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ORIGINAL ARTICLE OPEN ACCESS

A Note on the Use of Syndicated Loan Data

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ABSTRACT

Syndicated loan data provided by DealScan is an essential input in banking research to answer urging questions on bank lending, e.g., in the presence of financial or geopolitical shocks or climate change. However, many data options raise the question of how to choose the estimation sample. We employ a standard regression framework analyzing bank lending during the financial crisis of 2007/08 to study how conventional but varying usages of DealScan affect the estimates. The key finding is that the direction of coefficients remains relatively robust. However, statistical significance depends on the data and sampling choice, and we provide guidelines for applied research.

JEL Classification: C50, G15, G21

1 | Introduction

Past shocks, including the financial crisis starting in 2007/08, the exit of the United Kingdom from the European Union (“Brexit”), or recent geopolitical disruptions have shown the necessity to understand the transmission of shocks to the real sector via (international) banks (Ivashina and Scharfstein 2010a; Chodorow-Reich 2013; Cerutti et al. 2015; Kapan and Minoiu 2018; Doerr and Schaz 2021; Berg et al. 2021; Niepmann and Sheng 2024). The lack of data on banks’ (international) lending activities has significantly increased the interest in syndicated lending data provided by DealScan. A key feature of the database is the multitude of options to define sample and lending outcomes. Important and common decisions authors have to make are which syndicate members to retain in the sample or which loan types to consider. For example, depending on the question and/or data availability, one might keep only lead arrangers or lenders for which loan shares are available.

Our study employs a well-established laboratory to analyze the implications of such modeling choices banks and discusses the

economic reasoning for certain sample choices. The empirical set-up tests whether banks adjust lending during the financial crisis starting in 2007/08, depending on balance sheet characteristics like the tier 1 capital and deposit ratio. We contribute to the literature by highlighting how different sample selections using DealScan data affect the estimation results. We also provide upper and lower bounds of coefficient estimates across various specifications. We construct three samples as the basis for our analyses, varying in terms of which syndicate members are considered and how lead arrangers are defined (Ivashina 2009; Chakraborty et al. 2018; Doerr and Schaz 2021). The first two samples only include lead arrangers but vary in the definition of lead arrangers. In contrast, the third sample is based on all syndicate members, i.e., including participant lenders as well. We conduct various robustness tests for these three samples, which we identified to be the most commonly used in the literature. While each paper uses one option or the other, no study shows a structured analysis across all possible modeling choices.¹

We derive three main results. First, coefficient estimates are robustly comparable regarding the sign across the three

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baseline samples and modeling choices. Around 88% of estimates show the same sign across the different specifications when considering banks' lending response during the crisis, conditional on their capital ratios. For the deposit ratio interaction, the signs of the coefficient of interest coincide in 78% of cases. Second, the significance of coefficients varies depending on the sampling choice. Third, if a coefficient loses (or gains) significance, there is often reasoning provided by the sampling choice. Including participants next to lead arrangers, in particular, turns coefficients insignificant compared to the results obtained for the two samples focusing only on lead arrangers. This finding, however, applies to most variations for the sample of participant lenders and thus represents a consistent result in itself. Furthermore, narrowing loan observations down to specific loan types reduces sample size and results in changes in the significance of coefficients.

In sum, estimates are—across many definitions of the DealScan data—surprisingly robust, especially once the choice of whether to keep the entire syndicate or only lead arrangers has been made. The choice of whether to focus on lead arrangers or participants should be motivated by economic reasoning, as these two types of lenders have different roles in a syndicate. While lead arrangers are responsible for screening and monitoring borrowers, participant lenders provide additional capital but usually hold smaller shares in the total loan amount (Sufi 2007). Hence, participants' lending decisions should be less guided by risk considerations. The latter could explain why, in the full sample, including participants, the capital ratio loses relevance for lending decisions as the effects on risk become blurred. In this vein, our study provides insights to researchers on how specific usages of DealScan might affect coefficient estimates and offers structured guidance for possible modeling choices. The usage of DealScan data has increased after the global financial crisis as shown by Saharti et al. (2024) who also provide a breakdown of the number of publications related to syndicated loan data across journals. Especially given the heavy use of the data to answer urging questions on, for example, banks' responses to the sovereign debt crisis (Acharya et al. 2018), the Brexit (Berg et al. 2021), the Covid pandemic (Hasan et al. 2021) or their adjustments depending on climate risk exposures (Delis et al. 2024; Kacperczyk and Peydró 2021; Saharti et al. 2025; Sautner et al. 2025), a more structured analysis and understanding might be worthwhile.

The study is most related to the literature on banks' behavior during the financial crisis regarding lending responses. Seminal papers include the one by Ivashina and Scharfstein (2010a), who analyze the role of wholesale runs and credit line draw-downs on bank lending following the Lehman shock. Chodorow-Reich (2013) assesses, based on DealScan data, the role of credit market relationships for employment. Cerutti et al. (2015) find for the period 1995–2012 that syndicated loans constituted up to one-third of cross-border loans and confirm the draw-down of credit lines. Kapan and Minoiu (2018) show that being exposed to liquidity shocks during the financial crisis, banks maintained loan supply when having higher levels of common equity. Finally, when it comes to cross-border lending spillovers, studies are frequently based on syndicated lending data (e.g., De Haas and Van Horen 2012; Giannetti and Jang 2024).

Furthermore, we contribute to banking and finance studies by analyzing the robustness of results across various model specifications. For example, within the International Banking Research Network (IBRN), several studies used bank-level data from different central banks to study the same question on, e.g., the transmission of prudential or monetary shocks via banks' cross-border activities (Buch and Goldberg 2017; Buch et al. 2019). A meta-study of all results revealed consistent heterogeneity across country-specific findings. A recent study by Menkveld et al. (2024) analyzes results from the research outcome of 164 teams working independently and analyzing the same question on market efficiency based on the same data. The study reveals evidence of significant standard errors across the teams' results. Regarding DealScan data, a study that assesses differences in results across regions is Berg et al. (2016). The authors find differences in loan pricing structures in Europe compared to the United States (US) while the total borrowing costs resemble each other. Netter et al. (2011) show that when not applying conventional screens to data on mergers and acquisitions (M&As), results based on the consequently broader sample yield partially different conclusions. For example, less evidence is found for merger waves.

2 | Methodology and Data

This section first describes how we set up the regression model to estimate how banks adjust syndicate lending in times of a systemic shock depending on balance sheet characteristics. Second, we describe the core theme of our study: the different sample specifications we use to estimate the coefficients of interest. Third, we explain the data that underlies our estimations before presenting the results in the following section.

2.1 | Regression Equation

We use a straightforward research design to focus on the variation of results depending on the ingredients that enter into the estimations. We choose the emergence of liquidity strains in interbank markets in 2007, followed by the collapse of Lehman Brothers in 2008, as an unexpected event to analyze how banks adjust their syndicated lending volumes during the financial crisis. While we focus on the financial crisis, our setting could be applied to any research question on how unexpected shocks in the financial system transmit into banks' lending responses depending on banks' balance sheet strength. Equation (1) looks as follows:

$$y_{b,f,c,t} = \beta_1 z_{b,t-1} \times \text{Crisis}_t + \beta_2 z_{b,t-1} + \beta_3 X_{b,t-1} + \zeta_{b,f} + \zeta_{c,t} + \zeta_{f,t} + \varepsilon_{b,f,c,t}. \quad (1)$$

The dependent variable is the log of credit between bank b and firm f in quarter t with the bank being located in country c . Crisis_t divides the sample into a precrisis and crisis period and is a dummy variable being one from 2007 Q3 until 2009 Q2. The cut-off point at which the dummy variable turns to one corresponds to the unexpected emergence of liquidity strains in interbank markets. Following Cornett et al. (2011) or Kapan

and Minoiu (2018), we interact the financial crisis dummy with different bank balance sheet characteristics $z_{b,t-1}$ that are i) the risk-adjusted capital ratio or ii) the deposit ratio lagged by one quarter. We include a vector of control variables, $X_{b,t-1}$, that encompasses bank size, return on assets, the deposit ratio and the capital ratio.

We saturate the equation with bank-firm fixed effects ($\zeta_{b,f}$), country-time fixed effects ($\zeta_{c,t}$) as well as firm-time fixed effects ($\zeta_{f,t}$). The fixed effects absorb the single term $Crisis_t$ as well as they control for time-varying dynamics at the firm level. Country-time fixed effects are based on banks' location control for confounders, such as differences or adjustments in financial sector regulation across countries. $\varepsilon_{b,f,c,t}$ is the idiosyncratic error term. Standard errors are clustered at the bank level.²

2.2 | DealScan Variations

First, we specify three baseline samples following Chakraborty et al. (2018), Ivashina (2009) and Doerr and Schaz (2021).³ The first sample is limited to contain only the lead arranger(s), which are determined following the definition by Chakraborty et al. (2018).⁴ The second sample equally encompasses only the lead arranger(s). However, we define them following the definition by Ivashina (2009).⁵ The third sample comprises all lenders in the syndicate (e.g., Doerr and Schaz 2021).⁶

Second, we vary modeling choices across all of these three baseline samples. These tests are motivated by the data choices DealScan offers and the most common sample definitions in the related literature. While obviously, each study chooses the most appropriate tests for its purposes in isolation, we consider our paper complementary, providing a guideline on which options there are and how they might matter.

The following lists the tests we will conduct for the three baseline samples. The abbreviation in front of each test is used when we present estimation results⁷:

- Lead = 1.** Keep only facilities that have one lead arranger (if applicable) (Chakraborty et al. 2018; Schwert 2018)
- RS.** Keep only facilities that have more than one lender (Doerr and Schaz 2021)
- Lead < 11.** Keep only facilities that have less than 11 lead arrangers (if applicable)
 - AS.** Keep only loans for which the loan share is available in DealScan (Chu et al. 2019)
 - NFC.** Keep only nonfinancial borrowers (Doerr and Schaz 2021; Giometti and Pietrosanti 2022)
 - NFCP.** Keep only nonfinancial and private borrowers (Giannetti and Saidi 2019; Wix 2023)
 - CLT.** Keep only common loan types (i.e., credit lines and term loans) (Wix 2023)
 - CL.** Keep only credit lines (Berg et al. 2016; Doerr and Schaz 2021)

- TL.** Keep only term loans (Berg et al. 2016; Doerr and Schaz 2021)
- WP.** Keep only loans with a purpose that is either working capital or corporate purposes (Chodorow-Reich 2013)
- GP.** Keep only loans that can be considered general purpose loans (Giannetti and Saidi 2019)
- NTP.** Keep only loans that do not have a purpose of a takeover or acquisition (Chakraborty et al. 2018)
- CB.** Keep only commercial banks (Gatev and Strahan 2009)

2.3 | Data and Summary Statistics

We draw on two primary data sources. First, to obtain information on syndicated lending, we use data provided by DealScan. The sample spans the period from 2005 Q3 until 2009 Q2. The length of the global financial crisis is adopted from Cornett et al. (2011) such that the dummy variable takes on a value of one between 2007 Q3 and 2009 Q2 and zero otherwise. We select an equally long precrisis period. The loan-level data is aggregated at the ultimate parent level for banks and firms. We focus on banks in advanced economies, including the US and the European Union (EU), being part of a syndicate that provides credit to US and non-US firms.

We treat facilities as individual loans (see, e.g., Ferreira and Matos 2012). If applicable, we convert facility volumes to US\$ million utilizing the spot exchange rate that DealScan provides at loan origination. We allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, we distribute the facility amount equally among all lenders in the syndicate (De Haas and Van Horen 2013). On this basis, we consider the volume of loans at origination as provided by DealScan as an outcome variable.⁸

Second, we complement the data set by adding bank-level information from Compustat. Given that no common identifier exists between DealScan and Compustat, we rely on the link file provided by Schwert (2018). Compustat provides measures for bank size, profitability, risk-adjusted capital ratio, and deposit share. Table 1 provides a more detailed overview of variable descriptions. We require total assets to be non-negative and non-zero. Bank-level variables are winsorized at the 1st and 99th percentile to adjust for extreme outliers (Chen and Chen 2012; Kahle and Stulz 2013).

Table 2 shows summary statistics for the variables of interest for each of the three baseline samples and the related robustness tests summarized above. The average loan volume across the three baseline samples lies between US\$ 60 million and US\$ 64 million, whereas the average for $\ln(\text{loan volume})$ of about 2.8 is comparable across samples. In general, banks are well-capitalized compared to regulatory capital requirements, around 8%. Their average tier 1 capital ratio ranges between 8.4% and 9.3%. The capital ratio is, on average, a bit higher in the participant sample, which could reflect lower capital buffers of too-big-to-fail banks in the sample of lead arrangers. Deposit

TABLE 1 | Variable definitions.

| Variable | Description | Source | Data items |
|-----------------------------|--|-----------|------------|
| Loan volume | Newly originated loans in US\$ million between bank b and firm f in quarter t | DealScan | |
| Ln(loan volume) | Log of newly originated loans in US\$ million between bank b and firm f in quarter t | DealScan | |
| Crisis | Dummy variable that takes on a value of one between 2007 Q3 and 2009 Q2 and zero otherwise | | |
| <i>Bank characteristics</i> | | | |
| Size | Log of total assets | Compustat | Ln(atq) |
| ROA | Net income divided by total assets | Compustat | niq/atq |
| Tier 1 | Risk-adjusted capital ratio | Compustat | capr1q |
| Deposit | Total deposits divided by total assets | Compustat | dptcq/atq |

funding constitutes, on average, between 54% and 61% of total assets. Banks with a higher deposit ratio might be shielded more from wholesale funding runs during the financial crisis and consequent liquidity strains.

Figure 1 visualizes the number of observations across the three baseline samples and the corresponding subsamples underlying the modeling choice. This comparison yields relevant insights regarding the implication of a considered restriction for sample size. An obvious observation when considering the first three bars is that the baseline sample, including lead arrangers and participants, is considerably larger. Restricting the sample to, for example, those facilities with one lead arranger only ($Lead=1$) lowers sample size by around one-half. In contrast, the number of observations does not drop much if we keep facilities that have more than one lender (RS) or less than 11 lead arrangers ($Lead < 11$), which hence applies to most of the facilities. One of the most pronounced declines in sample size arises when we restrict the sample to loans for which the loan share is available in DealScan (AS). Restricting the sample in terms of borrower type or loan type causes a relevant decline in sample size once we keep only specific types of loans such as credit lines (CL) or term loans (TL). Finally, the share of commercial banks is so high that restricting lenders to be classified as commercial banks (CB) does not lead to a relevant decline in sample size. This observation also implies that when not limiting the sample to commercial banks, results might be interpreted as stemming from the commercial banking sector due to these banks' dominance as syndicate lenders.

In sum, the sample size is mainly affected by the choice to (i) (not) include participants or keep facilities with only one lead arranger, (ii) condition on the availability of the loan share, (iii) restrict the analysis to a specific loan type. The following analysis will reveal whether these choices result in differences regarding coefficient signs and significance.

3 | Results

We first show in Table 3 the regression results across the three baseline samples when interacting the crisis dummy with (i) the

capital ratio (Columns (1)-(3)) and (ii) the deposit ratio (Columns (4)-(6)). Then, we repeat the estimations for these three samples and the two interacting variables for the 13 tests outlined above. For better comparability, we plot the coefficient estimates (indicating their magnitudes and significance) across these iterations in Figure 2a,b.⁹

3.1 | Regression Results

Results in Columns (1) to (3) in Table 3 reveal that the interaction term between the financial crisis indicator and the lagged tier 1 capital ratio is positive. Hence, while capitalization seems to enter with a negative (but mostly insignificant) sign, better-capitalized banks tend to originate more loans in syndicated markets during the financial crisis. The latter result is significant in Columns (1) to (3), while the coefficient is only weakly significant when we consider the sample including the participants in Column (3). In principle, bank capitalization can relate to lending decisions differently. On the one hand, better-capitalized banks might have more buffer to expand lending (Chu et al. 2019). On the other hand, banks with low capital ratios have less equity at stake, which might increase risky lending activities. For example, Cerutti et al. (2015) find that syndicated lending declines with higher capital ratios, suggesting that low-capitalized banks use syndicated lending by having a small share in the total loan, which might be feasible despite their capital constraint. Similarly, we find in Columns (4) to (5) in Table 3 that a higher deposit ratio relates positively to lending during crisis times. The significant result vanishes in Column (6) for the sample, including all syndicate members. Our results suggest that during periods of financial stress, and especially for lead arrangers, higher capital and deposit ratios stabilize lending activities by US and EU banks in syndicated loan markets (Cornett et al. 2011).

3.2 | Modeling Choices

Our key contribution is to test the estimates for the three baseline samples through our proposed alternative specifications as outlined in Section 2. Figure 2a,b presents the effect size and significance of

TABLE 2 | Summary statistics.

| | Bank-firm sample | | | Periods | Loan volume | Ln(loan volume) | | Bank sample | | Tier 1 | | Deposits | | Size | | ROA | | |
|-----------|------------------|-------|------|---------|-------------|-----------------|-----|-------------|-------|--------|-----|----------|------|------|-----|------|------|------|
| | N | Firms | | | | Mean | SD | N | Banks | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean |
| Sample: | | | | | | | | | | | | | | | | | | |
| Baseline | 19098 | 3923 | 60.1 | 16 | 60.1 | 2.8 | 1.7 | 548 | 54 | 8.8 | 1.5 | 57.2 | 13.1 | 12.5 | 1.5 | 25.7 | 25.1 | |
| Lead = 1 | 9525 | 2845 | 42.4 | 16 | 42.4 | 2.6 | 1.5 | 437 | 44 | 8.7 | 1.4 | 56.9 | 12.6 | 12.5 | 1.5 | 25.7 | 24.7 | |
| RS | 16897 | 3548 | 60.6 | 16 | 60.6 | 2.9 | 1.7 | 524 | 52 | 8.7 | 1.4 | 56.7 | 13.1 | 12.6 | 1.4 | 25.4 | 25.5 | |
| Lead < 11 | 17025 | 3840 | 61.0 | 16 | 61.0 | 2.8 | 1.7 | 540 | 53 | 8.7 | 1.4 | 57.1 | 13.0 | 12.5 | 1.5 | 25.7 | 25.3 | |
| AS | 2121 | 475 | 48.6 | 16 | 48.6 | 2.7 | 1.7 | 322 | 39 | 8.4 | 1.1 | 54.1 | 13.1 | 13.1 | 1.1 | 25.8 | 20.9 | |
| NFC | 15387 | 3346 | 63.7 | 16 | 63.7 | 2.9 | 1.6 | 533 | 53 | 8.7 | 1.5 | 57.1 | 13.1 | 12.5 | 1.5 | 26.1 | 24.1 | |
| NFCP | 14214 | 3089 | 65.6 | 16 | 65.6 | 2.9 | 1.7 | 525 | 52 | 8.7 | 1.4 | 56.9 | 13.1 | 12.5 | 1.5 | 26.4 | 24.0 | |
| CLT | 15225 | 3539 | 59.1 | 16 | 59.1 | 2.8 | 1.6 | 541 | 53 | 8.7 | 1.4 | 57.1 | 13.1 | 12.5 | 1.5 | 25.6 | 25.1 | |
| CL | 2222 | 598 | 73.9 | 16 | 73.9 | 3.1 | 1.6 | 333 | 36 | 8.4 | 1.3 | 53.8 | 12.6 | 13.1 | 1.1 | 27.7 | 18.6 | |
| TL | 7104 | 1617 | 70.4 | 16 | 70.4 | 2.9 | 1.7 | 453 | 45 | 8.6 | 1.3 | 55.3 | 13.2 | 12.7 | 1.3 | 26.9 | 22.9 | |
| WP | 6677 | 1713 | 45.7 | 16 | 45.7 | 2.6 | 1.6 | 428 | 49 | 8.6 | 1.3 | 56.0 | 13.0 | 12.7 | 1.4 | 25.7 | 21.7 | |
| GP | 7462 | 1926 | 45.1 | 16 | 45.1 | 2.5 | 1.6 | 456 | 49 | 8.7 | 1.3 | 56.6 | 13.2 | 12.7 | 1.4 | 25.4 | 23.3 | |
| NTP | 6247 | 1439 | 76.2 | 16 | 76.2 | 3.0 | 1.7 | 402 | 46 | 8.5 | 1.3 | 54.8 | 12.8 | 12.8 | 1.4 | 26.4 | 21.8 | |
| CB | 16531 | 3192 | 61.2 | 16 | 61.2 | 2.8 | 1.7 | 513 | 48 | 8.7 | 1.4 | 57.1 | 13.1 | 12.6 | 1.3 | 26.1 | 23.1 | |
| Sample: | | | | | | | | | | | | | | | | | | |
| Baseline | 20363 | 3864 | 62.1 | 16 | 62.1 | 2.8 | 1.6 | 553 | 56 | 8.9 | 2.0 | 56.8 | 13.3 | 12.4 | 1.5 | 25.9 | 24.2 | |
| Lead=1 | 9051 | 2713 | 39.4 | 16 | 39.4 | 2.5 | 1.4 | 417 | 44 | 8.7 | 1.4 | 56.9 | 12.8 | 12.5 | 1.6 | 25.1 | 24.9 | |
| RS | 18302 | 3533 | 63.1 | 16 | 63.1 | 2.9 | 1.6 | 524 | 54 | 8.8 | 1.9 | 56.3 | 13.2 | 12.6 | 1.4 | 25.6 | 24.6 | |
| Lead < 11 | 16691 | 3704 | 64.7 | 16 | 64.7 | 2.8 | 1.6 | 525 | 56 | 8.8 | 1.7 | 56.8 | 13.1 | 12.5 | 1.5 | 25.5 | 24.5 | |
| AS | 1727 | 397 | 57.2 | 16 | 57.2 | 2.6 | 1.7 | 290 | 40 | 8.4 | 1.1 | 53.6 | 12.9 | 13.1 | 1.1 | 26.5 | 18.1 | |
| NFC | 16847 | 3305 | 65.5 | 16 | 65.5 | 2.9 | 1.6 | 527 | 55 | 8.8 | 1.8 | 56.6 | 13.2 | 12.5 | 1.5 | 26.2 | 23.3 | |
| NFCP | 15673 | 3051 | 67.4 | 16 | 67.4 | 3.0 | 1.6 | 519 | 53 | 8.8 | 1.8 | 56.6 | 13.2 | 12.5 | 1.5 | 26.4 | 23.2 | |
| CLT | 16370 | 3516 | 62.5 | 16 | 62.5 | 2.9 | 1.6 | 536 | 55 | 8.8 | 1.7 | 56.8 | 13.2 | 12.5 | 1.5 | 25.8 | 24.4 | |
| CL | 2468 | 605 | 99.9 | 16 | 99.9 | 3.2 | 1.6 | 343 | 37 | 8.5 | 1.2 | 53.8 | 12.8 | 13.0 | 1.1 | 26.6 | 18.7 | |
| TL | 7765 | 1608 | 69.4 | 16 | 69.4 | 3.0 | 1.7 | 431 | 48 | 8.7 | 1.7 | 54.9 | 13.4 | 12.7 | 1.4 | 27.6 | 21.9 | |
| WP | 7095 | 1733 | 44.4 | 16 | 44.4 | 2.6 | 1.6 | 442 | 51 | 8.8 | 1.9 | 56.0 | 13.2 | 12.7 | 1.4 | 25.5 | 21.7 | |
| GP | 7796 | 1934 | 44.6 | 16 | 44.6 | 2.6 | 1.6 | 463 | 51 | 8.8 | 1.9 | 56.3 | 13.1 | 12.6 | 1.4 | 25.3 | 23.4 | |
| NTP | 7090 | 1436 | 83.6 | 16 | 83.6 | 3.0 | 1.6 | 407 | 46 | 8.6 | 1.3 | 55.0 | 13.2 | 12.8 | 1.4 | 26.5 | 22.4 | |

Chakraborty's lead

Ivashina's lead

(Continues)

TABLE 2 | (Continued)

| | Bank-firm sample | | Periods | Loan volume | Ln(loan volume) | | Bank sample | | Tier 1 | | Deposits | | Size | | ROA | | | |
|----------|------------------|-------|---------|-------------|-----------------|-----|-------------|-------|--------|-----|----------|------|------|-----|------|------|------|----|
| | N | Firms | | | Mean | SD | N | Banks | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| | | | | | | | | | | | | | | | | | | |
| CB | 17679 | 3143 | 16 | 63.9 | 2.9 | 1.6 | 514 | 50 | 8.8 | 1.9 | 56.7 | 13.3 | 12.5 | 1.3 | 26.4 | 22.1 | | |
| Sample: | | | | | Participants | | | | | | | | | | | | | |
| Baseline | 72520 | 6203 | 16 | 54.1 | 2.9 | 1.5 | 843 | 69 | 9.3 | 2.2 | 60.5 | 13.3 | 11.7 | 1.8 | 22.6 | 29.1 | | |
| RS | 70274 | 5866 | 16 | 54.0 | 3.0 | 1.5 | 839 | 69 | 9.3 | 2.2 | 60.5 | 13.3 | 11.7 | 1.8 | 22.6 | 29.1 | | |
| AS | 8365 | 808 | 16 | 52.6 | 2.9 | 1.5 | 604 | 57 | 8.8 | 1.5 | 58.3 | 12.7 | 12.3 | 1.4 | 24.7 | 24.7 | | |
| NFC | 60408 | 5253 | 16 | 55.8 | 3.0 | 1.5 | 816 | 69 | 9.2 | 2.0 | 60.4 | 13.2 | 11.7 | 1.8 | 22.8 | 29.0 | | |
| NFCP | 56371 | 4857 | 16 | 56.7 | 3.0 | 1.5 | 807 | 68 | 9.2 | 2.0 | 60.3 | 13.2 | 11.7 | 1.8 | 23.1 | 28.6 | | |
| CLT | 64860 | 5793 | 16 | 52.2 | 3.0 | 1.4 | 836 | 68 | 9.3 | 2.1 | 60.5 | 13.3 | 11.7 | 1.8 | 22.6 | 29.2 | | |
| CL | 25021 | 2091 | 16 | 61.8 | 3.3 | 1.3 | 757 | 63 | 9.1 | 1.9 | 59.7 | 13.0 | 11.9 | 1.7 | 23.8 | 26.0 | | |
| TL | 22684 | 2704 | 16 | 55.7 | 2.9 | 1.6 | 697 | 62 | 8.9 | 1.8 | 58.9 | 13.0 | 12.1 | 1.6 | 24.4 | 25.9 | | |
| WP | 32631 | 2937 | 16 | 48.1 | 3.0 | 1.3 | 792 | 67 | 9.2 | 2.1 | 60.1 | 13.1 | 11.8 | 1.8 | 23.2 | 27.6 | | |
| GP | 36094 | 3310 | 16 | 48.4 | 3.0 | 1.4 | 802 | 67 | 9.2 | 2.1 | 60.2 | 13.1 | 11.8 | 1.8 | 23.3 | 27.4 | | |
| NTP | 20256 | 2125 | 16 | 63.9 | 2.9 | 1.6 | 655 | 62 | 8.9 | 1.6 | 58.6 | 13.0 | 12.1 | 1.6 | 25.1 | 24.3 | | |
| CB | 63464 | 5806 | 16 | 53.3 | 2.9 | 1.5 | 781 | 62 | 9.3 | 2.3 | 60.3 | 13.4 | 11.8 | 1.7 | 22.6 | 28.6 | | |

Note: This table shows summary statistics of all variables that we use, including the number of observations (N), mean, and standard deviation (SD). The dependent variables are defined at the bank-firm level, and the control variables are defined at the bank level. For each of the three baseline samples (*Baseline*) and the related robustness tests, we report the descriptives across the three panels (*Chakraborty's lead*; *Iwashina's lead*; *Participants*). The first column indicates to which robustness test the sample descriptives belong.

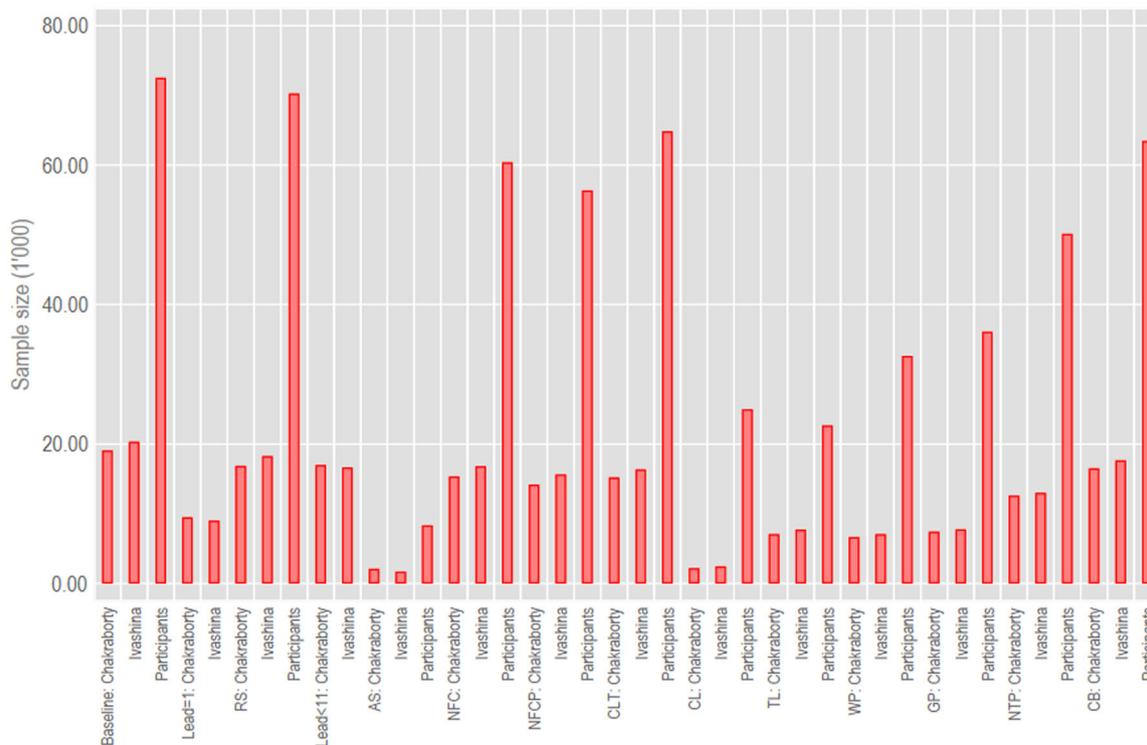


FIGURE 1 | Number of observations per sample. *Note:* This figure plots the number of observations from estimating Equation (1) for each of the three baseline samples (*Chakraborty's lead*; *Ivashina's lead*; *Participants*) and each robustness test, respectively. [Color figure can be viewed at wileyonlinelibrary.com]

the coefficient of the interaction term across different specifications. Figure 2a presents the ones when considering the interaction with the capital ratio and Figure 2b with the deposit ratio. Results based on the lead arranger definition by Chakraborty et al. (2018) are depicted by a circle, results based on the definition by Ivashina (2009) are depicted by squares, and those for the sample containing the entire syndicate by diamonds. The different colors followed by the abbreviation indicate the type of variation that we apply to re-estimate the model.¹⁰

For example, Figure 2a starts by depicting (in green color) the three coefficient estimates of the interaction term of the financial crisis dummy with the capital ratio in line with results shown in Table 3, Columns (1)–(3). We then re-estimate the model by keeping only facilities in the sample that have one lead arranger (*Lead=1*). Results are shown in olive green color.¹¹ In the following specification, we keep only facilities that have more than one lender (*RS*). Results are shown in a light violet color. We proceed like this and show estimates of all alternative specifications previously described.

Comparing results across specifications, we derive the following three main conclusions. First, the two figures reveal that the coefficient results are pretty robust regarding their signs. Only in 5 out of 40 cases the sign turns negative for the capital ratio in Figure 2a. In Figure 2b, the coefficient of the interaction term with the deposit ratio is positive for 31 out of 40 cases. Second, results in terms of significance are more mixed. Significant results emerge in 27 out of 40 cases in Figure 2a, respectively 19 out of 40 in Figure 2b.

Third, the deviation in the significance of the results is not random. For example, Figure 2a indicates that the

interaction term with the capital ratio becomes insignificant for selected alternative specifications. The first one relates to the third baseline sample containing the whole syndicate (marked by diamonds) where insignificant coefficients arise in 8 out of 12 cases.¹² This deviation is not a contradicting result as participants take a different role as lead arrangers. For example, Sufi (2007) finds that lead arrangers take the role of monitoring and screening and retain larger loan shares, especially for borrowers for which information asymmetries prevail to a larger extent.¹³ As a further test, we repeat our baseline estimation by controlling for the lead arrangers' loan share (Table A1). While our results remain robust, the coefficient on the lead arrangers' loan share is positive and significant which reflects the findings in the literature regarding the relevant role of lead arrangers in syndicate structures. The importance of analyzing the syndicate structure is also detected by Saharti et al. (2024), who find based on a literature review that next to the most common choice of outcomes variables being either lending volumes or spreads, the syndicate structure ranks third as an outcome variables in studies using syndicated loan data. Further, there is evidence highlighting differences in lead banks and participants that might result in heterogeneous reactions during crisis times (Ivashina 2009; Ivashina and Scharfstein 2010b). Furthermore, zooming into the two samples based on the lead arranger definitions, coefficients lose significance when we conduct alternative tests, resulting in a sizable decline in the number of observations (see also Figure 1). These cases refer to the robustness tests of keeping only loans for which the loan share is available (*AS*) or keeping only credit lines (*CL*). The pattern shown by Figure 2b confirms previous conclusions.

TABLE 3 | Results for the three baseline samples.

| Sample | Chakraborty's lead (1) | Ivashina's lead (2) | Participants (3) | Chakraborty's lead (4) | Ivashina's lead (5) | Participants (6) |
|--------------------|------------------------------|---------------------------|---------------------|------------------------------|---------------------------|---------------------|
| L.Tier 1 | -0.116* (0.059) | -0.034 (0.044) | -0.001 (0.011) | 0.100** (0.043) | 0.127** (0.056) | 0.011 (0.008) |
| Crisis × L.Tier 1 | 0.342*** (0.071) | 0.287*** (0.056) | 0.019* (0.011) | | | |
| L.Deposit | -0.005 (0.009) | 0.000 (0.008) | -0.000 (0.001) | -0.007 (0.011) | -0.003 (0.009) | -0.000 (0.002) |
| Crisis × L.Deposit | | | | 0.020** (0.008) | 0.016** (0.007) | -0.000 (0.001) |
| L.Size | 0.356 (0.282) | 0.706 (0.427) | 0.076 (0.046) | 0.214 (0.289) | 0.755 (0.560) | 0.073 (0.045) |
| L.ROA | -0.004 (0.005) | -0.005 (0.004) | -0.000 (0.000) | -0.003 (0.005) | -0.004 (0.003) | -0.000 (0.000) |
| Observations | 19098 | 20357 | 72520 | 19098 | 20357 | 72520 |
| Bank-Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm-Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Country-Time FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R^2 | 0.439 | 0.463 | 0.545 | 0.438 | 0.463 | 0.545 |
| Number of banks | 54 | 56 | 69 | 54 | 56 | 69 |
| Number of firms | 3923 | 3864 | 6203 | 3923 | 3864 | 6203 |
| Clustering | Bank | Bank | Bank | Bank | Bank | Bank |

Note: This table explores how banks adjust their lending following the global financial crisis, as specified in Equation (1). The dependent variable is the log of loans at the bank-firm-quarter level. $Crisis_t$ indicates the duration of the global financial crisis from 2007 Q3 until 2009 Q2. $Tier1_{h,t-1}$ is the risk-adjusted capital ratio lagged by one quarter. $Deposit_{h,t-1}$ is the ratio of total deposits to total assets, lagging by one quarter. We include lagged bank size, return on assets, and the deposit ratio (tier 1 ratio) in Columns (1) to (3) (Columns (4) to (6)) as controls. Standard errors are clustered at the bank level and reported in parentheses.

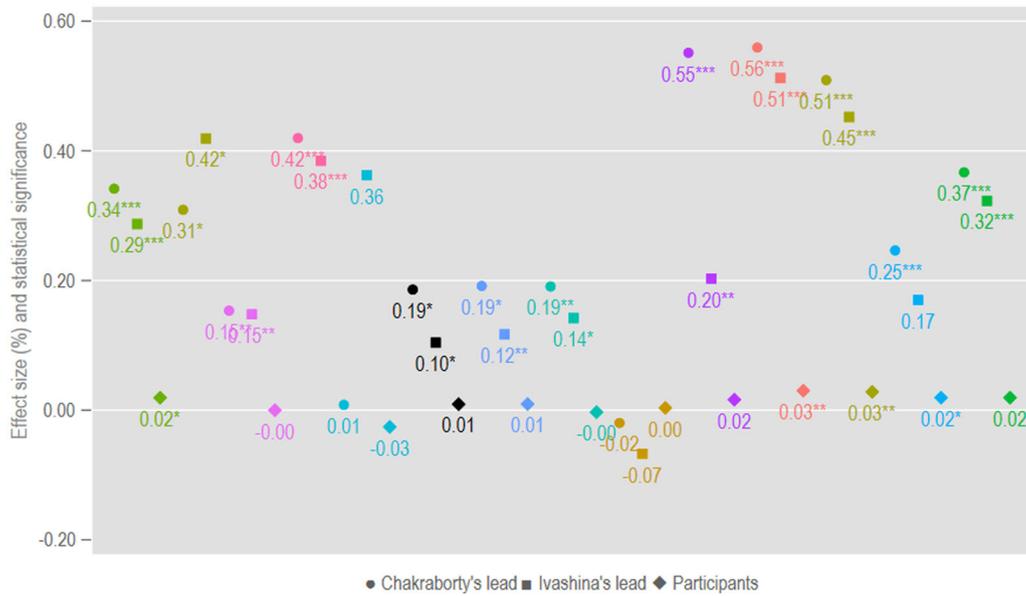
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

We summarize in Table 4 the different modeling choices by ordering them in the decision layers that arise from the structure of the DealScan database. The last column shows which choice has the most relevant effects on sample size and significance across and within the three baseline samples. A key decision is whether the study should be limited to lead arrangers or cover the entire syndicate. Including participants members could lead to different conclusions and the sample choice should thus be justified by the studied research question. For example, if the research interest is in how loan rates and quantities are set, then the focus would be on lead arrangers (maybe even the largest ones) making the relevant decisions. If, instead, the focus is on diversification aspects, including lead arrangers and the mostly smaller participants with no active role in the syndicate's decision making could make sense from an economic perspective. Most of the remaining choices underlying the conducted tests can be justified by data quality or the economics behind the research question. For example, a study focusing on commercial banks or credit lines obviously restricts the lender and loan type accordingly and might arrive at different conclusions compared to a study focusing on the role of term loans as a substitute for bond financing.¹⁴ Our results merely demonstrate that these decisions have implications for sample size and obtained results and thus need to be well-motivated.

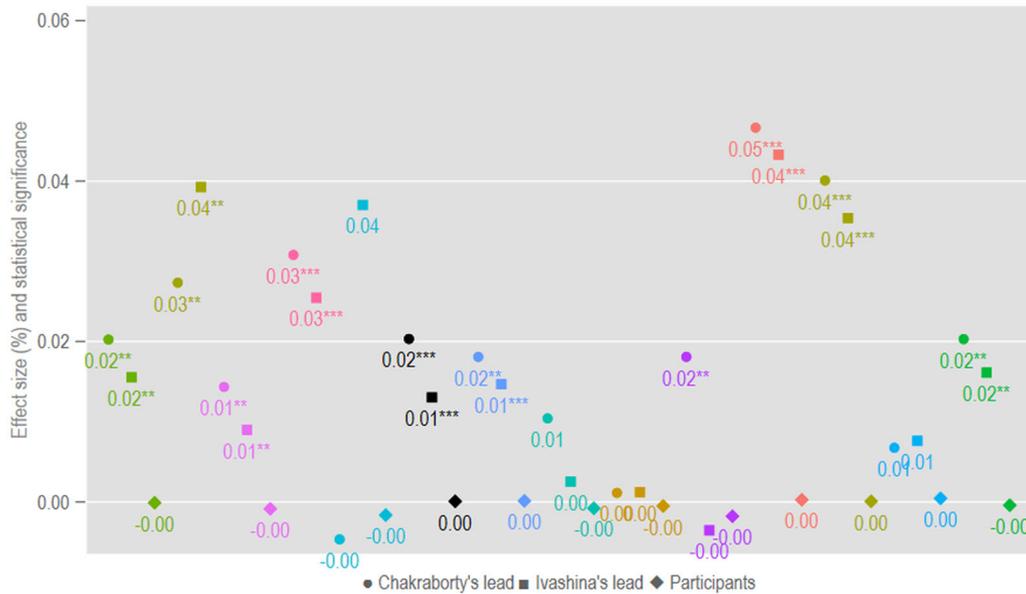
3.3 | Further Robustness

In further tests, we change the clustering scheme and cluster standard errors at the bank-firm level.¹⁵ The alternative clustering reduces the number of significant coefficients for the interaction term of the financial crisis dummy with both the capital and deposit ratio (Figure A2). Nevertheless, the main conclusions from the previous paragraph on “modeling choices” remain valid.

Additionally, we estimate the baseline models shown in Table 3 but employ an alternative rule to allocate loan shares. Again, we allocate loan shares according to the breakdown provided by DealScan. If this information is missing, lead arranger(s) and participants now receive 50% of the facility volume while equally subdividing within these two groups (De Haas and Van Horen 2013). Note that this approach results in differences in the loan amounts of participants depending on whether the lead arranger definition by Chakraborty et al. (2018) or Ivashina (2009) is used. Therefore, we show the results for the entire syndicate for both definitions in Figures A3 and A4. Especially for the interactions with the capital ratio, changing the allocation of the loan shares does not substantially change results regarding coefficient signs and significance compared to



(a) Tier 1 ratio



(b) Deposit ratio

FIGURE 2 | Coefficient estimates and confidence bands across sample specifications. (a) Tier 1 ratio, (b) Deposit ratio. *Note:* This figure plots the coefficients from estimating Equation (1) for the two interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each robustness test (color), respectively. The first three coefficients (Baseline) in each sub-figure correspond to the results presented in Table 3 for the baseline samples. We provide the value of the estimated coefficients and indicate statistical significance levels of 1, 5, and 10% by ***, **, and *, respectively. Standard errors are clustered at the bank level. [Color figure can be viewed at wileyonlinelibrary.com]

Figure 2a. Significance changes in the same instances as outlined above (i.e., for the sample including participants and certain restrictions on loan types). Moreover, the interactions with the deposit ratio change slightly but the differences are insignificant compared to Figure 2 when we employ the

alternative allocation rules. More critically, the estimates more often turn insignificant (8 vs. 10 with Chakraborty's lead definition and 6 vs. 9 with Ivashina's lead definition), making the comparison between sample choices for explaining deposit ratios more sensitive for these kinds of loan allocations.

TABLE 4 | Overview of key sampling and data choices.

| Type of choice | Dimensions | Far-reaching choice |
|-----------------------|---|----------------------------|
| Syndicate composition | <i>All syndicate members</i> | |
| | Lead arranger(s) | X |
| | Lead arranger(s) & participants | X |
| Facility composition | <i>All facility members</i> | |
| | Only if 1 lead arranger | |
| | Only if more than 1 lead arranger | |
| | Only if less than 11 lead arrangers | |
| | Only if loan shares available | X |
| Lender type | <i>All lenders</i> | |
| | Commercial banks | |
| | Investment banks | |
| | etc. | |
| Borrower type | <i>All borrowers</i> | |
| | Nonfinancial borrowers | |
| | Private borrowers | |
| | etc. | |
| Loan type | <i>All loans</i> | |
| | Common loan types (credit lines & term loans) | |
| | Credit lines | X |
| | Term loans | |
| | Working capital | |
| | General purpose loans | |
| | Loans for takeover/acquisition | |
| | etc. | |

4 | Conclusions

We assess the role of sampling choices for the sensitivity and magnitude of results when using syndicated loan data from DealScan. The estimation sample includes banks from advanced economies active in the syndicated market and the period from 2005 Q3 to 2009 Q2. Based on an established setting in the literature, we exploit the financial crisis as an exogenous shock and analyze banks' lending responses depending on balance sheet variables. We then compare results across many specifications derived from specifics of the DealScan data structure. Most importantly, we conduct the estimations based on three sample definitions regarding lead arrangers and participants, which the literature uses when drawing on syndicated loan data from DealScan. For these three baseline samples, we repeat the estimations for different data adjustments commonly used in related work, such as the choice of loan types.

The broad dimension of results we obtain from our approach helps detect three key patterns. First, the signs of the coefficient estimates are quite robust across the three samples and modeling choices. Second, significance varies and depends on the sampling choice. Third, the latter result is not random but goes back to the specific information content of the considered

test. For example, we consistently find significant differences when comparing results for lead arrangers only versus all syndicate members. Furthermore, filtering loan types reduces sample size and affects the significance of coefficient estimates.

Consequently, our results provide further insights into the usefulness of syndicated loan data provided by DealScan and reveal potential data avenues that researchers might choose, which might lead to diverging findings. Researchers should be careful when including only lead arrangers or participant members of a syndicate. Once this decision is made, our study supports the robustness of estimates obtained based on syndicated loan data irrespective of (the many) options DealScan data offers.

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Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

The data that support the findings can be obtained upon purchase of syndicated loan data from DealScan.

Endnotes

¹Similar to the literature on the transmission of shocks, we focus on the issuance of syndicated loans and not on the trading of these loans in secondary loan markets.

²While our focus is on the role of DealScan data choices for differences in results, in robustness tests, we also cluster standard errors at the bank-firm level.

³For example, a literature review on syndicated lending studies by Saharti et al. (2024) shows that the work by Victoria Ivashina ranks highest in terms of citation count. For alternative lead arranger definitions, see also Bharath et al. (2011) or Sufi (2007).

⁴Chakraborty et al. (2018) follow a ranking hierarchy and the lender in the syndicate with the highest rank is considered the lead agent: 1) lender is denoted as “Admin Agent,” 2) lender is denoted as “Lead bank,” 3) lender is denoted as “Lead arranger,” 4) lender is denoted as “Mandated lead arranger,” 5) lender is denoted as “Mandated arranger,” 6) lender is denoted as either “Arranger” or “Agent” and has a “yes” for the lead arranger credit, 7) lender is denoted as either “Arranger” or “Agent” and has a “no” for the lead arranger credit, 8) lender has a “yes” for the lead arranger credit but has a role other than those previously listed (“Participant” and “Secondary investor” are also excluded), 9) the lender has a “no” for the lead arranger credit but has a role other than those previously listed (“Participant” and “Secondary investor” are also excluded), and 10) lender is denoted as a “Participant” or “Secondary investor.”

⁵Ivashina (2009) defines the lead arranger(s) as follows: If identified, the administrative agent is defined to be the lead bank. If the syndicate does not have an administrative agent, then lenders that act as book runner, lead arranger, lead bank, lead manager, agent, or arranger are defined as the lead bank.

⁶We show the development of lending behaviors of lead arrangers and syndicate participants across time in Figure A1. The different timelines suggest that ignoring the syndicate participants does not determine the trend of syndicated lending for our sample period.

⁷Please note that it does not make logical sense to conduct some of the tests on the third sample that encompasses the full syndicate. These tests are indicated with “if applicable.”

⁸Chakraborty et al. (2018) and Doerr and Schaz (2021) undertake an alternative route and create a stock variable that captures the outstanding loan volume of each bank-firm pair. This approach requires a loan to enter a bank’s book from origination until maturity. Outstanding loan volumes could then be summed up each quarter per bank-firm pair to arrive at bank-firm-quarter as the observation level. However, Roberts (2015) or Bord and Santos (2012), for example, provide evidence that over the life of a loan, renegotiations might occur as well as banks might sell (part of) the loan such that loan volumes and loan shares might not be constant over time.

⁹We provide the underlying regression tables upon request.

¹⁰The legend provides more information on the selected specification and has to be read from left to right, while the ordering of specifications resembles the one in Section 2.

¹¹Note that this specification only applies to the two samples that depend on keeping the lead arranger(s).

¹²For the deposit ratio, the coefficient of the interaction term is consistently insignificant across all iterations (Figure 2b).

¹³Coordination problems and problems of asymmetric information might also arise in case of many lead arrangers. Restricting the sample to observations with less than 11 lead arrangers yields stronger results

in terms of magnitude and/or significance. This points into the direction of confounding effects if the number of lead banks is large.

¹⁴Examples of respective sample choices motivated by the research question are, among many others, Ferreira and Matos (2012) who focus on universal banks being lead arrangers and borrowers being nonfinancial private firms to study whether existing bank-firm links affect the probability that a lead arranger provides a loan, or Lim et al. (2014) studying the implications of having a non-bank lender in the syndicate for leveraged loans spreads.

¹⁵In unreported robustness checks, we have added further bank-level control including the cash-flow scaled by assets and the loan share in total assets, which did not change our main results though.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section.
Supplementary Information.