IWH Halle Institute for Economic Research
Member of the Leibniz Association

Discussion Papers

No. 17
July 2022

A Note on the Use of Syndicated Loan Data

Isabella Müller, Felix Noth, Lena Tonzer

Authors

Isabella Müller
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Financial Markets
E-mail: isabella.mueller@iwh-halle.de

Felix Noth
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Financial Markets, and Otto von Guericke University Magdeburg
E-mail: felix.noth@iwh-halle.de
Tel +49 345 7753 702

Lena Tonzer
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Financial Markets, and Otto von Guericke University Magdeburg
E-mail: lena.tonzer@iwh-halle.de
Tel +49 345 7753 835

The responsibility for discussion papers lies solely with the individual authors. The views expressed herein do not necessarily represent those of IWH. The papers represent preliminary work and are circulated to encourage discussion with the authors. Citation of the discussion papers should account for their provisional character; a revised version may be available directly from the authors.

Comments and suggestions on the methods and results presented are welcome.

IWH Discussion Papers are indexed in RePEc-EconPapers and in ECONIS.

Editor

Halle Institute for Economic Research (IWH) – Member of the Leibniz Association

Address: Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany
Postal Address: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60
Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

A Note on the Use of Syndicated Loan Data*

First version: 07.07.2022
This version: 02.09.2024

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 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.

Keywords: DealScan, meta-analysis, scrutiny, syndicated lending

JEL classification: C50, G15, G21

* We thank Thomas Krause and Daniel Streitz for valuable comments and suggestions. All errors are our own.

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 starting in 2007/08 and 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). The first two samples only include lead arrangers but vary in the definition of lead arrangers, while the third sample is based on all syndicate members, i.e., including participant lenders as well. For these three samples, we conduct various scrutiny 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. First, across the three baseline samples and scrutiny tests, coefficient estimates are robustly comparable in terms of the sign. 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. Especially the inclusion of participants next to lead arrangers 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 full syndicate or only lead arrangers has been made. 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 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., 2024; 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; Giannetti and Jang, 2024).

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. (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 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 (US). 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 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.

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:

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

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 pre-crisis 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 one corresponds to the unexpeted 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, 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 ($\zeta_{b,f}$), country-time fixed effects ($\zeta_{c,t}$) as well as firm-time fixed effects ($\zeta_{f,t}$). The fixed effects absorb the single term $Crisis_t$. Country-time fixed effects 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.[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 the lead arranger(s).

[1]While 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.

[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

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 data choices DealScan offers as well as 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.

In the following, we provide a list of the tests that we will conduct for each of the three baseline samples. The abbreviation in front of each test is used when we present estimation results[4]:

- 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 non-financial borrowers (Doerr and Schaz, 2021; Giometti and Pietrosanti, 2022)
- NFCP Keep only non-financial 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)

also excluded), and 10) lender is denoted as a "Participant" or "Secondary investor".

[3] Ivashina (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.

[4] Please 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."

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. 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.

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		
Bank characteristics			
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

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 as outcome the volume of loans at origination as provided by DealScan.[5]

[5]Chakraborty 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 would require that a loan enters 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

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, 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 different baseline samples and the related scrutiny tests summarized above. The average loan volume across the three baseline samples lies between US$ 19108 million and US$ 72525 million whereas the average for Ln(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 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 scrutiny tests. 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

(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.

Table 2: Summary statistics

	N	Firms	Periods	Loan volume	Ln(loan volume)		N	Banks	Tier 1		Deposits		Size		ROA	
	Bank-firm	sample			Mean	SD	Bank	sample	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Sample:	Chakraborty's lead															
Baseline	19108	3915	16	19108.0	2.8	1.7	549	54	8.8	1.5	57.2	13.1	12.5	1.5	25.7	25.1
Lead=1	9497	2838	16	9497.0	2.6	1.5	437	44	8.7	1.4	56.9	12.6	12.5	1.5	25.7	24.7
RS	16915	3543	16	16915.0	2.9	1.7	525	52	8.7	1.4	56.7	13.1	12.6	1.4	25.4	25.5
Lead¡11	17023	3833	16	17023.0	2.8	1.7	541	53	8.7	1.4	57.1	13.0	12.5	1.5	25.7	25.2
AS	2114	476	16	2114.0	2.7	1.7	324	39	8.4	1.1	54.0	13.1	13.1	1.1	25.8	20.8
NFC	15391	3338	16	15391.0	2.9	1.6	533	53	8.7	1.5	57.1	13.1	12.5	1.5	26.1	24.1
NFCP	14221	3080	16	14221.0	2.9	1.7	525	52	8.7	1.4	56.9	13.1	12.5	1.5	26.4	24.0
CLT	15254	3539	16	15254.0	2.8	1.6	542	53	8.8	1.4	57.2	13.1	12.5	1.5	25.6	25.1
CL	2215	592	16	2215.0	3.1	1.6	332	36	8.4	1.3	53.9	12.7	13.0	1.1	27.7	18.6
TL	7114	1625	16	7114.0	2.9	1.7	453	45	8.6	1.3	55.3	13.2	12.7	1.3	26.9	22.9
WP	6673	1711	16	6673.0	2.6	1.6	429	49	8.6	1.3	56.0	13.0	12.7	1.4	25.7	21.7
GP	7465	1926	16	7465.0	2.5	1.6	457	49	8.7	1.3	56.6	13.2	12.6	1.4	25.4	23.2
NTP	6232	1433	16	6232.0	3.0	1.7	402	46	8.5	1.3	54.8	12.8	12.8	1.4	26.4	21.8
CB	16553	3188	16	16553.0	2.8	1.7	514	48	8.7	1.4	57.1	13.2	12.6	1.3	26.1	23.1
Sample:	Ivashina's lead															
Baseline	20360	3857	16	20360.0	2.8	1.6	554	56	8.9	2.0	56.9	13.3	12.4	1.5	25.9	24.2
Lead=1	9025	2707	16	9025.0	2.5	1.4	417	44	8.7	1.4	56.9	12.8	12.5	1.6	25.1	24.9
RS	18305	3528	16	18305.0	2.9	1.6	524	54	8.8	1.9	56.3	13.2	12.6	1.4	25.6	24.6
Lead¡11	16685	3698	16	16685.0	2.8	1.6	525	56	8.8	1.7	56.8	13.1	12.5	1.5	25.5	24.5
AS	1726	398	16	1726.0	2.6	1.7	293	40	8.4	1.1	53.5	12.9	13.1	1.1	26.5	18.1
NFC	16832	3297	16	16832.0	2.9	1.6	527	55	8.8	1.8	56.6	13.2	12.5	1.5	26.2	23.3
NFCP	15667	3043	16	15667.0	3.0	1.6	519	53	8.8	1.8	56.6	13.2	12.5	1.5	26.4	23.2
CLT	16374	3515	16	16374.0	2.9	1.6	536	55	8.8	1.7	56.8	13.2	12.5	1.5	25.8	24.4
CL	2471	602	16	2471.0	3.2	1.6	343	37	8.5	1.2	53.8	12.8	13.0	1.1	26.6	18.7
TL	7779	1615	16	7779.0	3.0	1.6	431	48	8.7	1.7	54.8	13.4	12.7	1.4	27.7	21.8
WP	7099	1732	16	7099.0	2.6	1.6	443	51	8.8	1.9	56.0	13.1	12.7	1.4	25.5	21.7
GP	7811	1935	16	7811.0	2.6	1.6	464	51	8.8	1.9	56.4	13.1	12.6	1.4	25.4	23.4
NTP	7079	1429	16	7079.0	3.0	1.6	407	46	8.6	1.3	55.0	13.2	12.8	1.4	26.5	22.4
CB	17686	3140	16	17686.0	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	72525	6204	16	72525.0	2.9	1.5	843	69	9.3	2.2	60.5	13.3	11.7	1.8	22.6	29.1
RS	70279	5867	16	70279.0	3.0	1.4	839	69	9.3	2.2	60.5	13.3	11.7	1.8	22.6	29.1
AS	8367	809	16	8367.0	2.9	1.5	604	57	8.8	1.5	58.3	12.7	12.3	1.4	24.7	24.7
NFC	60407	5253	16	60407.0	3.0	1.5	816	69	9.2	2.0	60.4	13.2	11.7	1.8	22.8	29.0
NFCP	56370	4857	16	56370.0	3.0	1.5	807	68	9.2	2.0	60.3	13.2	11.7	1.8	23.1	28.6
CLT	64865	5794	16	64865.0	3.0	1.4	836	68	9.3	2.1	60.5	13.3	11.7	1.8	22.6	29.2
CL	25018	2091	16	25018.0	3.3	1.3	757	63	9.1	1.9	59.7	13.0	11.9	1.7	23.8	26.0
TL	22692	2707	16	22692.0	2.9	1.6	697	62	8.9	1.8	58.9	13.0	12.1	1.6	24.4	25.9
WP	32634	2938	16	32634.0	3.0	1.3	792	67	9.2	2.1	60.1	13.1	11.8	1.8	23.2	27.6
GP	36097	3311	16	36097.0	3.0	1.4	802	67	9.2	2.1	60.2	13.1	11.8	1.8	23.3	27.4
NTP	20257	2125	16	20257.0	2.9	1.6	655	62	8.9	1.6	58.6	13.0	12.1	1.6	25.1	24.3
CB	63467	5807	16	63467.0	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 scrutiny tests, we report the descriptives across the three panels (*Chakraborty's lead*; *Ivashina's lead*; *Participants*). The first column indicates to which scrutiny test the sample descriptives belong.

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 a commercial bank (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.

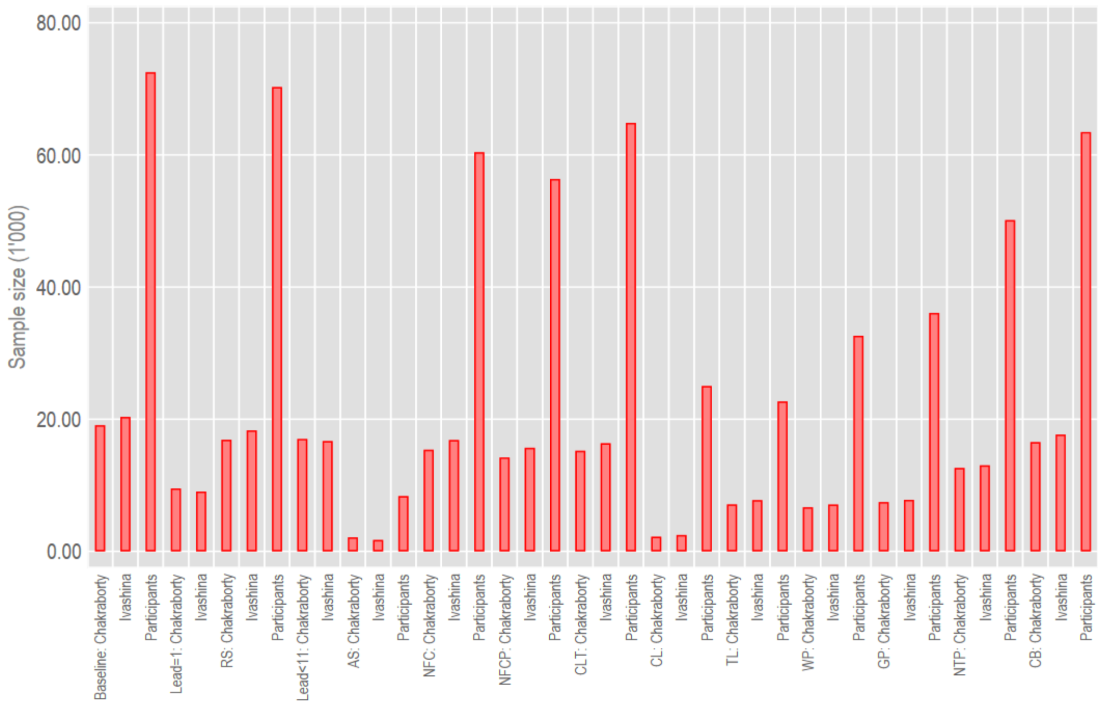
In sum, sample size is mostly affected by the choice to (i) (not) include participants or keep facilities with only one lead arranger, (ii) condition on the availability of the loan share, (iii) restrict the analysis to a certain loan type. The following analysis will reveal whether these choices result in differences regarding coefficient sign 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 different tests as outlined above. For better comparability, we plot the coefficient estimates (indicating their magnitudes and significance) across these iterations in Figure 2(a)-(b).[6]

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

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 scrutiny test, respectively.

Table 3: Results for the three baseline samples

Sample:	Chakraborty's lead	Ivashina's lead	Participants	Chakraborty's lead	Ivashina's lead	Participants
	(1)	(2)	(3)	(4)	(5)	(6)
L.Tier 1	-0.116*	-0.034	-0.001	0.100**	0.127**	0.011
	(0.059)	(0.044)	(0.011)	(0.043)	(0.056)	(0.008)
Crisis × L.Tier 1	0.342***	0.287***	0.019*			
	(0.071)	(0.056)	(0.011)			
L.Deposit	-0.005	0.000	-0.000	-0.007	-0.003	-0.000
	(0.009)	(0.008)	(0.001)	(0.011)	(0.009)	(0.002)
Crisis × L.Deposit				0.020**	0.016**	-0.000
				(0.008)	(0.007)	(0.001)
L.Size	0.356	0.706	0.076	0.214	0.755	0.073
	(0.282)	(0.427)	(0.046)	(0.289)	(0.560)	(0.045)
L.ROA	-0.004	-0.005	-0.000	-0.003	-0.004	-0.000
	(0.005)	(0.004)	(0.000)	(0.005)	(0.003)	(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. $Tier\ 1_{b,t-1}$ 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 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 (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 hence 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).

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

For example, Figure 2(a) 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.[8] In the next 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 2(a). In Figure 2(b), 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 2(a), respectively 19 out of 40 in Figure 2(b).

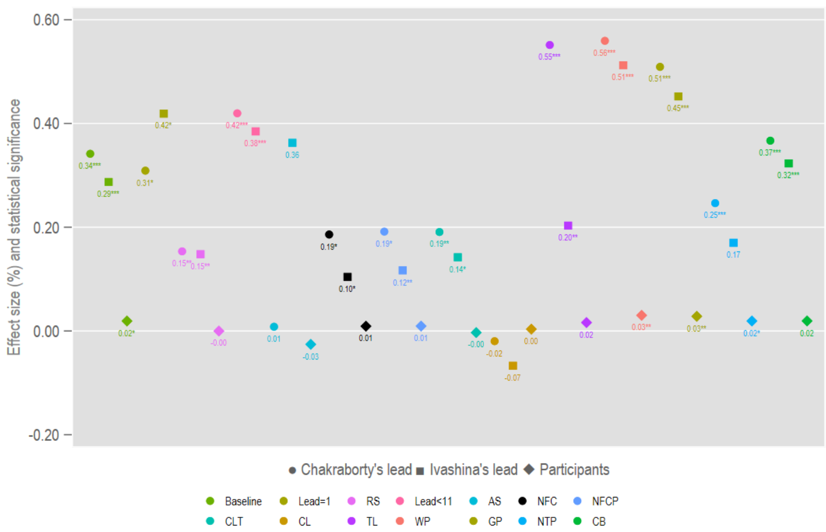
Third, the deviation in the significance of the results is not random. For example, Figure 2(a) indicates that the interaction term with the capital ratio becomes insignificant for selected alternative specifications. The first one relates to the third baseline sample containing the whole syndicate (marked by diamonds) where insignificant coefficients arise in 8 out of 12 cases.[9] This deviation is not a contradicting result as participants take a different role as lead arrangers. 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 scrutiny tests that are accompanied by a sizable decline in the number of observations (see also Figure 1). These cases refer to

[7] The 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.

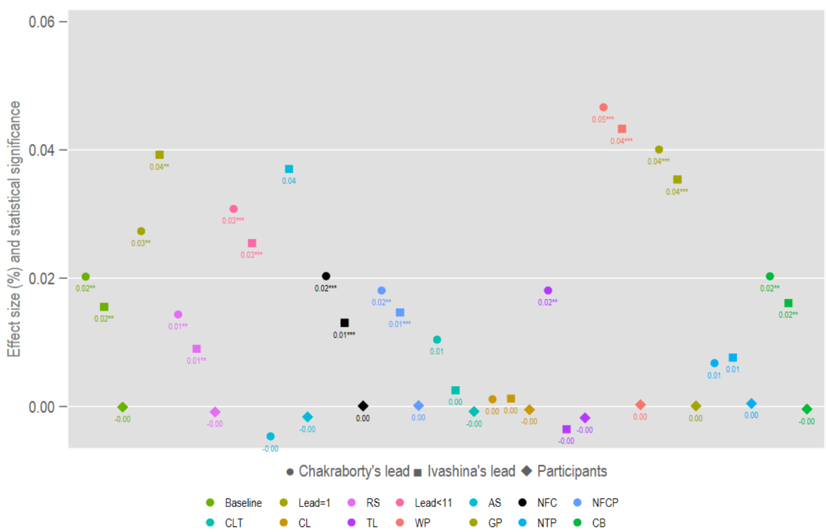
[8] Note that this specification only applies to the two samples that depend on keeping the lead arranger(s).

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

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 scrutiny 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.

the scrutiny tests of keeping only loans for which the loan share is available (AS) or keeping only credit lines (CL). The pattern shown by Figure 2(b) confirms previous conclusions.

We summarize in Table 4 the different scrutiny tests by ordering them in the decision layers that arise from the structure of the DealScan database. The last column shows which choice has most relevant effects for sample size and significance across and within the three baseline samples. A key decision to be made is to whether the study should be limited to lead arrangers or cover the full 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, the inclusion of both 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 scrutiny 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.[10] Our results merely demonstrate that these decisions have implications for sample size as well as obtained results and thus need to be well-motivated.

Further robustness In further tests, we change the clustering scheme and cluster standard errors at the bank-firm level. 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 A1). Yet the main conclusions from the previous paragraph on "scrutiny tests" remain valid.

[10]Examples of respective sample choices motivated by the research question are, amongst many others, Ferreira and Matos (2012) who focus on universal banks being lead arrangers and borrowers being non-financial 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.

Table 4: Overview of key sampling and data choices

Type of choice	**Dimensions**	**Far-reaching choice**
Syndicate composition	*All syndicate members*	
	Lead arranger(s)	X
	Lead arranger(s) & participants	X
Facility composition	*All facility members*	
	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	*All lenders*	
	Commercial banks	
	Investment banks	
	etc.	
Borrower type	*All borrowers*	
	Non-financial borrowers	
	Private borrowers	
	etc.	
Loan type	*All loans*	
	Common loan types (credit lines & term loans)	
	Credit lines	X
	Term loans	
	Working capital	
	General purpose loans	
	Loans for takeover/ acquisition	
	etc.	

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 full syndicate for both definitions in Figures A2 and A3. 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 Figure 2(a). Significance changes in the same instances as outlined above (i.e., for the sample including participants and for certain restrictions on loan types). Moreover, the interactions with the deposit ratio change slightly but the differences are not significant 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.

4 Conclusions

We use syndicated lending data from DealScan to analyze banks' lending responses depending on balance sheet variables and 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 estimations are based on a sample of banks from advanced economies that are 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 the three samples and scrutiny tests. 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 differences in significance when comparing results for lead arrangers only versus all members of a syndicate. Furthermore, the filtering of loan types reduces sample size and affects 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 and that might lead to diverging findings. Researchers should be careful regarding the choice of including only lead arrangers or also 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.

References

Acharya, V.V., Eisert, T., Eufinger, C., Hirsch, C., 2018. Real Effects of the Sovereign Debt Crisis in Europe: Evidence from Syndicated Loans. The Review of Financial Studies 31, 2855–2896.

Berg, T., Saunders, A., Schäfer, L., Steffen, S., 2021. Brexit and the contraction of syndicated lending. Journal of Financial Economics 141, 66–82.

Berg, T., Saunders, A., Steffen, S., Streitz, D., 2016. Mind the Gap: The Difference between U.S. and European Loan Rates. The Review of Financial Studies 30, 948–987.

Bord, V.M., Santos, J., 2012. The rise of the originate-to-distribute model and the role of banks in financial intermediation. Economic Policy Review (July), 21–34.

Buch, C., Bussière, M., Chinn, M., Goldberg, L., Hills, R., 2019. International spillovers of monetary policy through global banks: introduction to the special issue. Journal of International Money and Finance 91, 101–104.

Buch, C.M., Goldberg, L.S., 2017. Cross-Border Prudential Policy Spillovers: How Much? How Important? Evidence from the International Banking Research Network. International Journal of Central Banking 13, 505–558.

Cerutti, E., Hale, G., Minoiu, C., 2015. Financial crises and the composition of cross-border lending. Journal of International Money and Finance 52, 60–81.

Chakraborty, I., Goldstein, I., MacKinlay, A., 2018. Housing price booms and crowding-out effects in bank lending. The Review of Financial Studies 31, 2806–2853.

Chen, H.J., Chen, S.J., 2012. Investment-cash flow sensitivity cannot be a good measure of financial constraints: Evidence from the time series. Journal of Financial Economics 103, 393–410.

Chodorow-Reich, G., 2013. The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–9 Financial Crisis. The Quarterly Journal of Economics 129, 1–59.

Chu, Y., Zhang, D., Zhao, Y.E., 2019. Bank capital and lending: Evidence from syndicated loans. Journal of Financial and Quantitative Analysis 54, 667–694.

Cornett, M.M., McNutt, J.J., Strahan, P.E., Tehranian, H., 2011. Liquidity risk management and credit supply in the financial crisis. Journal of Financial Economics 101, 297–312.

De Haas, R., Van Horen, N., 2012. International Shock Transmission after the Lehman Brothers Collapse: Evidence from Syndicated Lending. American Economic Review 102, 231–37.

De Haas, R., Van Horen, N., 2013. Running for the exit? International bank lending during a financial crisis. The Review of Financial Studies 26, 244–285.

Delis, M.D., Greiff, K.d., Iosifidi, M., Ongena, S., 2024. Being stranded with fossil fuel reserves? Climate policy risk and the pricing of bank loans. Financial Markets, Institutions & Instruments 33.

Doerr, S., Schaz, P., 2021. Geographic diversification and bank lending during crises. Journal of Financial Economics 140, 768–788.

Ferreira, M.A., Matos, P., 2012. Universal banks and corporate control: Evidence from the global syndicated loan market. The Review of Financial Studies 25, 2703–2744.

Gatev, E., Strahan, P.E., 2009. Liquidity risk and syndicate structure. Journal of Financial Economics 93, 490–504.

Giannetti, M., Jang, Y., 2024. Who Lends Before Banking Crises? Evidence from the International Syndicated Loan Market. Management Science Published Online: https://doi.org/10.1287/mnsc.2022.03505.

Giannetti, M., Saidi, F., 2019. Shock propagation and banking structure. The Review of Financial Studies 32, 2499–2540.

Giometti, M., Pietrosanti, S., 2022. Bank specialization and the design of loan contracts. Available at SSRN: https://ssrn.com/abstract=4470135.

Hasan, I., Politsidis, P.N., Sharma, Z., 2021. Global syndicated lending during the COVID-19 pandemic. Journal of Banking & Finance 133, 106121.

Ivashina, V., 2009. Asymmetric information effects on loan spreads. Journal of Financial Economics 92, 300–319.

Ivashina, V., Scharfstein, D., 2010a. Bank lending during the financial crisis of 2008. Journal of Financial Economics 97, 319–338.

Ivashina, V., Scharfstein, D., 2010b. Loan Syndication and Credit Cycles. American Economic Review 100, 57–61.

Kacperczyk, M.T., Peydró, J.L., 2021. Carbon emissions and the bank-lending channel. CEPR Discussion Paper DP16778.

Kahle, K.M., Stulz, R.M., 2013. Access to capital, investment, and the financial crisis. Journal of Financial Economics 110, 280–299.

Kapan, T., Minoiu, C., 2018. Balance sheet strength and bank lending: Evidence from the global financial crisis. Journal of Banking & Finance 92, 35–50.

Lim, J., Minton, B.A., Weisbach, M.S., 2014. Syndicated loan spreads and the composition of the syndicate. Journal of Financial Economics 111, 45–69.

Menkveld, A.J., Dreber, A., Holzmeister, F., Huber, J., Johannesson, M., Kirchler, M., Neusüss, S., Razen, M., Weitzel, U., 2024. Nonstandard errors. The Journal of Finance 79, 2339–2390.

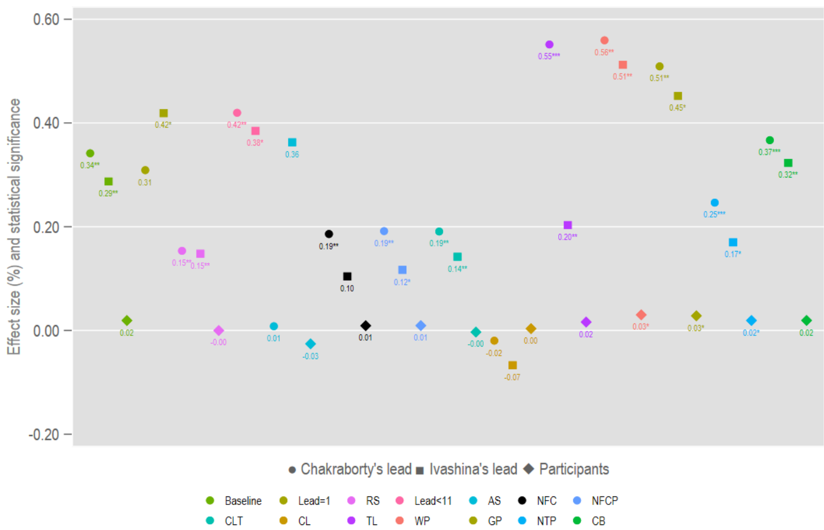
Roberts, M.R., 2015. The role of dynamic renegotiation and asymmetric information in financial contracting. Journal of Financial Economics 116, 61–81.

Schwert, M., 2018. Bank capital and lending relationships. The Journal of Finance 73, 787–830.

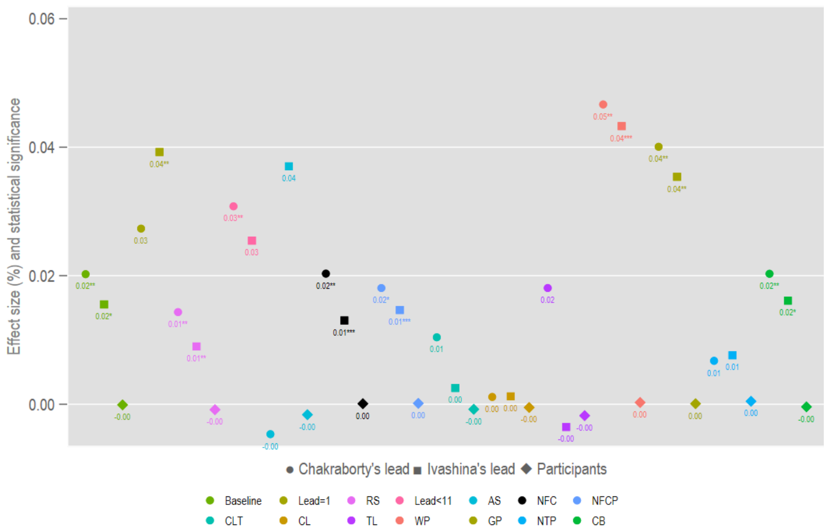
Wix, C., 2023. The Long-Run Real Effects of Banking Crises: Firm-Level Investment Dynamics and the Role of Wage Rigidity. FEDS Working Paper No. 2023-19. Board of Governors of the Federal Reserve System.

Online Appendix

Figure A1: Coefficient estimates and confidence bands across sample specifications: Alternative clustering



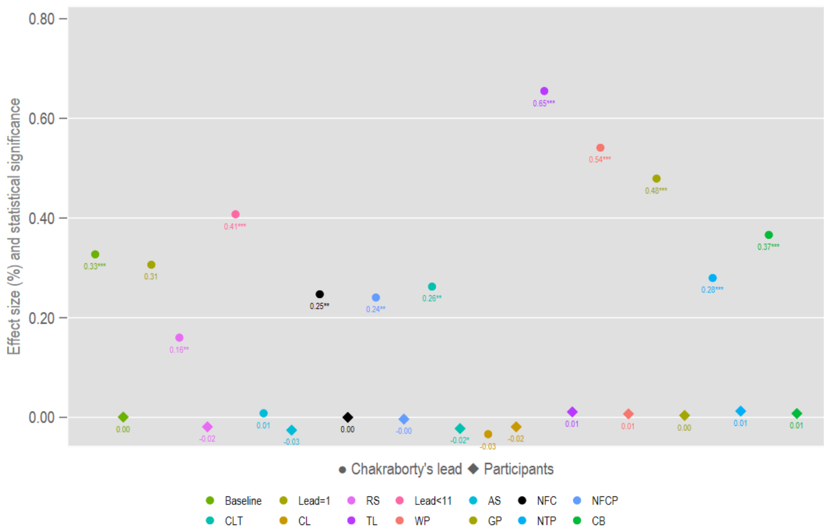
(a) Tier 1 ratio



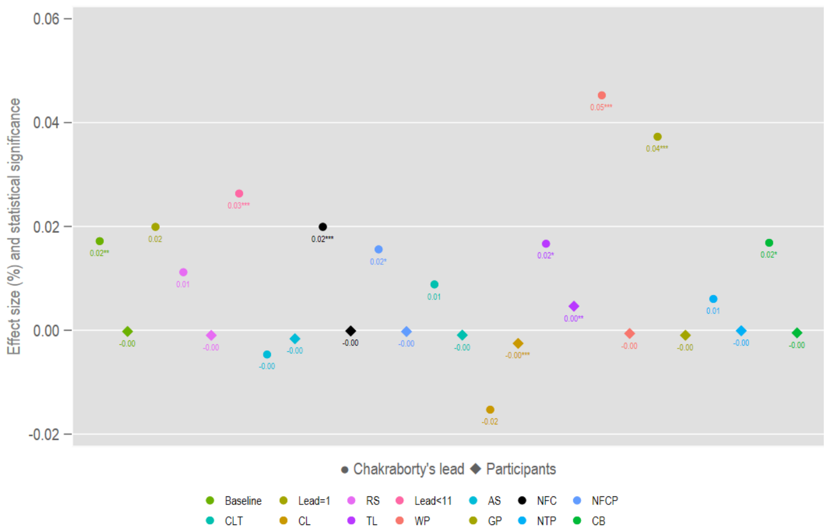
(b) Deposit ratio

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 (color), respectively. The first three coefficients (Baseline) in each sub-figure correspond to the results 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-firm level.

Figure A2: Coefficient estimates across sample specifications: Alternative allocation rule (following Chakraborty's lead)



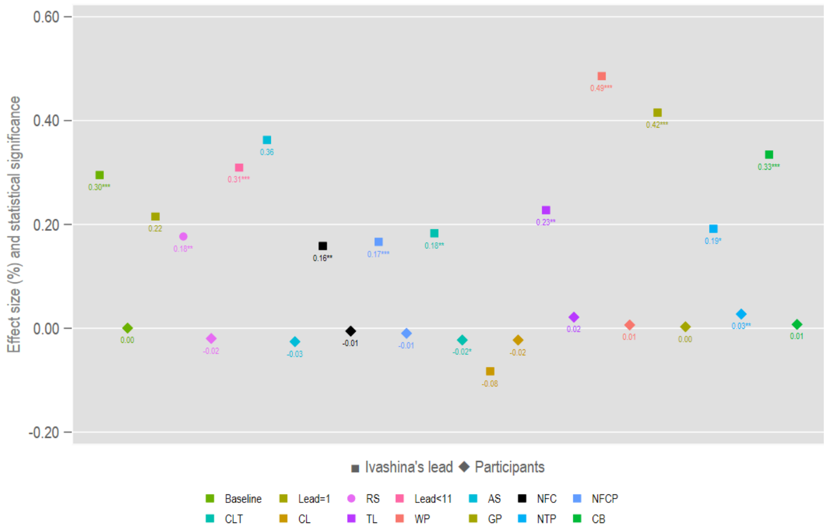
(a) Tier 1 ratio



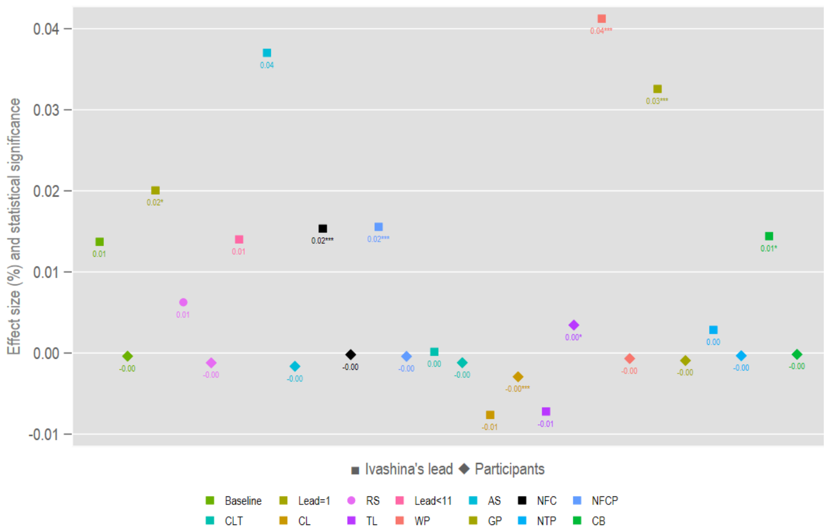
(b) Deposit ratio

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 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.

Figure A3: Coefficient estimates across sample specifications: Alternative allocation rule (following Ivashina's lead)



(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 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.

Halle Institute for Economic Research –
Member of the Leibniz Association

Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany

Postal Adress: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60
Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188