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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. This data is rich enough to answer urging questions on bank lending, e.g., in the presence of financial 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 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 seems to depend on the data and sampling choice.

Keywords: Syndicated Lending; DealScan; Scrutiny; Meta-Analysis

JEL Classification: C50; G15; G21;

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1 Introduction

The financial crisis starting in 2007/08 has 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). 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. For example, a common decision authors have to make is which syndicate members to retain in the sample or which loan types to consider.

Our study employs a well-established laboratory to analyze how banks adjust lending during the financial crisis 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 and we provide upper and lower bounds of coefficient estimates across various specifications. We specifically 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). For these three samples, we conduct various tests, 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 scrutiny analysis across all possible choices.

We derive three main results for our baseline sample. First, coefficient estimates are robustly comparable in terms of the sign. On average, 95% of estimates show the same sign when considering banks' lending response during the crisis conditional on their capital ratios. For the deposit ratio interaction, the sign of the coefficient coincides in 100% of cases. Second, the significance of coefficients can vary across specifications. This result holds either way: when looking at how the capital ratio matters for lending during the crisis, most coefficients show null results. Nevertheless, one can always find a case that yields significant estimates. Vice versa, we find mainly significant results for the deposit ratio interaction, whereas significance vanishes in a few circumstances. Third, if a coefficient significantly deviates from the others, there is often reasoning provided by the selected sample choice. For example, we observe that a sample containing only participant lenders might yield a consistently different result in terms of coefficient significance. The latter, however, applies to most variations for the sample of participant lenders and thus represents a consistent result in itself.

In sum, we consider our results a somewhat positive outcome. Estimates are – across many definitions of the DealScan data – surprisingly robust. At the same time, we show in further tests that the treatment of loan observations is relevant for these conclusions. In this vein, our study provides insights to researchers on how specific usages of DealScan might affect coefficient estimates and offer structured guidance for possible scrutiny tests. 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., 2019; Kacperczyk and Peydró, 2021), 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).

Furthermore, we contribute to banking and finance studies 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. (2021) 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 for 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. At the same time, the total borrowing costs resemble each other.

2 Methodology and Data

This section first describes how we set up the regression model to estimate how banks adjust syndicate lending during the financial crisis 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.

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 fall of Lehman Brothers (e.g., Chodorow-Reich, 2013; De Haas and Van Horen, 2013) as an unexpected event to analyze how banks adjust their syndicated lending volumes during the financial crisis. Equation (1) looks as follows:

$$\begin{aligned} \mathrm{y}_{b,f,t} = {} & \beta_1 \mathrm{z}_{b,t-1} \times \mathrm{Crisis}_t + \beta_2 \mathrm{z}_{b,t-1} \\ & + \beta_3 \mathrm{X}_{b,t-1} + \zeta_{b,f} + \zeta_{f,t} + \varepsilon_{b,f,t}. \end{aligned} \tag{1}$$

The dependent variable is the log of outstanding credit between bank b and firm f in quarter t. $Crisis_t$ divides the sample into a pre-crisis and crisis period. The cut-off point at which the dummy variable turns one is the third quarter of 2007, which corresponds to the failure of Lehman Brothers. Following Cornett et al. (2011) or Kapan and Minoiu (2018), we interact the financial crisis dummy with different balance sheet characteristics $z_{b,t-1}$ that are i) banks' risk-adjusted capital ratio or ii) their deposit ratio lagged by one quarter. We include a vector of control variables, $X_{b,t-1}$, that encompasses bank size, return on assets, as well as the respective other balance sheet characteristic, that is the deposit or capital ratio.

We saturate the equation with bank-firm fixed effects ($\zeta_{b,f}$) as well as firm-time fixed effects ($\zeta_{f,t}$). $\varepsilon_{b,f,t}$ is the idiosyncratic error term. The fixed effects absorb the single term $Crisis_t$. Standard errors are clustered at the bank level.[1]

DealScan variations First, we specify three baseline samples. The first sample is limited to contain only the lead arranger(s), which are determined following the definition by Chakraborty et al. (2018).[2] The second sample equally encompasses only lead arranger(s). However, we define them following the definition by Ivashina (2009).[3] The third sample comprises all lenders in the syndicate (e.g., Doerr and Schaz, 2021).

Second, we conduct scrutiny tests across all of these three baseline samples. These tests are motivated by the related literature, and we consider the most commonly applied robustness checks. The main difference is that the relevant papers do not show the complete set of

[1]In robustness tests, we also cluster standard errors at the bank-firm level.

[2]Chakraborty 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".

[3]Ivashina (2009) defines 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.

combinations of tests. 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.

We provide a list of the tests that we will conduct in the following[4]:

1. Keep only facilities that have one lead arranger (if applicable) (Chakraborty et al., 2018; Schwert, 2018)
2. Keep only facilities that have more than one lender (Doerr and Schaz, 2021)
3. Keep only facilities that have less than 11 lead arrangers (if applicable) (Giometti and Pietrosanti, 2019)
4. Keep only loans for which the loan share is available in DealScan (Chu et al., 2019)
5. Keep only non-financial borrowers (Doerr and Schaz, 2021)
6. Keep only non-financial and private borrowers (Giannetti and Saidi, 2019; Wix, 2017)
7. Keep only common loan types (i.e., credit lines and term loans) (Wix, 2017)
8. Keep only credit lines (Berg et al., 2016; Doerr and Schaz, 2021)
9. Keep only term loans (Berg et al., 2016; Doerr and Schaz, 2021)
10. Keep only loans with a purpose that is either working capital or corporate purposes (Chodorow-Reich, 2013)
11. Keep only loans that can be considered general purpose loans (Giannetti and Saidi, 2019)
12. Keep only loans that do not have a purpose of a takeover or acquisition (Chakraborty et al., 2018)
13. Keep only commercial banks (Gatev and Strahan, 2009)

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 pre-crisis period.[5] The loan-level data is aggregated at the ultimate parent level for banks and firms. We focus on

[4]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."

[5]In a robustness check, we consider a more prolonged crisis definition (2007 Q2 until 2010 Q1) and an equally extended pre-shock period.

US banks being part of a syndicate that provides credit to US and non-US firms. This choice reduces potential confounders, for example, due to differences in financial sector regulation across countries.

Table 1: Variable definitions

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

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 follow the most recent approach in the literature and use loan shares to create a stock variable that captures the outstanding loan volume of each bank-firm pair (Chakraborty et al., 2018; Doerr and Schaz, 2021). We follow this approach to remedy that DealScan captures loan information at loan origination. It implies that a loan enters a bank's book from origination until maturity. Outstanding loan volumes are then summed up each quarter per bank-firm pair to arrive at bank-firm-quarter as the observation level. In further analysis, we also investigate the implication of this sampling approach.

Second, we complement the dataset by adding bank-level information from Compustat. Given that there is no common identifier between DealScan and Compustat, we rely on the link file provided by Schwert (2018). Compustat provides measures for bank size, profitability,

Table 2: Summary statistics

Sample: *Variable*	Chakraborty's lead (1)	Ivashina's lead (2)	Participants (3)
Panel A: Loan characteristics			
Ln(loan volume) Mean	3.94	3.84	3.56
SD	1.27	1.37	1.11
Loan volume Mean	128.89	134.86	74.72
SD	301.11	343.39	170.48
N	3914	2411	24748
Panel B: Bank characteristics			
Size Mean	11.12	11.15	10.13
SD	1.74	1.59	2.09
ROA Mean	0.33	0.33	0.30
SD	0.14	0.13	0.15
Tier 1 Mean	9.10	9.13	9.74
SD	1.40	1.25	1.96
Deposit Mean	64.54	64.07	67.11
SD	9.31	9.57	10.04
N	26	26	47

Note: This table shows summary statistics of the dependent variable defined at the bank-firm level in Panel A and of the control variables at the bank level in Panel B for each of the three baseline samples respectively. All variables are reported as averages over the pre-crises period that ranges from 2005 Q3 to 2007 Q2.

deposit share, and risk-adjusted capital ratio. 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 different baseline samples. The average pre-crisis outstanding loan volume lies between US$ 75 million and US$ 135 million. The average loan volume in the sample encompassing the whole syndicates (Column (3)) is lower than in the two samples containing only lead arrangers (Columns (1) and (2)). The reason is that participants usually retain lower loan shares (Sufi, 2007). Irrespective of the underlying sample, banks are well-capitalized. Their average tier 1 capital ratio ranges between 9.10% and 9.74%. Capital requirements at that time stipulate a ratio of 8%. Deposit funding constitutes, on average, between 64% and 67% of total assets. Banks with a higher deposit ratio might be shielded more from wholesale funding runs during the financial crisis.

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 different specifications as outlined above. For better comparability, we plot the coefficient estimates surrounded by their 90% confidence bands across these iterations in Figure 1(a)-(b).[6]

[6] We provide the underlying regression tables upon request.

Table 3: Baseline

Sample:	Chakraborty's lead (1)	Ivashina's lead (2)	Participants (3)	Chakraborty's lead (4)	Ivashina's lead (5)	Participants (6)
L.Tier 1 × Crisis	-0.032*	-0.019	-0.012**			
	(0.019)	(0.016)	(0.005)			
L.Deposit × Crisis				-0.003*	-0.005***	-0.001**
				(0.002)	(0.001)	(0.001)
L.Size	-0.065**	-0.049	-0.004	-0.058**	-0.045	-0.001
	(0.030)	(0.057)	(0.026)	(0.027)	(0.054)	(0.025)
L.ROA	0.007	0.037	0.014	0.004	0.027	0.010
	(0.031)	(0.037)	(0.011)	(0.025)	(0.031)	(0.010)
L.Tier 1	0.008	0.005	0.000	-0.021***	-0.012**	-0.010**
	(0.019)	(0.015)	(0.008)	(0.006)	(0.005)	(0.005)
L.Deposit	-0.000	0.001	-0.000	0.001	0.003	0.000
	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
Observations	18,951	23,794	324,480	18,951	23,794	324,480
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.936	0.942	0.942	0.936	0.942	0.942
Number of banks	26	27	50	26	27	50
Number of firms	983	1,185	7,473	983	1,185	7,473
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

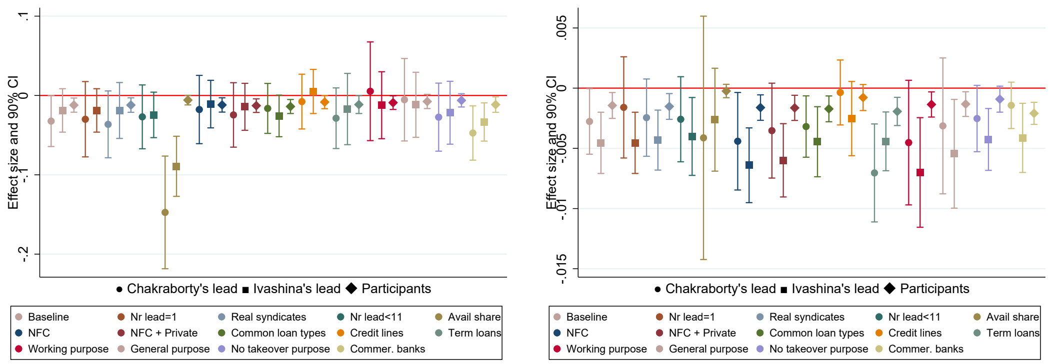
Note: This table explores how banks adjust their lending following the global financial crisis, as specified in Equation 1. The dependent variable is the log of outstanding loans at the bank-firm-quarter level. $Crisis_t$ indicates the duration of the global financial crisis from 2007 Q3 until 2009 Q2. $Tier\ 1_{b,t-1}$ is the risk-adjusted capital ratio lagged by one quarter. $Deposit_{b,t-1}$ is the ratio of total deposits to total assets and it is lagged by one quarter. We include lagged bank size, return on assets, as well as 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 negative. Hence, while capitalization seems to enter with a positive (but insignificant) sign, better-capitalized banks tend to lend less in syndicated markets during the financial crisis. The latter result is significant in Columns (1) and (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. 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 make use of 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 (6) in Table 3 that a higher deposit ratio relates positively to lending. However, the effects are mitigated during crisis times. This result might indicate that banks with higher capital and deposit ratios behaved more prudently and retracted from (often international) syndicated loan markets during the financial crisis. Furthermore, these banks applied less likely for TARP funding due to their more solid balance sheets (Duchin and Sosyura, 2012), without possible stimulating effects as concerns lending (Duchin and Sosyura, 2014; Berger et al., 2019).

Our key contribution is to test the estimates for the three baseline samples through our proposed alternative sample specifications as outlined in Section 2. Figures 1(a)-(b) present the effect size of the coefficient of the interaction term across different specifications. Figure 1(a) presents the ones when considering the interaction with the capital ratio and Figure 1(b) with the deposit ratio respectively. 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 full syndicate by diamonds. The different colors indicate the type of variation that we apply to re-estimate the model.[7]

Figure 1: 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 scrutiny test (colour), respectively. The first three coefficients (Baseline) in each sub-figure correspond to the results presented in Table 3 for the baseline samples. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure 1(a) starts by depicting the three coefficient estimates of the interaction term with the capital ratio in line with results in Table 3, Columns (1)-(3). In the second step, we keep only facilities in the sample that have one lead arranger ("Nr lead=1"). Results are shown in reddish color.[8] We proceed like this and show estimates of all alternative specifications previously described.

Comparing results across specifications, we derive the following main conclusions. First, the two figures reveal that the coefficient results regarding their signs are pretty robust. Only in 2 out of 40 cases, the sign turns positive for the capital ratio in Figure 1(a). In Figure 1(b), the coefficient of the interaction term with the deposit ratio is always negative.

Second, also in terms of significance, results seem quite robust. For example, they show a high fraction of null results for the capital ratio (27/40). A significant result appears across

[7] The legend provides more information on the selected specification and has to be read from left to right, while the ordering resembles the bullet points in Section 2.

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

most specifications for the interaction with the deposit ratio, excluding 15 out of 40 cases.

Third, the deviation in the significance of the results is not random. For example, Figure 1(a) indicates that the interaction term with the capital ratio becomes significantly negative if we consider the sample containing the whole syndicate. This deviation is not a contradicting result. Participants take a different role as lead arrangers, and the sample size is more extensive, which might result in more variation. Also, 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). Again, the result remains consistent for all iterations based on the participant sample, with few insignificant cases.

In Figure 1(b), the coefficient loses significance when considering the interaction with the deposit ratio in selected cases. However, these results might not impede the general message but fit the selected samples' information content. For example, adding restrictions that reduce sample size might result in lower significance (e.g., when focusing on syndicates with one lead arranger ("Nr lead=1") or when keeping only loans for which the lead share is available in DealScan ("Avail share")).

In further tests, we change the clustering scheme and how loans between bank-firm pairs are treated. Regarding the latter point, we do not use the outstanding loan volumes but only look at the loan volumes at origination. The sample that results from this alternative approach is significantly smaller and consequently does not allow to go through all of the 13 specifications. Thus, we compare the baseline results from Table 3 with the findings obtained when running the same regression but only considering loans at origination. In this estimation, we additionally vary the chosen clustering scheme and compare results when clustering at the bank level with those obtained when there is no clustering of standard errors. This might be of relevance since the number of clusters turns relatively small when only loans at origination are considered.

Figure A1 in the Online Appendix shows results for the Tier 1 ratio in Panel A and the deposit ratio in Panel B. The left panels show results for the three baseline samples and outstanding loan volumes, the right panels show results when keeping only loans at origination. Considering the results for the baselines sample in the left panels, it turns out that the choice of clustering can result in confidence bands narrowing down once no clustering is applied. Comparing results in Panel A for the baseline (left side) and original structure (right side), it becomes visible that coefficient signs go in the same direction while there are relevant differences in terms of significance. When turning to the interaction with the deposit ratio in Panel B, differences in results become even more evident as previously significant coefficient estimates turn insignificant for the sample for which we retain loans only at origination (right side).

Additionally, we estimate the baseline models shown in Table 3 but include standard errors clustered at the bank-firm instead of the bank level, or we define the deposit ratio as in Cornett et al. (2011) by $Deposits_t/Assets_{t-1}$ (Figures A2-A3).[9] These tests do not affect our main conclusions in the case of the alternatively chosen clustering scheme. At the same time, there seems to be some level effect when changing the definition of the interacted deposit variable. Moreover, we employ an alternative rule to allocate loan shares: Again, we allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, lead arranger(s) and participants receive 50% of the facility volume, respectively, 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 full syndicate for both definitions in Figures A4 and A5, whereas the results remain their key pattern. However, coefficient estimates show a slight tendency to gain significance in the case of the sample defined following Chakraborty et al. (2018). Lastly, we allow for an extended crisis period following Kapan and Minoiu (2018) with results remaining mostly robust regarding the key pattern. Yet this does not rule out that changes in significance can occur for some coefficients (Figure A6).

[9] Banks' capital ratio is pre-constructed in Compustat such that we cannot show this robustness test for banks' tier 1 ratio.

In sum, our checks show that we observe relatively robust results across all iterations and sample choices. The only restriction is the change to a sampling structure where we consider loans at origination instead of the stock of outstanding loan volume. The significantly reduced number of observations by 11% to 37% might be one explanation for this result, which itself might point towards the trade-off between gaining variation versus changing sample structure when varying between the two approaches.

4 Conclusions

We use syndicated lending data from DealScan to analyze banks' lending responses depending on balance sheet variables exploiting the occurrence of the financial crisis as an exogenous event. Based on this established setting in the literature, we scrutinize our results across many specifications derived from specifics of the DealScan data structure. The baseline estimations are based on a sample of US banks active in the syndicated market and the period from 2005 Q3 to 2009 Q2. 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 samples. Second, the same holds for significance. Third, if some coefficients are significant while others are not (or vice versa), this is not random but goes back to the specific information content of the considered specification. For example, we consistently find differences in significance when comparing results for lead arrangers only versus all participants of a syndicate.

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 and that might lead to diverging findings such as the treatment and allocation of loans at origination. Nevertheless, depending on the chosen sampling method, 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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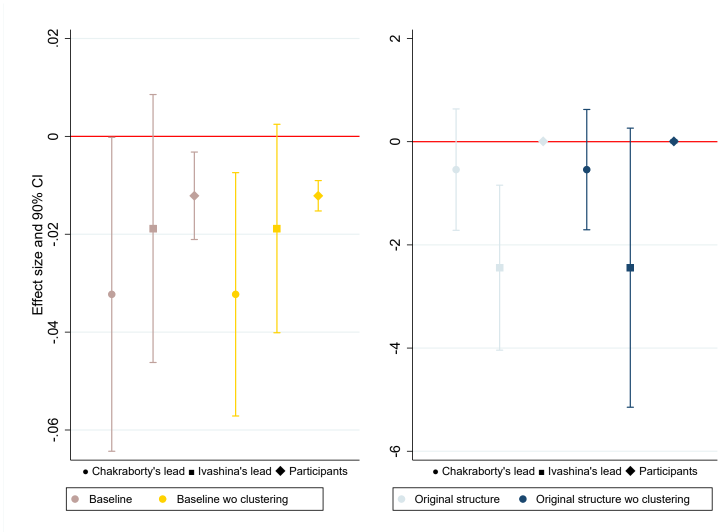
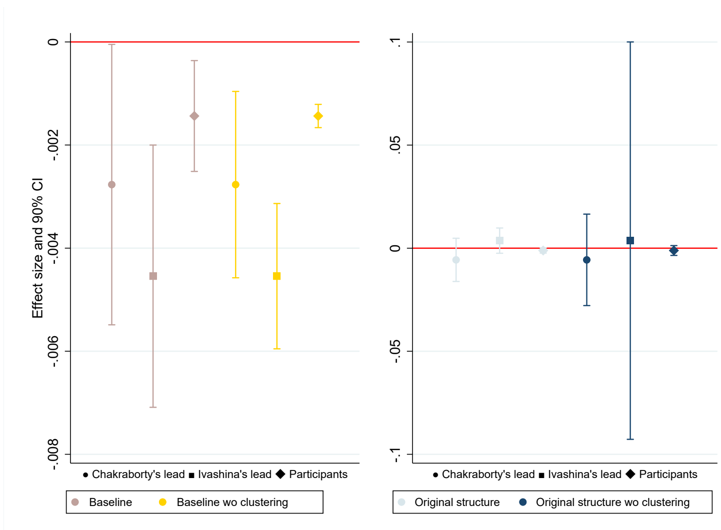
Online Appendix

Figure A1: Coefficient estimates and confidence bands across sample specifications: No clustering scheme and alternative sample structure



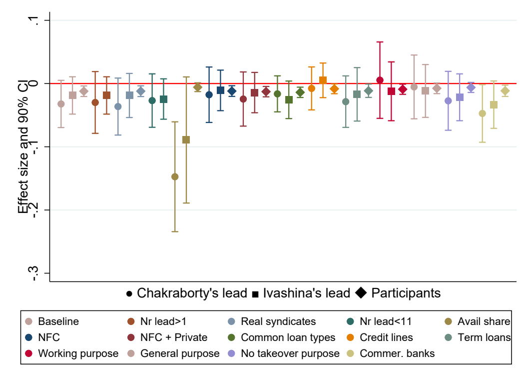
(a) Tier 1 ratio



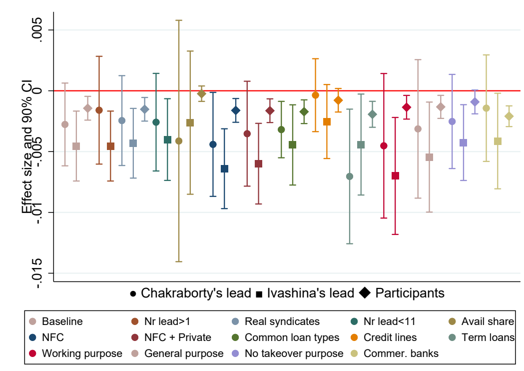
(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol), respectively. We alter whether a clustering scheme is applied (or not) and how the DealScan data is constructed (outstanding loans (baseline) versus loans at origination (original structure)). We show the 90% confidence intervals for each estimate. If a clustering scheme is applied, standard errors are clustered at the bank level.

Figure A2: Coefficient estimates and confidence bands across sample specifications: Alternative clustering



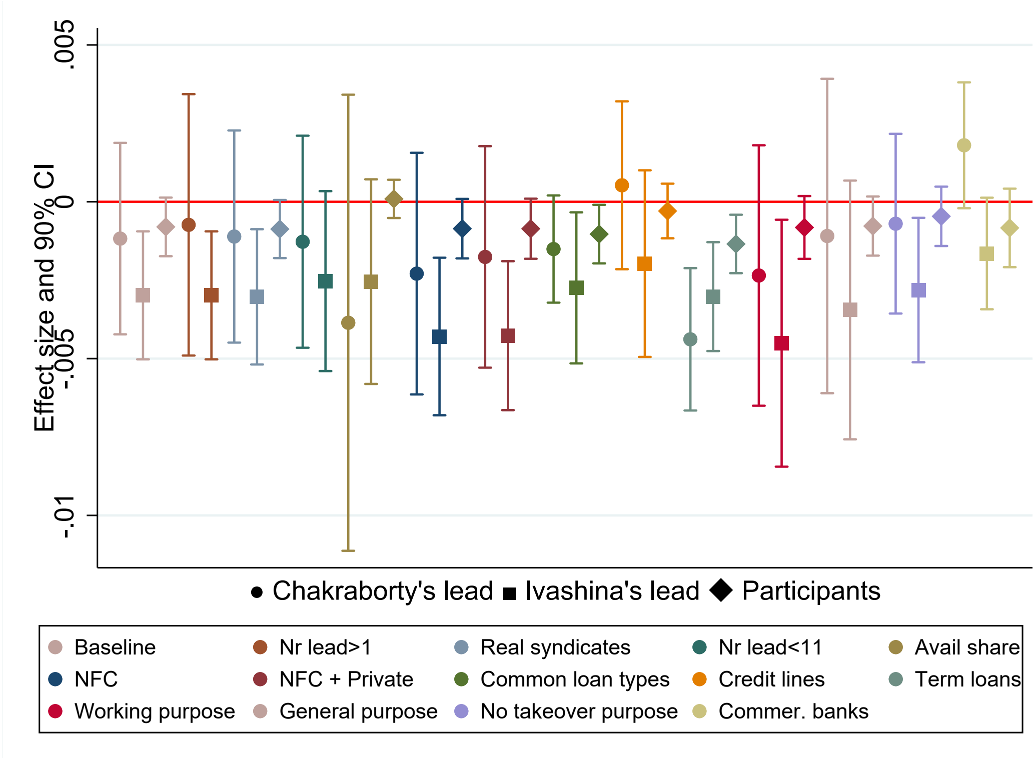
(a) Tier 1 ratio



(b) Deposit ratio

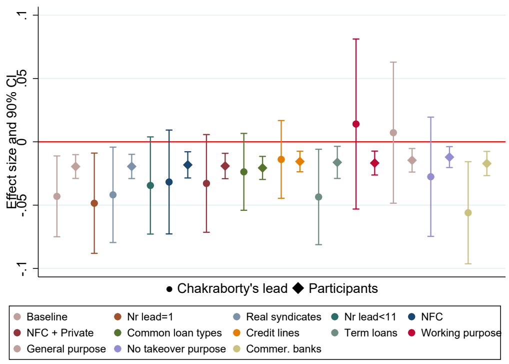
Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (colour), respectively. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank-firm level.

Figure A3: Coefficient estimates and confidence bands across sample specifications: Change in definition of deposit ratio

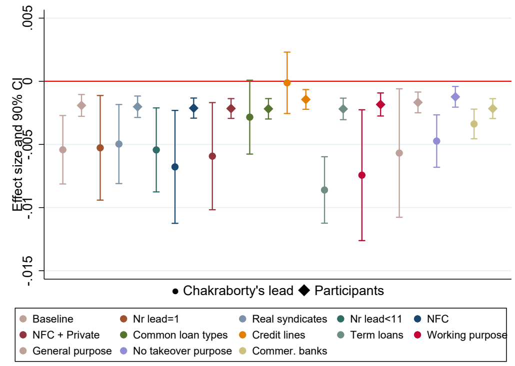


Note: This figure plots the coefficients from estimating Equation (1) for the interaction with banks' deposit ratio (defined as in Cornett et al. (2011)) as the independent variable for each sample (symbol) and each scrutiny test (colour), respectively. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure A4: Coefficient estimates and confidence bands across sample specifications: Alternative allocation rule



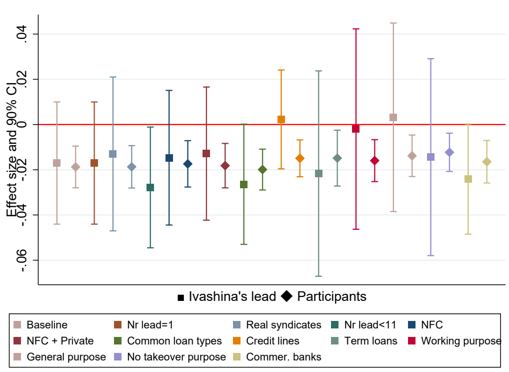
(a) Tier 1 ratio



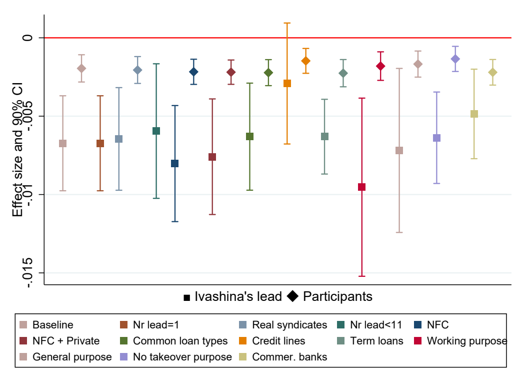
(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (colour), respectively. We allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, lead arranger(s) and participants receive 50% of the facility volume, respectively, while equally subdividing within these two groups. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure A5: Coefficient estimates and confidence bands across sample specifications: Alternative allocation rule



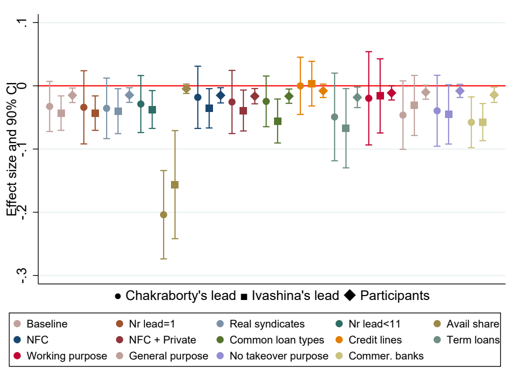
(a) Tier 1 ratio



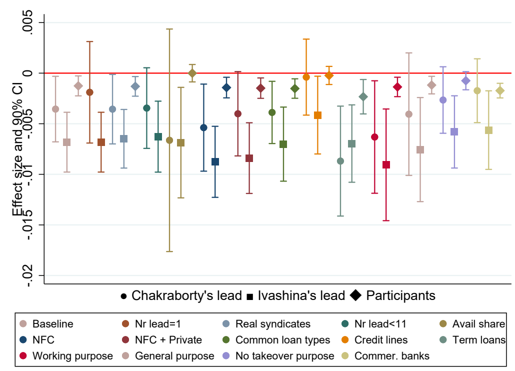
(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. We allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, lead arranger(s) and participants receive 50% of the facility volume, respectively, while equally subdividing within these two groups. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure A6: Coefficient estimates and confidence bands across sample specifications: Alternative crisis length



(a) Tier 1 ratio



(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (colour), respectively. The global financial crisis dates from 2007 Q3 until 2010 Q1. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.