysis with a series of non-parametric tests in [Table 2](#). Panel A reports the frequencies at which borrowers switch from bank to non-bank lenders (or vice versa), or stay with the same type of lender, when taking new loans. Frequencies are tabulated separately for the periods before and after the Clarification. The two leftmost subpanels show that, after the Clarification, the chance that a borrower switches from a bank to a non-bank lender increases from 3.3% to 5.1%, a relative increase of almost 60%. The probability of switching from a non-bank lender to a bank instead drops from 47.3% to 35.8%, a relative drop of about 25%. The rightmost subpanel shows that all changes are statistically significant at the 1% level.

Panel B tabulates the fraction of covenant-lite to total loans for the same four groups of lenders in Panel A. Around the Clarification, the fraction of covenant-lite lending for borrowers switching from banks to non-banks rises from 11.9% to 73.6%. The share of covenant-lite lending for borrowers switching from non-banks to banks only increases from 35.7% to 57.2%.¹⁷

Altogether, the descriptive analysis in [Table 2](#) hints that the Clarification has an effect on lender–borrower relationships, as borrowers switching from banks to non-banks receive loans with less covenant protection.

4.2 Switching Lenders

Empirical Strategy: Relationship Lenders. We support the non-parametric inference in [Table 2](#) with an empirical loan-level setup exploiting the stickiness of banking relationships. Our identifying assumption is that borrowers generally approach their relationship banks for new loans, as extensively documented in the literature ([Degryse & Van Cayseele, 2000](#); [Degryse & Ongena, 2005](#); [Ivashina & Scharfstein, 2010](#)). As in [Chodorow-Reich \(2013\)](#), we therefore assign treatment status based on a borrower's last previous lender. If borrower b takes out a loan at time t , and its last lender was a regulated bank, the indicator $BankBorrower_{b,t}$ takes value one.

¹⁷As expected, the use of covenants is waning for all lender types, consistent with the aggregate trend documented in the literature.

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 the 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.¹⁸ Accordingly, we define the *relationship lender* as the last lender to which borrower b was linked in our sample before quarter t .

We estimate the following specification:

$$NewLender_{b,l,t} = \beta_0 + \beta_1 Post_t \times BankBorrower_{b,t} + \beta_2 BankBorrower_{b,t} + \beta_3 X_{b,l,t} + \delta_b + \eta_{l,t} + \varepsilon_{b,l,t}. \quad (1)$$

The outcome variable, $NewLender_{b,l,t}$, is an indicator for whether a loan is taken out from a lender that had not previously extended credit to the firm. $BankBorrower_{b,t}$ is an indicator equal to one if the borrower’s relationship lender is regulated, that is, a bank.¹⁹ As these lenders were affected by the Clarification, our “treatment” group consists of borrowers that 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. Observe that, unlike in standard DiD settings, our treatment indicator is time varying. This is because the same borrower b could have borrowed from either a bank or a non-bank at different times. The advantage of this setup is that it allows us to dynamically update each borrower’s relationship to the regulated banking sector. For example, a borrower that only borrowed from regulated lenders prior to the Clarification, and switched to a non-bank for their first loan afterwards, would still be considered a “bank borrower” in a static setup, while our dynamic treatment definition allows this borrower to be considered a bank borrower only for their first loan. In unreported analysis we verify that our results are statistically and economically almost identical if we use an alternative, time-invariant, treatment assignment based on whether all borrowing during the pre-period was from regulated lenders.

¹⁸Our results are unchanged when including these observations.

¹⁹Syndicated 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 lenders 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 lenders. We therefore assign treatment based on the lead arranger being a regulated bank only.

$Post_t$ is 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 it is divided into two portions: one that spans roughly three years before the Clarification, and another that spans four years after it.²⁰ The variables in $X_{b,l,t}$ are vectors of indicators for loan purpose and the presence of a credit rating.²¹ δ_b denotes borrower fixed effects, $\eta_{l,t}$ lender \times is quarter fixed effects which nest quarter fixed effects that absorb seasonality in lending (Murfin & Petersen, 2016). $\varepsilon_{b,l,t}$ is an error term.

Estimation Results. Table 3 reports estimates from Specification (1). In all specifications, we cluster standard errors at the borrower level to account for arbitrary within-borrower correlation of errors.²² The specifications from the leftmost to the rightmost column correspond to increasing levels of model saturation with high-dimensional fixed effects. Across all specifications, the coefficient of $Post_t \times BankBorrower_{b,t}$ is positive and statistically significant at the 1% level. Estimated coefficients range from 0.140 in Column (1) to 0.371 in the most stringent specification of Column (8). Economically, this coefficient indicates that borrowers with existing bank relationship have a roughly 35% higher probability of leaving their relationship lenders after the Clarification, in comparison to borrowers from non-banks. Observe that the coefficient of $BankBorrower_{b,t}$ is negative in all specifications, as bank borrowers are on average less likely to break relationships and switch lenders.

Overall, the table shows that firms that historically borrowed from regulated banks are more likely to switch to new lenders after the Clarification. This result is consistent with broad trends in lending around the Clarification, and in particular with Kim et al. (2018), who offer a timely program assessment.

Pre-Trends and Two-Way Fixed-Effects (TWFE). Panel A of Figure 1 inspects the parallel trends assumption. The figure depicts the fraction of bank and non-bank borrowers switching lenders over time. The plots do not provide evidence of divergent pre-trends before the Clarification.

²⁰In robustness tests presented in Appendix Table A.4 and Table A.5, we show that our results are robust to changing this time window to a symmetric one-, two-, three-, or four-year window, respectively, with the strongest effects observed in the one-year window surrounding the Clarification.

²¹Since loan terms are set jointly, our specifications do 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.

²²As an alternative, we verify that our results are robust to clustering at the lender level.

Prior to the Clarification, borrowers switch to banks and non-banks at similar rates and evolve in parallel. Following the Clarification, instead, the fraction of new loans made out to switchers by non-banks 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%.

While the summary statistics of [Table 1](#) suggest that banks and non-banks offer different loan terms, the highly saturated fixed-effect specifications alleviate these concerns by absorbing possible confounding factors. This approach relaxes the parallel trends assumption and increases its credibility by requiring that it holds only conditional on controls. Borrower fixed effects account for unobserved heterogeneity in borrowers' propensity to change their lenders. Lender \times time fixed effects account for differences in the intensity of lenders' activity in the leveraged loan market.²³

Although the treatment timing involves a single date, the treatment status $BankBorrower_{b,t}$ is time varying. Thus, our TWFE specifications with both unit and time fixed effects are potentially affected by negative weighting issues that usually arise with staggered-treatment timing. As discussed for example in [Roth, Sant'Anna, Bilinski & Poe \(2022\)](#), OLS predictions with binary outcomes fall outside the unit interval in TWFE models, resulting in negative weights on some units in the estimation of OLS coefficients. Reassuringly enough, the estimations in Columns (1) and (2) without fixed effects are not affected by these issues, and deliver results that are qualitatively consistent with our more saturated specifications. In addition, we verify in unreported results that our results hold when collapsing all observations in the pre- and post-period into a single observation per borrower.

4.3 Covenants and Relationships

Empirical Strategy. As discussed in Section 3.1, anecdotal evidence highlights the covenant channel as a mechanism affecting the dynamics of lending after the regulation. Our empirical strategy to test this conjecture mimics the one in the previous section. The dependent variable $CovliteOffer_{b,l,t}$ proxies the likelihood that borrower b receives a loan offer containing no financial covenants from its relationship lender, irrespective of the identity of lender l in quarter t . We define

²³Notice that these are current lender fixed effect, while the treatment is determined based on the previous relationship lender.

$CovliteOffer_{b,l,t}$ as the proportion of covenant-lite loans issued by the relationship lender of loan (b, t) , to all other borrowers in the same quarter t when borrower b obtains their loan. We drop loans for which we have no comparison loan to determine loan offers. The assumption is that average realized loan terms at any point in time, conditional on controls and fixed effects, represent a good proxy for the unobservable, offered loan terms.²⁴

Thus, we estimate the following specification:

$$CovliteOffer_{b,l,t} = \beta_0 + \beta_1 Post_t \times BankBorrower_{b,t} + \beta_2 BankBorrower_{b,t} + \beta_3 X_{b,l,t} + \delta_b + \eta_{l,t} + \varepsilon_{b,l,t}. \quad (2)$$

Estimation Results. Table 4 reports estimation results for different levels of saturation with high-dimensional fixed effects and controls. From left to right, the interpretation of the estimates changes from *mutatis mutandis*, in which lender and borrower characteristics are free to vary with loan non-price terms (covenants), to *ceteris paribus*, in which this heterogeneity is progressively absorbed. Although more saturated specifications reduce the scope for confounds, *mutatis mutandis* specifications are informative as covenants are set endogenously to borrower and lender characteristics and other loan terms, as the conceptual framework in Section 2 suggests.

Column (1) shows that the coefficient of $Post_t \times BankBorrower_{b,t}$ is -0.216 and statistically significant at the 1% level. Column (8) presents the most stringent specification, in which we include borrower, lender \times quarter, loan purpose, and rating fixed effects. In particular, lender \times quarter fixed effects assuage the concern that the most optimistic banks are the most likely to offer covenant-lite loans and, at the same time, the most likely to show the largest expansion of their loan portfolios. This specification absorbs a large share of time series variation in the supply side determinants of covenants (Murfin, 2012), with an R-squared of 88%. The coefficient has a point estimate of -0.193 and is statistically significant at the 1% level. Economically, this coefficient indicates that borrowers with an existing bank relationship have a roughly 20% higher probability of facing a loan offer that includes covenants from their relationship lenders after the Clarification compared to borrowers from non-banks. The specifications in Columns (2) to (7) estimate similar loadings for $Post_t \times BankBorrower_{b,t}$, all statistically significant at the 1% level. Observe that,

²⁴This setup means we only consider loans for which we can determine offered terms, that is, those where the relationship bank offers at least one loan to another borrower.

across all specifications, $BankBorrower_{b,t}$ has a negative sign. As $Post_t \times BankBorrower_{b,t}$ is an independent variable, the coefficient of $BankBorrower_{b,t}$ indicates that bank borrowers used to offer more covenant-lite loans prior to the Clarification. Panel B of Figure 1 shows that this is the case, but that the trend has reversed after the Clarification.

Overall, the table shows borrowers that historically borrowed from regulated lenders 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 commercial banks and reducing their supply of covenant-lite loans.

Pre-Trends and Two-Way Fixed-Effects (TWFE). Panel B of Figure 1 inspects the parallel trends assumption. The figure depicts the time series of the proportion of non-covenant-lite loans offered by banks (red line) and non-banks (blue line). Amid the waning use of covenants, banks offer relatively more covenant-lite loans before the Clarification. The two trends diverge after the Clarification. Consistent with the negative estimate of $Post_t \times BankBorrower_{b,t}$ in Table 4, the blue line moves above the red line, and non-banks offer more covenant-lite loan structures than banks following the Clarification.

4.4 Lender Choice: Preference for Covenant-Lite Loans

The results presented in both Table 3 and Table 4 suggest that borrowers are more inclined to change lenders following the Clarification, since they are less likely to receive a covenant-lite loan offer from their relationship bank. These results are silent on the loan terms their *new lender* offers to entice them to move.

In Table 5 we sharpen our analysis and investigate borrowers' propensity to switch to new lenders based on the loan terms offered by the new lender. To this end, we refine the outcome variable $NewLender_{b,l,t}$ for specific subgroups of new lenders in (1). For convenience, Column (1) re-prints the result of our most complete specification in Table 3, which estimates an increase in the likelihood of switching to any new lender after receiving a non-covenant-lite loan offer of roughly 37 percentage points. In Column (2), the outcome variable takes the value of one only if the new lender is also offering a covenant-lite loan. The coefficient of $Post_t \times BankBorrower_{b,t}$ is 0.589, statistically significant at the 1% level. The outcome variable in Column (3) instead takes

the value of one for loans that are both from a new lender and not covenant-lite. The coefficient is economically small and not statistically different from zero. In addition, a two-tailed test of equality of the key coefficients of interest in Columns 2 and 3 rejects the null that they are equal. Overall, the increased inclination to switch lenders is evident only when the new lender proposes a covenant-lite loan. There is no statistically or economically significant impact of switching to new lenders that do not provide covenant-lite structures, highlighting the importance of covenants in driving competition.

4.5 Trade-off Between Price and Non-Price Terms

The model in Section 2 embeds a trade-off between stricter covenant protection and lower loan rates. Empirically, the “package” of offered loan terms should reveal trade-offs between loan price and non-price terms, as well as between different non-price terms. Trade-offs among loan terms are known to be complex. [Roberts & Schwert \(2022\)](#), for example, find that the relationship between covenants and interest rates is not trivially that more covenants allow for lower rates. Instead, the interaction between covenants and rates depends heavily on the specific circumstances of each loan, specifically the lending banks, borrower characteristics, and contemporary interest rate environments.

[Table 6](#) investigates the trade-off between loan terms using our setting. The table reports estimates from the most stringent version of Specification (2), with outcomes being other loan dimensions, namely loan interest rate, maturity, collateral, and size. We expect the Clarification to have a direct effect on equilibrium loan terms and bring to light existing trade-offs. The coefficient on $Post_t \times BankBorrower_{b,t}$ in Column (1) is negative and statistically significant at the 1% level. This indicates that banks charge lower interest rates following the Clarification, consistent with the prediction that attractive price terms can substitute for attractive non-price terms. The estimates in Columns (2) to (4) study the effect of the Clarification on other non-price terms. As the Clarification asks for broadly more conservative lending terms, we expect other loan dimensions to adjust as well, even though practitioners generally perceived its main effect to come through loan covenants. Indeed, we find an economically small but statistically significant drop in loan maturity of about two months. We also find evidence of increased collateral requirements, consistent with the Clarification stressing the need for proper collateral in leveraged loans. Finally, the estimates

in Column (4) do not provide evidence of significant changes in loan size after the Clarification.

Overall, the results in Table 6 highlight significant trade-offs among loan terms. Regulated lenders appear to follow more cautious lending practices after the Clarification. Loan rates fall to compensate for banks' lower ability to offer covenant-lite loan structures and, more generally, more conservative non-price terms.

5 Market Structure

In this section we explore changes to the structure of the leveraged loan market following the Clarification.

5.1 Extensive and Intensive Margins

Our previous results show that, following the changed set of offered loan terms after the Clarification, some bank borrowers switch from regulated lenders to shadow banks conditional on staying in the market. These results are necessarily silent on two questions. First, some previous bank borrowers might drop out of the leveraged lending market altogether following the regulatory change. The coefficient estimates in our main regression pick up only on those firms that remain in the market. An unanswered question is: Which firms decided to leave the market altogether? That is, what drives movement in the leveraged lending market on the extensive margin? A second, related, question is which characteristics of firms in the pre-Clarification period predict their switching in the post-Clarification period? Were those borrowers that switched to non-banks afterwards those that received particularly attractive loan terms in the pre-Clarification period, and hence had the easiest time switching lenders? Or did marginal borrowers, which highly value covenant-lite loan structures due to their weak finances, drive the migration in the market on the intensive margin? We explore this change to the market structure in Table 7. To this end, the analyses in Table 7 limit data to bank borrowers and only contains the pre-Clarification period. We then estimate:

$$Outcome_{b,i,t} = \beta_0 + \beta_1 X_{b,t} + \eta_i + \eta_t + \varepsilon_{b,i,t}, \quad (3)$$

where $Outcome_{b,i,t}$ is the outcome variable for borrower b in industry i originated in quarter t ,

$X_{b,t}$ is a set of explanatory variables, and η_i and η_t are industry and quarter fixed effects, respectively.

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.²⁵ Importantly, we do not observe borrowers after LCD stops tracking them. These borrowers might, for example, tap other sources of capital, lose funding, or switch to other types of financing. With this caveat in mind, we refer to this effect of the Clarification as its “extensive” margin. We then relate the decision to leave the market using various pre-Clarification period firm and loan characteristics.

In Column (1), the coefficient on the borrowers’ credit rating is positive and statistically significant at the 1% level. Ratings are coded using a discrete scale in which lower values correspond to better ratings (see Appendix Table A.1). Thus, borrowers with worse credit ratings are more likely to exit the market. The results in Column (2) show that there is no economically or statistically significant link between the number of pre-Clarification period loans and likelihood of leaving the market. Column (3) turns to the loan terms offered before the Clarification to borrowers leaving the market. The results suggest that borrowers whose previous loans are smaller and more expensive are leaving at higher rates. Interestingly, the coefficient on whether loans prior to the Clarification were covenant-lite is not statistically and economically significant in these specifications, implying that covenant-liteness might not have mattered on the extensive margin. Taken together, these results are consistent with the most marginal borrowers being pushed out of the market.

In Columns (4) to (6), we investigate the “intensive” margin of the Clarification. Conditional on staying, we find similar patterns as for the extensive margin. Poorly rated borrowers and those with less attractive previous loan terms are the first to switch lenders. While coefficients generally point in the same direction as in the extensive margin tests, their magnitude is substantially weaker: it is about one-third smaller than those for the extensive margin. Two coefficients show up as different in explaining switching as opposed to leaving. First, the number of previous loans in the leveraged lending market has a strong positive and significant coefficient estimate. This result is consistent with firms that rely more heavily on leveraged loans being more willing to stay in the market and switch to non-banks, as opposed to leaving the market. Second, the coefficient on covenant-lite loan

²⁵Note that, due to this change in data structure, these models have to feature industry fixed effects *in lieu* of the borrower fixed effects in our previous specifications.

terms turns positive and statistically significant at the 5% level, meaning firms that have obtained covenant-lite loans previously from banks are more likely to switch to non-banks once regulated lenders cannot offer them these terms any more, consistent with our main results.

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 do not contradict the anecdotal evidence, given in Section 3.1, that the main channel through which the Clarification affects loan terms is covenants, and, to a lesser degree, collateral. Financially weaker borrowers might be the least likely to be able to comply with heightened covenant requirements or to put up sufficient collateral, and hence react more sensitively to changes in loan terms.

5.2 Heterogeneity in Borrower and Lender Reactions

The model in Section 2 hints at heterogeneity in borrowers with respect to the value they attach to covenant-lite loan structures. We expect the Clarification's impact to be amplified for borrowers that, before the Clarification, revealed a stronger preference for covenant-lite loans. Recent literature has highlighted the importance of lender specialization (Paravisini et al., 2023). Hence, lenders that used to offer less covenant-lite loan structures before the Clarification are less constrained by the regulatory change. Accordingly, their clients should be less affected than those of lenders that specialize in covenant-lite loans, as the loan contracts they took before the Clarification included covenants to begin with. We test these conjectures through a set of triple DiD estimations, presented in Table 8.

Column (1) investigates the cross-sectional effect of *Covlite Demand*, an indicator equal to one if the borrower's last loan prior to the Clarification was covenant-lite, on lender switching. This specification tests for cross-sectional heterogeneity based on demonstrated covenant-lite loan demand.²⁶ The dependent variable is $NewLender_{b,l,t}$. The estimated coefficient of the interaction of *Covlite Demand* with $Post_t \times BankBorrower_{b,t}$ is 0.197 and is statistically significant at the 1%

²⁶Note that the un-interacted indicator *Covlite Demand* in Table 8 is absorbed by borrower fixed effects. For ease of exposition, *Covlite Demand* and *Covlite Supply* are standardized to have a mean of zero and a standard deviation of one. Thus, the coefficient represents an increase in the probability to switch to a new lender in a response to a one standard deviation increase in a demand for or supply of covenant-lite loans.

level. Thus, borrowers that were more dependent on covenant-lite loans before the Clarification are roughly 20% more likely to switch lenders than those for which *Covlite Demand* is equal to zero.

In Column (2), we look into the cross-sectional effects of the lender’s propensity to offer covenant-lite contracts, namely the indicator *Covlite Supply*. The latter is equal to one for relationship lenders that extended an above-median fraction of covenant-lite loans before the Clarification, and zero otherwise. This specification therefore tests for cross-sectional heterogeneity based on demonstrated specialization in supplying covenant-lite loans. The estimated coefficient of the interaction of *Covlite Supply* with $Post_t \times BankBorrower_{b,t}$ is 0.238 and statistically significant at the 1% level. These results suggest that relationship lenders that extended more covenant-lite loans before the Clarification were more likely to lose borrowers that sought funding without covenant protection.

5.3 Market Shares

We estimate the extent to which the restrictions on issuing covenant-lite loans for banks after the Clarification translate into a loss of market share to the advantage of the shadow banking sector. Our analysis to estimate the effect of loan covenants on banks’ market share is similar to the strategy we employ at the loan level. The key difference is that we aggregate data at the lender–quarter level to analyze the effect of the Clarification on lenders’ market share. We estimate the following DiD specification around the Clarification, with regulated lenders as treatment group:

$$Share_{l,t} = \beta_0 + \beta_1 Post_t \times Bank_l + \beta_2 X_{l,t} + \eta_l + \eta_t + \varepsilon_{l,t}, \quad (4)$$

where $Share_{l,t}$ is the outcome variable, $Post_t$ is an indicator for all quarters after 2014Q4, $Bank_l$ is an indicator for regulated lenders, $X_{l,t}$ is a vector of control variables, and η_l and η_t are lender and quarter fixed effects, respectively.

Table 9 presents the estimation results. Columns (1) and (2) refer to the fraction of covenant-lite loans offered by lenders (“Covlite Share”), while the outcome variable in Columns (3) and (4) is lenders’ market share in the leveraged loan market (“Market Share”). The coefficient on the interaction term $Post_t \times Bank_l$ in Column (1) is -0.148 , statistically significant at the 1% level.

The specification in Column (2) adds control variables, which only marginally affect the estimation results. Thus, after the Clarification, regulated lenders 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 lenders from engaging in a race to the bottom in terms of covenants in the leveraged loan market.

The coefficients on $Bank_l \times Post_t$ in Columns (3) and (4) are -1.215 and -1.210 respectively, both statistically significant at the 10% level. This implies that regulated lenders lost about 1.2% of market share following the Clarification.

Overall, the decisions of individual borrowers to switch lenders leads to a drop in the aggregate 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 (Irani et al., 2021).

6 Additional Analyses and Robustness

6.1 Alternative Channels

In this section, we investigate alternative channels that could plausibly confound the effect of the Clarification on loan covenants and lender choices. The Clarification refers not only to loan non-price terms, but also to other aspects in loan contract design. In particular, the Clarification mentions 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 whether the Clarification could have impacted lenders' choice of borrowers and the market share of banks through those two alternative dimensions.

Changes in Borrower Quality. Panel A of Table 10 offers tests of whether lenders engage in a flight to quality by reducing loans to high-risk borrowers altogether. The table 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 lower values correspond to better 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 0.096. Similarly, in Column (2), the estimated coefficient on the borrower having an investment-grade rating is close to zero. 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.

Panel B of [Table 10](#) considers changes in the debt-to-EBITDA ratios of borrowers following the Clarification. The Clarification specifically mentions loan-to-EBITDA ratios above six as a “red flag” in the evaluation of borrowers’ repayment capacity. 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 six. The estimated coefficients on $Post_t \times BankBorrower_{b,t}$ in 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 0.094, which suggests 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), such a point estimate is -0.575, which suggests that regulated lenders 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 lenders are less likely to lend to borrowers with extremely high debt-to-EBITDA ratios. This evidence is consistent with [Newton et al. \(2022\)](#).

Changes in Loan Supply. Around the Clarification, regulated lenders might have reduced their overall level of leveraged lending, leading to a relative decline in market share compared to non-banks. Panel C of [Table 10](#) tests 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 ratio of leveraged lending from the LCD dataset 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 a lender. These two outcome variables capture the absolute and relative loan supply, respectively, in the leveraged lending segment. If regulated lenders rationed their 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 show that, in both columns, the coefficient on $Post_t$ is not statistically different from zero. Specifically, the coefficient of the regression of total loan supply as an outcome is -0.039, with a standard error of 0.037, and the one with the fraction of leveraged lending as an outcome is actually positive, at 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.

6.2 Additional Tests

We provide a number of robustness tests in the online Appendix. Instead of estimating two separate difference in differences estimates for covenant lite offers and lender switching, we can estimate a 2SLS IV setup. In the first stage, our DiD estimates from [Table 4](#) explain covenant lite loan offers using previous relationship banks being shocked. In the second stage, presented in [Table A.3](#), the instrumented covenant lite loan offer explains lender switching. The results in this alternative estimation setup remain the same, directly linking the lower likelihood of receiving covenant lite offers with increased lender switching.

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.4](#) and [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](#) and [Table A.7](#)). Our results are robust to dropping the quarter with MRIA letters ([Table A.8](#) and [Table A.9](#)), and interacting all control variables with the post indicator ([Table A.10](#) and [Table A.11](#)). [Table A.12](#) and [Table A.13](#) implement placebo tests that replace the Clarification with the original guidance.

[Table A.14](#) investigates whether the repeal of the Clarification had the reverse impact on covenant-lite loans and lender switching compared to the initial passage. When the GAO in 2017 determined that congressional authorization for the Clarification was required, it effectively inval-

idated its initial passage. We find that this reversal indeed led to regulated lenders increasing the number of covenant-lite loans they issued, and in turn, fewer bank borrowers switch lenders. However, these tests have substantially less power than our main regressions, since our sample ends just two quarters after the reversal. In addition, the reversal was anticipated and coincides with other regulatory changes under President Trump. As a result, while the coefficients in this test have the predicted signs, they are not statistically significant at conventional levels.

Table A.15, presents a robustness test regarding the interaction of the Clarification with other loan contract dimensions. The estimates 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)).

Table A.16 repeats the analysis in Table 5 for other dimensions of loan contracts. Columns (1) and (2) present results with the outcome variable defined as a new loan with above (below) median interest rates. The following columns show results for above (below) median maturity, collateral, and loan size, respectively. The general pattern of these tests is that former bank-borrowers switch to new lenders after the Clarification, irrespective of loan spread, amount, or maturity. The only other relevant dimension seems to be collateral, with borrowers switching more if the new loan contains low demands for collateral. Interestingly, this finding is consistent with the anecdotal evidence in Section 3.1, which shows that regulators were concerned mostly about covenants and collateral. As regulated lenders adjusted these two parameters, unregulated shadow banks captured 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 is covenants. Only 2% of loans in our sample carry specific collateral, while about half feature covenants.

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.17 reports the estimation results from two analyses. In these tests, we find evidence that is consistent with a more general association between competition and lax non-price terms, as predicted in Lee & Mann (2021). On the bank level, we find that banks that reduce covenants gain

market share compared to other banks at the same point of time. Consistent with [Fahlenbrach, Prilmeier & Stulz \(2017\)](#), we find that lenders relax contractual safeguards without extending credit to objectively worse borrowers. To complement these results, our last tests in columns 3 and 4 look into loan terms around borrowers' decisions to switch lenders, similar to [Ioannidou & Ongena \(2010\)](#). We find that the first loan between a borrower 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 document a link between non-price terms and competition in credit markets. Firms with past loans from regulated lenders are more likely to seek funding from non-banking institutions to receive covenant-lite loans. We find evidence of significant trade-offs among loan terms. Banks appear to adapt more prudent lending practices after the regulation. Loan rates adjust to compensate for less attractive non-price terms, consistent with the idea of a "market price of covenants".

Non-price competition affects the structure of the leveraged lending market. Following regulatory recommendations, regulated lending institutions slowed their issuance of covenant-lite loans relative to non-banks, effectively stopping what regulators had feared was a race to the bottom. Financially weaker borrowers either turned to the shadow banking sector or exited the market entirely. As a result, banks lost market share to the advantage of non-bank institutions.

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 lending 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.

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Figure 1
Trends in Lender Switches and Covenant-Lite Lending

Panel A plots the frequency of switching to banks and non-banks over time. The blue (red) line represents the fraction of new borrowers among non-banks (banks). Panel B plots the fraction of covenant-lite lending originated to non-financial US borrowers over time. The blue (red) line represents the fraction of covenant-lite lending in the total amount of leveraged lending among non-banks (banks). The vertical lines mark the quarter of the Clarification in the fourth quarter of 2014. Data on leveraged loans come from S&P's LCD database.

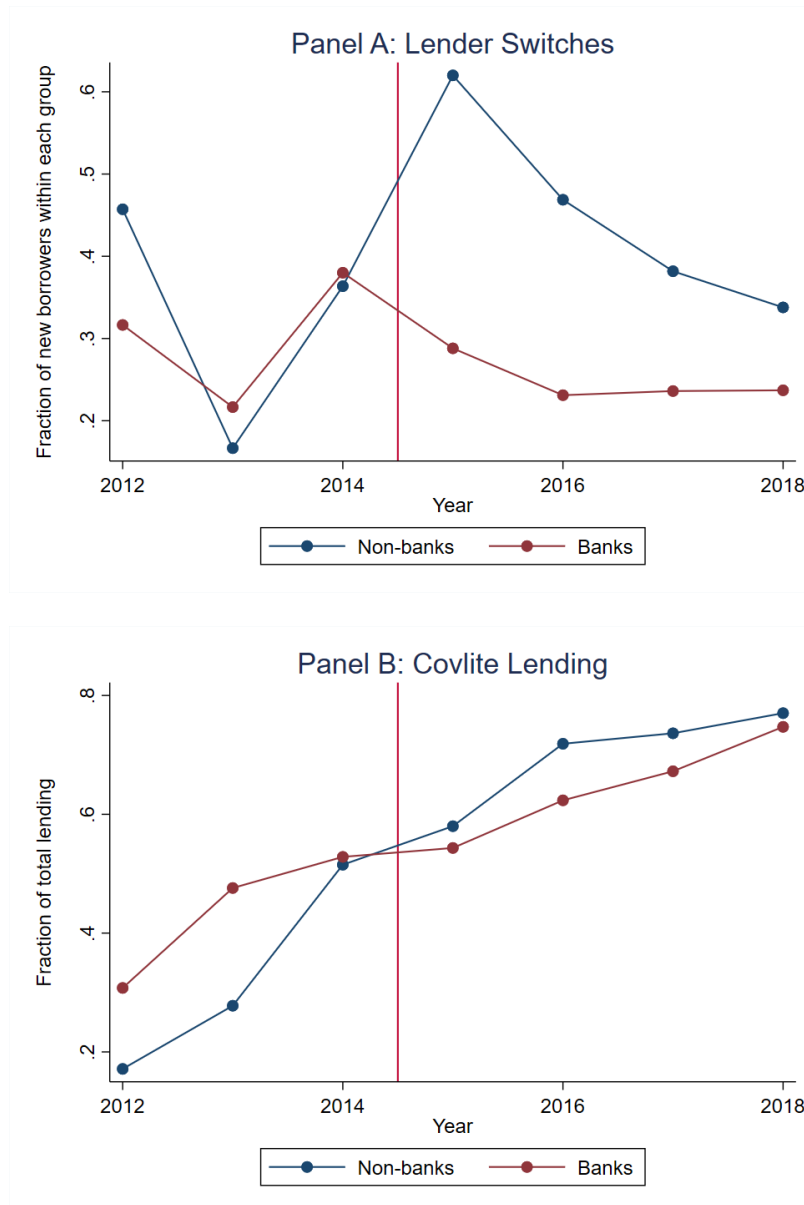


Table 1

Summary Statistics

The table reports descriptive statistics for our sample of leveraged loans from LCD. The sample period spans from 2012 to 2018. Panel A refers to the full sample; Panel B contrasts bank and non-bank institutions; and 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 levels, 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

Empirical Facts: Non-Parametric Evidence

The table presents non-parametric evidence about borrower-lender matches before and after the Clarification (2014Q4). Panel A reports the frequency of borrowers transitioning from bank to non-bank lenders (and vice versa) when taking new loans. Panel B tabulates the fraction of corresponding covenant-lite loans. For each loan and the one directly preceding it, the table traces whether they are from a bank or a non-bank, and whether they are covenant-lite or non-covenant-lite. Both panels report tests 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 levels, respectively.

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

Panel B: Fraction of Covenant-Lite Lending									
Before the Clarification			After the Clarification			Changes in Covenant-Lite Fraction			
From	To: Bank	Non-Bank	From	To: Bank	Non-Bank	From	To: Bank	Non-bank	
Bank	19.8%	11.9%	Bank	56.9%	73.6%	Bank	37.1%***	61.7%***	(14.33)
Non-Bank	35.7%	5.6%	Non-Bank	57.2%	62.8%	Non-Bank	21.5%***	57.1%***	(3.8)

Table 3

Lender Switching after the Clarification

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching to a new lender. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. The outcome variable is “New Lender”, an indicator taking a value of one for a loan between a borrower and a lender without previous interactions. “Bank Borrower” is an indicator variable for borrowers whose last loan was with a regulated, commercial US bank. “Post” is an indicator for all quarters after 2014Q4. All variables are defined in Appendix Table A.1. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively.

	New Lender							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Bank Borrower	0.140** (0.071)	0.136** (0.069)	0.216*** (0.082)	0.220*** (0.081)	0.179** (0.086)	0.182** (0.086)	0.235** (0.092)	0.371*** (0.117)
Bank Borrower	-0.174*** (0.057)	-0.163*** (0.055)	-0.182*** (0.064)	-0.177*** (0.064)	-0.134* (0.078)	-0.138* (0.078)	-0.150* (0.079)	-0.279*** (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

Covenant-Lite Loans and Relationship Lenders

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of being offered a covenant-lite loan. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. The outcome variable is “Covlite Offer”, the proportion of covenant-lite loans given out by borrower b 's relationship lender to all other borrowers at time t . “Bank Borrower” is an indicator variable for borrowers whose last loan was with a regulated, commercial US bank. “Post” is an indicator for all quarters after 2014Q4. All variables are defined in Appendix Table A.1. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Covlite Offer							
Post × Bank Borrower	-0.216*** (0.022)	-0.214*** (0.020)	-0.176*** (0.021)	-0.175*** (0.021)	-0.173*** (0.026)	-0.170*** (0.025)	-0.162*** (0.025)	-0.193*** (0.031)
Bank Borrower	0.190*** (0.017)	0.197*** (0.015)	0.180*** (0.017)	0.178*** (0.017)	0.186*** (0.023)	0.185*** (0.022)	0.180*** (0.022)	0.217*** (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
R ²	0.26	0.46	0.55	0.56	0.74	0.74	0.75	0.88

Table 5**Lender Switching after the Clarification: Sub-Segments of the Leveraged Loan Market**

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the likelihood of switching to a new lender based on the loan terms offered. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. The outcome variable in Column (1) is “Any New Lender”, the same as in Table 3. The outcome variable in Column (2) redefines “New Lender” by dropping all observations for new lenders that are not covenant-lite. Analogously, in Column (3), “New Lender” is redefined by dropping all new lender observations that feature covenants. “Bank Borrower” is an indicator variable for borrowers whose last loan was with a regulated, commercial US bank. “Post” is an indicator for all quarters after 2014Q4. All variables are defined in Appendix Table A.1. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively.

	New Lender Defined as:		
	(1) Any New Lender	(2) Only CovLite	(3) Only NonCovLite
Post × Bank Borrower	0.371*** (0.117)	0.589*** (0.126)	0.216 (0.136)
Bank Borrower	-0.279*** (0.083)	-0.378*** (0.099)	-0.203** (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
Other Loan Terms after the Clarification

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected various loan terms. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. The outcome variables in Columns (1), (2), (3), and (4) refer to results for interest rate, maturity, collateral and deal size, respectively. “Bank Borrower” is an indicator variable for borrowers whose last loan was with a regulated, commercial US bank. “Post” is an indicator for all quarters after 2014Q4. All variables are defined in Appendix Table A.1. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively.

	Interest	Maturity	Collateral	Deal Size
	(1)	(2)	(3)	(4)
Post × Bank Borrower	-0.493*** (0.139)	-0.199*** (0.074)	0.019** (0.009)	-0.016 (0.037)
Bank Borrower	-0.401*** (0.122)	0.036 (0.067)	0.006 (0.008)	0.275*** (0.026)
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	4453	4460	4460	4460
R ²	0.82	0.80	0.74	0.85

Table 7

Market Structure: Extensive and Intensive Margin

The table presents results from regressions of the choice of borrowers to leave the leveraged lending market or switch to non-banks on borrower and loan characteristics prior to the Clarification. The sample includes all loans prior to the Clarification. The outcome variable in Columns (1) to (3) is an indicator for whether borrowers stop taking out any more leveraged loans after the Clarification, in which case we assume they left the leveraged lending market. The outcome variable in Columns (4) to (6) is an indicator for whether, conditional on staying in the leveraged loan market, borrowers switch 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 levels, respectively.

	Borrowers Leaving the Market			Borrowers Switching to Non-Banks		
	(1)	(2)	(3)	(4)	(5)	(6)
Credit rating - Pre-period	0.053 *** (0.015)			0.025 *** (0.010)		
Number of leveraged Loans - Pre-period		-0.004 (0.012)			0.019 *** (0.009)	
Interest - Pre-period			0.033 *** (0.009)			0.006 *** (0.002)
Covlite - Pre-period			-0.013 (0.037)			0.026 *** (0.012)
Collateral - Pre-period			0.185* (0.098)			-0.007 (0.023)
Maturity - Pre-period			-0.007 (0.011)			0.001 (0.004)
Deal size - Pre-period			-0.031* (0.017)			-0.005 (0.004)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1739	1739	1739	1712	1712	1712
R ²	0.08	0.07	0.10	0.07	0.08	0.07

Table 8**Market Structure: Heterogeneity in Borrower and Lender Response**

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the likelihood of switching to a new lender based on the loan terms offered. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Column (1) investigates the cross-sectional impact for borrowers with demonstrated covenant-lite loan demand. *CovLite Demand* is measured as an indicator for borrowers whose last loan prior to the Clarification was covenant-lite. Column (2) the cross-sectional impact for banks with demonstrated high covenant-lite loan supply. *CovLite Supply* is measured as an indicator for lenders that have issued an above-median fraction of covenant-lite loans in the five years prior to the Clarification. “Bank Borrower” is an indicator variable for borrowers whose last loan was with a regulated, commercial US bank. “Post” is an indicator for all quarters after 2014Q4. All variables are defined in Appendix Table A.1. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively.

	New Lender	
	(1)	(2)
Bank Borrower	-0.263*** (0.084)	-0.422*** (0.107)
Post × Bank Borrower	0.373*** (0.116)	0.459*** (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 the Clarification on Lender-Market Shares and Covenant-Lite Lending**

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected lender-level covenant-lite loan offer and bank market share in a given quarter. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. “Covlite Share” is the fraction of covenant-lite loans offered by lenders. “Bank” is an indicator for regulated commercial lenders. “Market Share” is lender’s market share in the leveraged loan market. “Post” is an indicator for all quarters after 2014Q4. Controls include bank-quarter averages of interest rates, loan maturity, collateral, and credit ratings, weighted by loan amounts. All variables are defined in Appendix [Table A.1](#). 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 Share		Market Share	
	(1)	(2)	(3)	(4)
Post × Bank	-0.148*** (0.051)	-0.145*** (0.050)	-1.215* (0.647)	-1.210* (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 Borrower Quality 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 a value of one corresponds to the best rating group, and six to the worst. The dependent variable in Column (2) is an indicator for investment-grade borrowers, which is equal to one if the borrower has a investment-grade rating. Panel B considers changes in Debt-to-EBITDA ratios of borrowers, both in terms of the continuous ratios in Column (1), as well as an indicator for the most highly leveraged borrowers with Debt-to-EBITDA larger than six, in Column (2). The regressions in both Panels 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 natural logarithm 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). Controls include bank-quarter averages of interest rates, loan maturity, collateral, and credit ratings, weighted by loan amounts. All variables are defined in Appendix Table A.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Parentheses contain standard errors clustered by borrower (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 levels, respectively.

Panel A: Borrowers' Risk Profile		
	<u>Credit Rating (Numerical)</u>	<u>Investment Grade</u>
	(1)	(2)
Post × Bank Borrower	-0.072 (0.103)	0.004 (0.005)
Bank Borrower	0.040 (0.096)	-0.006 (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	Debt-To-EBITDA > 6
	(1)	(2)
Post × Bank Borrower	0.094 (0.233)	-0.575 (0.365)
Bank Borrower	-0.122 (0.122)	-0.331** (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 (0.037)	0.567 (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, and the shaded area represents 95% confidence intervals. Data on leveraged loans come from S&P's LCD database.

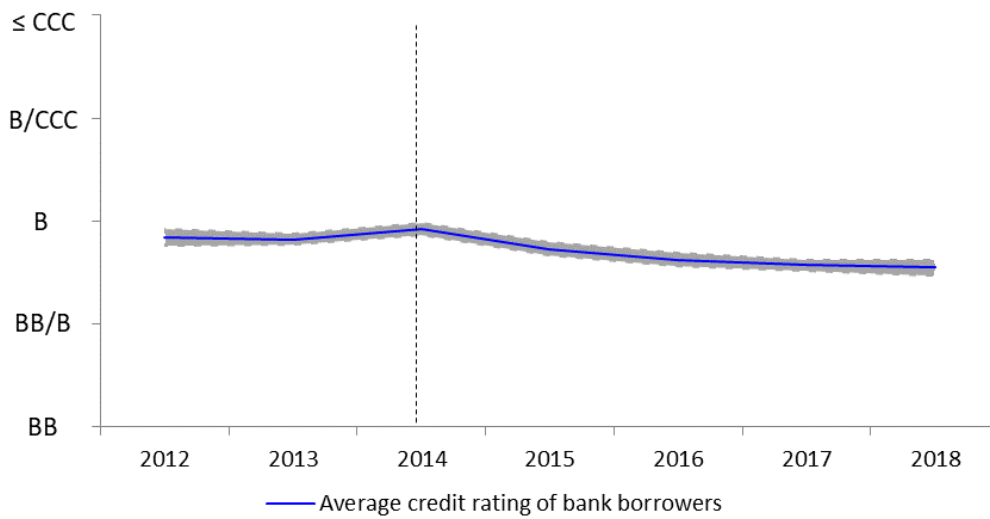


Table A.1
Variable Definitions

Variable	Definition
Bank Borrower	Indicator equal to one for borrowers whose last previous loan was issued by US and foreign banks supervised by the Federal Reserve, OCC, or FDIC, and zero in all other cases
Collateral	Average fraction of the deals that are backed by a specific asset and originated by a lender in a given quarter, weighted by the amount of loan
Covlite Share	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 Offer	The likelihood that a borrower's relationship lender would have offered a covenant-lite loan, measured as the fraction of 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
Credit rating	A categorical variable for each firm's credit rating level as provided by LCD, where one is assigned to the highest rating in the sample (BBB or higher) and six is the lowest rating category (unrated)
Debt to EBITDA	Borrower debt divided by EBITDA
Debt to EBITDA>6	Indicator if borrower debt divided by EBITDA exceeds six, a level of particular attention in the Leveraged Lending Clarification
Interest	Average all-in-drawn spread of the deals originated by lender in a given quarter weighted by the amount of loan

Market share	Dollar amount of loans originated by a lender / Total dollar amount of loans originated in the loan market in a given quarter
Maturity	Average maturity of the deals originated by a lender in a given quarter, weighted by the amount of a loan
Post	Indicator equal to one following the issuance of the Clarification in 2014Q4 and zero otherwise

Table A.2
Top-15 Lead Agents in Covenant-Lite Lending

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)					Panel B. After the Clarification (2014q4-2016q3)				
#	Lead Agent	N	%	mln	#	Lead Agent	N	%	mln
1	Credit Suisse	191	18.82	109.9	1	Credit Suisse	108	15.77	68.3
2	Bank of America	168	16.55	113.5	2	Bank of America	83	12.12	54.1
3	JP Morgan Chase	113	11.13	101.4	3	JP Morgan Chase	82	11.97	81.9
4	Deutsche Bank	102	10.05	71.6	4	Goldman Sachs	56	8.18	29.4
5	Morgan Stanley	79	7.78	37.8	5	Morgan Stanley	54	7.88	38.4
6	Barclays Bank	68	6.7	42.5	6	Deutsche Bank	51	7.45	34.7
7	Goldman Sachs	60	5.91	31.7	7	Jefferies Finance	45	6.57	18.2
8	Citigroup	59	5.81	44.2	8	Citigroup	43	6.28	45.7
9	UBS AG	44	4.33	17.5	9	Barclays Bank	39	5.69	30.6
10	Jefferies Finance	43	4.24	15.8	10	UBS AG	32	4.67	9.1
11	Royal Bank of Canada	40	3.94	9.6	11	Royal Bank of Canada	24	3.5	8.3
12	Wells Fargo	16	1.58	7.1	12	General Electric Capital Corp	15	2.19	4.5
13	General Electric Capital Corp.	8	0.79	2.7	13	Wells Fargo	11	1.61	4.0
14	SunTrust Bank	7	0.69	1.6	14	Antares Leveraged Capital Corp	9	1.31	1.9
15	BNP Paribas Group	3	0.3	0.9	15	Macquarie Capital	6	0.88	1.1

Table A.3

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 whose 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 levels respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	0.204** (0.092)	0.212** (0.093)	0.286*** (0.104)	0.380*** (0.126)
Bank Borrower	0.059 (0.062)	0.060 (0.062)	0.111* (0.067)	0.140 (0.096)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender \times 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.4**Robustness: Event Windows and Switching Lenders**

The table presents difference-in-differences estimates of the effect of the Leverage Lending Clarification (2014Q4) on borrowers switching lenders. We estimate the most complete specification from (Table 3, Column (4)) using a one-, two-, three-, and four-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. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels respectively.

	New Lender			
	1y	2y	3y	4y
	(1)	(2)	(3)	(4)
Post × Bank Borrower	1.094*** (0.331)	0.341* (0.180)	0.306** (0.134)	0.322*** (0.112)
Bank Borrower	-0.562*** (0.214)	-0.278** (0.112)	-0.191** (0.091)	-0.244*** (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.5**Robustness: Event Windows and Covenant-Lite Lending**

The table presents difference-in-differences estimates of the effect of the Leverage Lending Clarification (2014Q4) on the propensity to receive a covenant-lite loan offer. We estimate the most complete specification from (Table 4, Column (4)) using a one-, two-, three-, and four-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. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels respectively.

	Covlite Offer			
	1y	2y	3y	4y
	(1)	(2)	(3)	(4)
Post × Bank Borrower	-0.354*** (0.065)	-0.246*** (0.043)	-0.213*** (0.037)	-0.179*** (0.029)
Bank Borrower	0.267*** (0.062)	0.249*** (0.025)	0.215*** (0.020)	0.204*** (0.018)
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
Controls	Yes	Yes	Yes	Yes
Obs	1023	2288	3731	4885
R2	0.95	0.89	0.88	0.90

Table A.6**Placebo: Lender Switching 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 whose 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 levels 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 (0.358)	-0.432*** (0.148)	-0.256* (0.136)	-0.237** (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**Placebo: Covenant-Lite Loan Offers before the Clarification**

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of receiving covenant-lite loan offer. 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 whose 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 levels respectively.

	Covlite Offer			
	(1)	(2)	(3)	(4)
Bank Borrower × Post 2010Q4	0.263*** (0.034)			
Bank Borrower × Post 2011Q4		0.121*** (0.029)		
Bank Borrower × Post 2012Q4			0.116*** (0.030)	
Bank Borrower × Post 2013Q4				0.006 (0.037)
Bank Borrower	-0.071** (0.035)	0.086*** (0.025)	0.112*** (0.021)	0.182*** (0.018)
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	0.93	0.93	0.93	0.93

Table A.8**Robustness: Exclusion of 2014Q3 and Lender Switches**

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 out of the sample the third quarter of 2013 (which was marked by the issuance of the MRIA letters to Credit Suisse and other banks). Bank borrowers are borrowers whose 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 levels respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.192** (0.093)	0.194** (0.093)	0.234** (0.098)	0.360*** (0.119)
Bank Borrower	-0.140* (0.081)	-0.145* (0.081)	-0.143* (0.082)	-0.273*** (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.9**Robustness: Exclusion of 2014Q3 and Covenant-Lite Loan Offers**

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of receiving a covenant-lite loan offer. 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 out of the sample the third quarter of 2013 (which was marked by the issuance of the MRIA letters to Credit Suisse and other banks). Bank borrowers are borrowers whose 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 levels respectively.

	Covlite Offer			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	-0.171*** (0.027)	-0.168*** (0.026)	-0.163*** (0.026)	-0.198*** (0.032)
Bank Borrower	0.189*** (0.024)	0.188*** (0.023)	0.186*** (0.023)	0.223*** (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
Obs	4301	4301	4301	4300
R ²	0.74	0.74	0.75	0.88

Table A.10**Robustness: Lender Switches with Loan Level Controls (Table 3)**

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 whose last previous loan was with a commercial US bank. Controls include loan size, interest rate, maturity, and indicators for collateralized loans. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.182** (0.086)	0.178** (0.087)	0.231** (0.093)	0.367*** (0.118)
Bank Borrower	-0.138* (0.078)	-0.132* (0.078)	-0.145* (0.079)	-0.274*** (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.11**Robustness: Covenant-Lite Loan Offers with Loan Level Controls (Table 4)**

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 whose last previous loan was with a commercial US bank. Controls include loan size, interest rate, maturity, and indicators for collateralized loans. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

	Covlite offer			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	-0.173*** (0.025)	-0.169*** (0.025)	-0.162*** (0.025)	-0.193*** (0.031)
Bank Borrower	0.186*** (0.023)	0.185*** (0.022)	0.180*** (0.022)	0.218*** (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.12**Timing: *Initial Guidance* and Lender Switches**

The table presents difference-in-differences 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 non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers whose last previous loan was with a commercial US bank. Controls include loan size, interest rate, maturity, and indicators for collateralized loans. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post Guidance × Bank Borrower	-0.034 (0.114)	-0.029 (0.111)	-0.052 (0.115)	-0.063 (0.164)
Post Clarification × Bank Borrower	0.192** (0.097)	0.193** (0.096)	0.254** (0.102)	0.399*** (0.139)
Bank Borrower	-0.113 (0.106)	-0.120 (0.105)	-0.118 (0.110)	-0.244** (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.13**Timing: *Initial Guidance* and 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 non-financial borrowers in the years 2012 to 2018. Bank borrowers are borrowers whose last previous loan was with a commercial US bank. Controls include loan size, interest rate, maturity, and indicators for collateralized loans. Standard errors clustered by borrower and robust to heteroskedasticity are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

	Covlite Offer			
	(1)	(2)	(3)	(4)
Post Guidance × Bank Borrower	-0.022 (0.029)	-0.020 (0.028)	-0.022 (0.029)	0.015 (0.040)
Post Clarification × Bank Borrower	-0.165*** (0.029)	-0.162*** (0.028)	-0.154*** (0.028)	-0.199*** (0.037)
Bank Borrower	0.200*** (0.026)	0.197*** (0.025)	0.194*** (0.026)	0.209*** (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.14**Timing: Reversal after 2018**

The table presents difference-in-differences estimates of how the repeal of the Leverage Lending Guidance (2018Q4) affected the chance of being offered a covenant-lite loan and lender switching. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Bank borrowers are borrowers whose 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 levels respectively.

	Covlite Offer	New Lender
	(1)	(2)
Post Repeal \times Bank Borrower	0.085 (0.052)	-0.131 (0.217)
Post \times Bank Borrower	-0.203 *** (0.033)	0.386 *** (0.122)
Bank Borrower	0.216 *** (0.021)	-0.277 *** (0.084)
Borrower FE	Yes	Yes
Lender \times Time FE	Yes	Yes
Purpose FE	Yes	Yes
Rating FE	Yes	Yes
Controls	Yes	Yes
Obs	4460	4460
R ²	0.88	0.64

Table A.15**Robustness: Excluding Loans with Split Control Rights**

The table presents the same difference-in-differences estimates of the impact of the Clarification (2014Q4) as in Table 3, 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 whose 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 levels respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.168*** (0.031)	0.163*** (0.031)	0.143*** (0.031)	0.177*** (0.043)
Bank Borrower	-0.203*** (0.028)	-0.199*** (0.027)	-0.185*** (0.027)	-0.203*** (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.16

Effects for Different Sub-Segments of the Leveraged Lending Market: Other Loan Terms

The table presents difference-in-differences estimates for different sub-segments of lenders and loan terms. Column 1 (2) analyzes 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 whose last previous loan was with a commercial US bank. 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 levels respectively.

	New Lender							
	High rate (1)	Low rate (2)	High maturity (3)	Low maturity (4)	High collateral (5)	Low collateral (6)	High size (7)	Low size (8)
Post × Bank Borrower	0.434*** (0.122)	0.328** (0.136)	0.438*** (0.117)	0.376** (0.150)	0.060 (0.104)	0.389*** (0.116)	0.402*** (0.116)	0.329*** (0.135)
Bank Borrower	-0.340*** (0.094)	-0.210** (0.093)	-0.350*** (0.083)	-0.214* (0.120)	-0.092 (0.097)	-0.284*** (0.083)	-0.264*** (0.088)	-0.281*** (0.104)
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	3828	3645	3954	3496	3060	4406	3700	3760
R ²	0.70	0.57	0.63	0.58	0.60	0.63	0.53	0.70

Table A.17**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) and (2) is the bank's total market share (in percentages) in a given quarter; the dependent variable in Columns (3)-(4) is 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) and (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 versus 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 levels respectively.

	Bank Level: Market Share		Loan Level: 1(Loan from New Lenders)	
	(1)	(2)	(3)	(4)
Number of Covenants	-0.365 *** (0.094)	-0.250 *** (0.093)	-0.019** (0.009)	-0.023 *** (0.009)
Maturity		0.172* (0.094)		0.003 *** (0.001)
Interest		-0.105* (0.055)		0.012 (0.008)
Rating		-0.096 *** (0.019)		0.001 (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