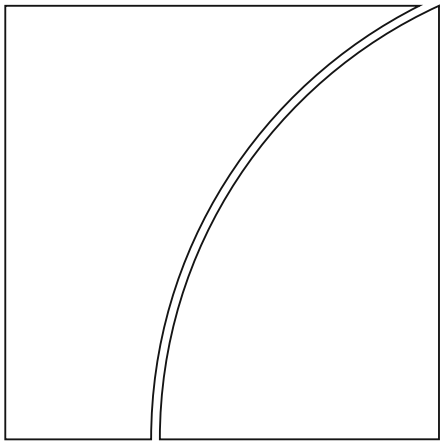

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by Iñaki Aldasoro, Sebastian Doerr and Haonan Zhou

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Non–bank Lending During Crises

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Abstract

For a large sample of countries this paper shows that non–banks curtail their syndicated lending by significantly more than banks during financial crises in borrower countries. Differences in the value of lending relationships explain most of the gap. Relationships with non–banks are less valuable in general and thereby do not improve borrowers' access to credit during crises. Non–banks are also less likely to form lasting relationships with borrowers. These findings imply that the rise of non–banks could increase the importance of transaction–based lenders and exacerbate the repercussions of financial shocks.

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Keywords: Non-banks, syndicated loans, financial crises, relationship lending, financial stability.

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1. Introduction

Non-bank financial institutions (non–banks) have steadily increased their global footprint since the Great Financial Crisis (GFC).[1] The shift in financial intermediation from banks to non–banks has raised concerns about detrimental implications for credit supply, financial stability, and the real economy. While a large literature investigates the effects of financial crises on bank lending (Claessens, 2017; Buch and Goldberg, 2020), comparable evidence is scarce for global non–bank lending and its drivers.

Using global data on syndicated loans, this paper provides novel evidence on non–bank lending and the role of lending relationships during financial crises. For a large sample of countries, we establish that non–banks contract their syndicated lending to non–financial firms by significantly more than banks during crises in borrower countries. This "lending gap" persists when we control for observable and unobservable time–varying borrower and lender characteristics. The gap halves, however, when we account for differences in the value of lending relationships. This suggests that relationships with non–banks provide fewer benefits to borrowers and that they provide less support during crises. Consistent with this argument, we find that non–banks form fewer lasting relationships than banks in general and that relationships benefit non–bank borrowers by substantially less than bank borrowers during crises, both in terms of loan amounts and loan spreads.

We use LSEG's Dealscan database on syndicated loans to classify lenders into banks and non–banks. Around one–third of global syndicated lenders are non–banks. Their origination of syndicated loans to non–financial firms grew twentyfold since 1990 and stands at about 20% of all new syndicated credit today. We combine these data with information on financial crises obtained from Laeven and Valencia (2020), which provide country–year–level information on episodes of financial distress. Our sample period from 1995 to 2018 covers a total of 83 crisis episodes in 148 countries.

We first establish that non–banks reduce their credit to non–financial firms by substantially more than banks during a financial crisis in the borrower country. Specifically, non–banks cut lending by about 19% more than banks. We observe this lending gap after controlling for unobservable time–varying lender and borrower heterogeneity. First,

[1] Non–banks account for about half of the assets of the global financial system (Financial Stability Board, 2020). On the rise of non–bank credit and its drivers, see Buchak et al. (2018), Fuster et al. (2019), Irani et al. (2021), Chernenko et al. (2022), Chen et al. (2023), Sarto and Wang (2023), Cramer et al. (2024), Davydiuk et al. (2024), Aldasoro and Doerr (2025), and Avalos et al. (2025).

we include lender parent×year fixed effects, which absorb differences in lenders' funding models.[2] Differences in funding models therefore do not fully explain the stronger decline in non–bank lending during crises in our global cross–country sample. Consistent with this argument, when we group non–banks into those with stable and unstable liabilities we find the contraction in lending to be present among both groups. Second, we account for differences in borrower characteristics through borrower×year fixed effects. These fixed effects capture, for example, firm profitability, size, or leverage, and help separate loan supply from loan demand effects.[3]

In a second step we investigate to what extent lending relationships can explain the divergence between bank and non–bank credit during crises. For banks, lending relationships have been shown to reduce information asymmetries and lead to better loan terms (Bharath et al., 2011; Ivashina and Kovner, 2011). They thus improve borrowers' access to credit during shock episodes (Sette and Gobbi, 2015; Bolton et al., 2016; Beck et al., 2018). Motivated by these findings, we construct measures of lender–borrower relationships with banks and non–banks based on the duration of the relationship as well as the frequency of previous interactions. To account for the effects of relationships in downturns versus normal times, we interact these measures with a dummy variable corresponding to the years of borrower–country crises.

Controlling for relationships and their impact during crises significantly reduces the gap between bank and non–bank lending. Depending on the relationship measure, the lending gap narrows from 19% to about 8%–10%, or by over 50%, an economically and statistically significant effect. The finding suggests both a lower prevalence and a lower quality of non–bank relationships. For one, banks on average have stronger relationships with their borrowers than non–banks, both in terms of duration and frequency. In addition, for the same relationship strength, benefits in terms of higher loan amounts are comparatively greater during crises if the relationship is with a bank rather than a non–bank. In sum, relationships with non-banks appear less valuable in general, which means that they provide less support during crises.

To buttress this finding we examine the impact of lending relationships on loan spreads

[2]Lender parent×year fixed effects bring additional advantages. They allow us to keep the full set of lenders in our sample, even if there are no balance sheet data available; they account for lender reputation; and they control for the role of internal capital markets among lenders of the same parent.

[3]Previous work has shown that borrowers connected to non–banks are significantly riskier than those that borrow only from banks, even when they are located in the same country and operate in the same industry (Aldasoro et al., 2022; Chernenko et al., 2022; Davydiuk et al., 2024).

during crises. Existing work has shown that having a stronger relationship with a lender is generally beneficial for access to credit during crises. But, as we establish, having a relationship with a non–bank provides fewer benefits compared to having one with a bank. Moreover, non–banks are less likely to form lasting relationships. An implication is hence that being connected to non–banks should lead to higher spreads during crises. In our sample of global syndicated loans, we find that, while loan spreads increase for all borrowers during crises, they do so significantly more for borrowers connected to non–banks. These findings are robust to controlling for various loan and facility characteristics, as well as when we compare firms in the same country and industry.

Finally, we find suggestive evidence that the relatively stronger contraction in non–bank lending has real effects: firms connected to non–banks see a significantly stronger decline in overall syndicated lending during financial crises than those that are not. This suggests that firms are unable to perfectly substitute the fall in syndicated lending from non–banks with funding from other sources. Consequently, their investment rates decline by relatively more and so does their employment. In interpreting these results, an important caveat is that the firm–year level analysis does not allow us to fully control for unobservable confounding factors.

Our findings are robust along various dimensions. In terms of relationships, we show that two alternative measures based on either total amounts lent or number of relationship years instead of duration or frequency yield similar results. Within credit lines, which are more likely to remain on balance sheet and are hence potentially more affected by relationships, we also find that the gap substantially narrows after accounting for relationships. Consistent with the argument that lending relationships are especially important when informational frictions are more pronounced, we find that the importance of relationships in explaining the lending gap is less pronounced in the U.S. (which has one of the best–developed and most transparent lending markets), as well as when we focus on countries ranking highly in terms of financial development. We further show that lenders' industry specialization or the geographic diversification of their loan portfolios do not explain the lending gap. These factors have been linked to higher bank credit supply during crises (De Jonghe et al., 2020; Doerr and Schaz, 2021). When we follow Irani et al. (2021) and split the sample of lenders based on whether they have stable or unstable liabilities, we find that the estimated coefficients for each group are not statistically significantly different from each other. Finally, our findings also remain after we aggregate data to the lender–borrower country–year level and use alternative de-

pendent variables, and after we consider public or private borrowers separately, exclude investment banks from the sample, focus on lenders from major countries, or restrict the sample to large individual lenders.

Taken together, our results suggest that the growth of non–bank lenders could amplify the effects of financial instabilities on the real economy. To the extent that the rise of non–banks comes at the expense of relationship–based bank lending, which has been shown to provide benefits to borrowers during crises, there could be negative consequences for borrowers' access to credit during crises. These effects could be especially pronounced for riskier firms, which are more non-bank dependent in general. At the same time, the value of relationships with banks may increase, offsetting some of these effects.

Our findings have important implications for policy and regulation. First, existing policy proposals focus mostly on non–bank financial institutions' contribution to liquidity stress in money markets and securities markets, including government bonds (Quarles, 2020; Hauser, 2021; Hubbard et al., 2021). Our results suggest that non–bank lending to non–financial firms also warrants close attention and monitoring. Second, while regulation enacted after the GFC has arguably made banks more resilient and dampened the transmission of shocks across borders through global banks, non–banks' greater presence and sharper contraction in lending during crises might offset some of these gains. Policy makers should take into account that risks may migrate across the financial system in response to tighter bank regulation, calling for a holistic perspective to financial regulation. Finally, our results for the global syndicated loan market suggest that non-bank lenders do not always act as shock absorbers or asset insulators during shock episodes (Chodorow-Reich et al., 2021; Elliott et al., 2024).

Literature and contribution. Our paper contributes to two strands of the literature. First, we speak to the literature investigating non–bank lending. This work has often focused on monetary policy shocks in single–country settings. Building on the insight that deposits flow out of banks during episodes of monetary policy tightening (Drechsler et al., 2017),[4] Chen et al. (2018) show that contractionary monetary policy leads to deposit flows from banks into non–banks in China. Accordingly, non–bank lending expands while bank lending contracts. Xiao (2020) supports this finding with a structural model: Shadow banks offset around one–third of the reduction in commercial bank deposits

[4] Aldasoro and Doerr (2023) show that a substantial share of those deposits end up in money market funds, who in turn fund banks and governments.

during monetary policy tightening cycles in the U.S. by serving a more price–sensitive clientele. For Denmark and the U.S., respectively, Cucic and Gorea (2024), Elliott et al. (2022), and Banerjee and Serena (2022) provide complementary evidence that non–banks moderate the impact of monetary policy on credit supply and the real economy. Elliott et al. (2024) do so in a cross–country setting.

Beyond monetary policy, recent evidence for the U.S. shows that non–banks cut their lending by more during episodes of market–wide uncertainty. Irani et al. (2021) show that loans funded by non–banks were less likely to be rolled over during the GFC. Fleckenstein et al. (2025) find non–bank syndicated lending to be more sensitive than bank lending to the excess bond premium. Both papers link this higher pro–cyclicality to U.S. non–banks' greater funding volatility. Our results provide external validity to these findings and highlight the important role of lending relationships in shaping global bank and non–bank lending during crises.

Second, we contribute to work on the effects of financial crises on credit supply and the importance of lending relationships. For banks, a large literature finds that nationality is an important determinant of loan supply and that global banks transmit shocks across markets (Cetorelli and Goldberg, 2012; Schnabl, 2012; Giannetti and Laeven, 2012; De Haas and Van Horen, 2013; Kalemli-Ozcan et al., 2013; Popov and Van Horen, 2015; Kalemli-Ozcan et al., 2016; Doerr et al., 2018; Hale et al., 2020; Doerr and Schaz, 2021). Claessens (2017) and Buch and Goldberg (2020) provide excellent overviews. Related work investigates the benefits of lending relationships for borrowers (Bharath et al., 2011; Ivashina and Kovner, 2011) and finds that relationships with banks alleviate borrowers' credit constraints during crises (Sette and Gobbi, 2015; Bolton et al., 2016; Beck et al., 2018). Our paper provides evidence that non–banks reduce lending by more than banks during crises and that lending relationships with non–banks provide fewer benefits to borrowers than those with banks.

2. Data and Descriptive Statistics

This section explains the data sources and construction of the main variables. It then provides summary statistics.

2.1 Data and Variable Definitions

LSEG's Dealscan database provides detailed information on syndicated loans. Syndicated loans are originated jointly by a group of financial institutions to a single borrower. The lending syndicate includes at least one lead institution (also called lead arranger) and usually further participants. Lead arrangers negotiate terms and conditions of deals, perform due diligence, and organize participants.[5] Compared to other types of loans, syndicated loans are on average larger in volume and extended to bigger borrowers.

Syndicated lending is an important source of financing for firms, in particular larger ones (Chodorow-Reich, 2014; Cerutti et al., 2015). It represents around three-quarters of total cross–border bank lending to non–financial corporations in both high– and middle–income economies (Doerr and Schaz, 2021). Non–banks have a significant presence in the syndicated loan market in all regions and sectors, both in terms of total and cross–border lending (Aldasoro et al., 2022; Elliott et al., 2024).[6] Their origination of syndicated loans to non–financial firms grew twenty–fold over the past 30 years and represents about 20% of all new syndicated credit today.

Dealscan provides detailed information on syndicated loans at origination, including loan amount, maturity, and interest, as well as the identity and type of lenders and borrowers. We follow the prior literature and restrict our sample in the following ways. We focus on syndicated lending to non-financial, non–utility firms, drop incomplete deals with status "cancelled", "suspended", or "rumour", and deals with no information on loan amounts. We manually identify and exclude lenders and borrowers linked to governments and government institutions, such as development banks. As Dealscan may report both the origination and amendments of the same deal (Roberts, 2015), we further drop deals containing the phrase "amends" or "amendment of" in their associated comments. We convert all deal values to 2012 U.S. dollars. Finally, we drop loans smaller than $10,000 (less than 1% of observations).

Information on the share that each syndicate participant contributes to a given facility is available only for a subset of the deals. To assign facility amounts to individual lenders in case of missing lending shares, or for loan facilities with aggregate lending shares

[5]Lending in the syndicated loan market is organized in packages and facilities: a package is a loan agreement between a borrower and a group of lenders, and each package can contain one or more facilities. Our basic unit of observation is the facility.

[6]See also **??** in the Online Appendix.

totaling more than 110%, we follow the prior literature and split facility volumes on a pro–rata basis among all lenders in the syndicate.

We classify lenders into banks and non–banks based on Dealscan's institution classification scheme. Accordingly, our focus is on the actual participation by bank and non–bank syndicate members (as in Lim et al. (2014) and Elliott et al. (2022, 2024)), rather than on the label assigned to the facility.[7] For example, investment banks, finance companies, and mutual funds are considered as non–bank financial institutions. We amend the Dealscan classification by matching a majority of unclassified or "other" lenders to banks and non–banks based on keyword search and manual reclassification.[8]

Non–banks differ from banks along several dimensions. For one, non–banks have no access to insured deposits and are generally subject to a lighter regulatory burden. Another important difference is that non–banks, whose funding structure is dominated by wholesale borrowing (Jiang et al., 2020), serve a more price–sensitive clientele (Xiao, 2020). Moreover, non–banks often lack access to liquidity provided by central banks (Irani et al., 2021).

To identify banking crises we rely on Laeven and Valencia's (2020) Systemic Banking Crises Database. These data provide country–year–level information on episodes of financial distress for a large number of countries up until 2018. Over our sample period from 1995 to 2018, the database reports 83 distinct banking crises. There is a concentration of financial turmoil around the late 1990s (Asian financial crisis) and from 2008 onward (Great Financial Crisis).[9] We define the dummy $crisis_{c,t}$ that equals one if borrower

[7]The focus of our analysis lies on loan origination to non–financial borrowers, which is why we classify lenders into banks and non–banks by lender type. An alternative is to classify loans by facility labels (eg classify term loans A as bank loans and term loans B as non–bank loans). However, this takes the perspective of which lender holds the loan on their balance sheet, which matters mostly for lenders' exposure to shocks through their loan portfolio. Given our focus on origination of credit to non–financial borrowers in the primary market, we assess bank and non–bank lending by examining actual participation instead of relying on labels applied to facilities.

[8]A lender is a bank in our sample if it belongs to one of the following types: African bank, Asia–Pacific bank, Eastern European / Russian bank, foreign bank, Middle Eastern bank, mortgage bank, thrift / S&L, U.S. bank and Western European bank. Consistent with our definition of non–banks, some major investment banks grouped into banks by Dealscan are reclassified as non–banks. Examples include Macquarie Bank, RBC Capital Markets, and Nomura Holdings. Lenders with SIC code 6211 classified by Dealscan as banks are reassigned to non–banks, following Lim et al. (2014). In a few cases (notably Morgan Stanley), Dealscan classifies lenders into a generic category named "corporations". We unpack this category using our manual procedure. We identify 3,026 out of 4,118 unclassified immediate lenders as banks or non–banks. For further details, see also Aldasoro et al. (2022).

[9]See **??** in the Online Appendix.

country c had a crisis in year t and zero otherwise.

To measure lending, we focus on the total amount of new syndicated credit extended by lender l to borrower b in a given year. Loan–level observations are aggregated to the lender–borrower–year level. The literature has highlighted the importance of lending along the extensive margin for syndicated credit.[10] To account for the formation and termination of lending relationships, we hence construct lending based on a panel with loan amounts of zero in the years immediately before and after lender–borrower observations with positive credit amounts (extensive margin). In robustness tests, we explore alternative measures of lending along the extensive margin. As we saturate our empirical model with a rich set of fixed effects, the sample is restricted to lenders and borrowers with at least two observations in a given year. As syndicated loans usually entail a group of lenders, the loss in sample size is negligible.

We measure the strength of lending relationships in terms of their duration and frequency. First, we capture the *duration* of a lending relationship by the number of years passed since the first syndicated loan recorded between a lender and a borrower since the late 1980s. This common measure proxies for lenders' accumulation of private information on borrowers (Petersen and Rajan, 1994; Degryse and Ongena, 2005; Sette and Gobbi, 2015). Second, the *frequency* measure counts the total number of syndicates involving a specific lender–borrower pair over the past five years prior to the origination of a new loan (Bharath et al., 2007; Ivashina et al., 2008; Bharath et al., 2011; Ivashina and Kovner, 2011, among others).[11] For robustness tests we compute two additional measures. First, we use the total amount of new loans between lender l and borrower b over the past five years, normalized by the total amount of new loans taken by the borrower over the same time span. Second, we consider a variable that counts the number of years in which there is any new lending from a given lender to a given borrower over the past five years. For

[10]For example, Giannetti and Saidi (2019) show that changes in U.S. bank syndicated lending to industries in distress is entirely driven by adjustments along the extensive margin. For the global syndicated loan market, Kempf et al. (2023) show that an increase in ideological distance between countries leads to a reduction in cross-border syndicated loan volumes, but that this is largely an extensive margin effect, as banks that experience a distance increase are more likely to stop lending to a country altogether. Elliott et al. (2024) provide comparable evidence for bank and non–bank syndicated lending in the aftermath of monetary policy shocks.

[11]The frequency of a lending relationship between lender l and borrower b in year t is measured by the total number of loans extended by lender l to borrower b from year $t-5$ to $t-1$, with the possibility of lender l participating in multiple facilities over the past five years. Bharath et al. (2007) argue that a five–year window is appropriate, as it corresponds to the typical time until the next refinancing for the firms' borrowing through syndicated loans.

all four measures, we set the value to zero if there was no previous relationship.

To examine real effects, we combine Dealscan data with information on listed firms from Compustat. To do so, we use the updated Chava and Roberts (2008) file, provided by WRDS, which ensures a match up until 2018. Overall, more than 13,000 firms in 90 countries in our regression sample are matched to Compustat.[12] We collect information on a variety of firm characteristics and compute leverage as long term debt plus current liabilities over equity.

2.2 Summary Statistics

Our sample runs from 1995 to 2018 and includes information on 9,600 lenders (of which 32% are non–banks) and 41,188 borrowers. It comprises a total of 1,222,273 lender–borrower–year observations. Non-banks extend on average 11% of all new credit in the global syndicated loan market during our sample period, and almost one–fifth of all new credit towards the end of our sample. They originate a significant share of all syndicated loans to borrowers located in all regions and sectors, with a share of foreign lending similar to banks (for further details on non–bank lenders in the syndicated loan market, see Aldasoro et al. (2022)). The bulk of non–bank syndicated lending is accounted for by investment banks and finance companies.

Table 1 provides summary statistics for our main variables. The unit of observation is at the lender–borrower–year level. Panel (a) pools banks and non–banks, while panel (b) reports summary statistics separately for banks and non–banks. In general, loans by non–banks are smaller in volume than bank loans, but facilities carry considerably higher loan spreads (255 basis points (bp) vs. 163 bp over the benchmark). Non–bank loans have on average a longer maturity than bank loans (55 vs. 44 months). Moreover, the relationship measures indicate that banks in general have longer and more frequent lending relationships compared to non–banks. Finally, non-banks' loan originations are generally more concentrated in certain industries, which is also reflected in a lower degree of loan portfolio diversification.

[12]Overall, the share of companies matched to Compustat is similar across North American countries (Canada, U.S.), Asia (Japan, Taiwan, India, Hong Kong, Malaysia), and the U.K. The coverage is not as good for some European countries (France and Spain). **??** and **??** in the Online Appendix report summary statistics and details on the quality of the match.

3. Empirical Strategy and Results

This section analyzes bank and non–bank lending during crises as well as its drivers. It first explains the empirical strategy and then presents the results.

3.1 Empirical Strategy

The baseline specification tests whether bank and non-bank lending evolve differently during financial turmoil in the country of the borrowing firm. We estimate the following specification:

$$log(credit)_{l,b,t} = \beta\ crisis_{c,t} \times non\text{-}bank_l + \phi_{l,b} + \tau_{b,t} + \psi_{L,t} + \varepsilon_{l,b,t}. \quad (1)$$

The dependent variable $log(credit)_{l,b,t}$ denotes the log of one plus new credit extended by lender l to borrower b in year t along the extensive margin. The variable $crisis_{c,t}$ takes on a value of one if the country of the borrower c experiences a financial crisis in year t. Note that Equation (1) focuses on crises in borrower countries, which mitigates the concern that a shock to the lender is the cause of the financial crises – a concern that would be more relevant if we were to analyze shocks to lenders' home markets. The dummy $non\text{–}bank_l$ takes on a value of one for non-bank lenders and a value of zero for banks.

The baseline specification includes lender×borrower fixed effects ($\phi_{l,b}$), which control for unobservable and time–invariant lender and borrower heterogeneity (such as industry, location, or distance). We thereby exploit only the variation within the same lender–borrower combination over time. We cluster standard errors at the lender parent level and borrower country level. In this way, our regressions account for serial correlation within the same borrower country across firms and time as well as among borrowers of the same lender.[13]

The coefficient β in Equation (1) captures the differential evolution of non–bank ver-

[13]Dealscan assigns lender parents using a proprietary system taking into account ownership and control. For bank lenders, the parents almost always refer to bank holding companies. Non–bank lenders could have bank or non-bank parents. A non–bank parent may also have bank and non–bank affiliates at the same time. Clustering at the lender (as opposed to lender parent) level generally reduces the estimated standard errors, but has the drawback of not accounting for potential correlation among observations across lenders belonging to the same parent.

sus bank lending during crises in borrower countries. However, any observed differential lending behavior between banks and non–banks could in principle be driven by confounding factors at both the lender and borrower level, affecting the estimate of β. We address this concern through the inclusion of granular time–varying fixed effects.

One potentially confounding factor is the difference in funding structure between banks and non–banks. Recent literature, mostly in the U.S. context, shows that non–banks rely more on wholesale funding. U.S. banks, on the other hand, are predominately funded with retail deposits (Jiang et al., 2020; Chen et al., 2018; Xiao, 2020; Elliott et al., 2022; Doerr et al., 2024). Suppliers of wholesale funding are generally more price sensitive. Together with limited access to central bank liquidity, this could make non–bank funding more fragile (Fleckenstein et al., 2025; Irani et al., 2021). One important aspect to keep in mind, however, is that in the global syndicated loan market a significant share of lending by non–U.S. banks is dollar denominated (Ivashina et al., 2015). With limited access to U.S. dollar retail deposits, these banks must rely on more volatile wholesale dollar funding markets (Aldasoro et al., 2022).

To control for differences in funding models, we include lender parent×year fixed effects ($\psi_{L,t}$). Not only do these fixed effects control for aggregate factors affecting all lenders (eg the global financial cycle), they also absorb any observable and unobservable time–varying lender heterogeneity, for example size, profitability, or the reliance on wholesale funding. There are a number of additional advantages to this approach. First, since there exists only scant balance sheet data for non–banks (especially in a cross–country setting), lender parent×year fixed effects allow us to keep the full set of lenders in our sample, while controlling for differences in funding conditions and other lender characteristics. Second, the fixed effects account for lender reputation, which has been shown to matter for syndicated loan origination (Sufi, 2007). And third, they control for the role of internal capital markets among lenders belonging to the same parent (Cetorelli and Goldberg, 2012).

Beyond lender characteristics, a common challenge to identification is that banks and non–banks could serve different clients. Previous work has shown that firms borrowing from non–banks are on average riskier, which could partly explain observed differences in lending (Aldasoro et al., 2022; Chernenko et al., 2022; Davydiuk et al., 2024). We address this challenge through the inclusion of granular time–varying fixed effects ($\tau_{b,t}$ in Equation (1)) at the borrower level. With borrower×year fixed effects, we essentially

compare lending by banks and non–banks to the same borrower in the same year (Khwaja and Mian, 2008; Jiménez et al., 2014).

3.2 Loan Amounts

Figure 1, panel (a) examines bank and non–bank lending during crises non–parametrically. It plots the evolution of the log of new credit by banks (blue solid line) and non–banks (black dashed line) around banking crises. Each series is standardized to a value of one in the year before the crisis. Loan volumes follow a similar trend for both types of lenders in the years preceding a crisis. However, they diverge sharply once the crisis hits, indicated by a value of zero on the horizontal axis. While both lender types see a contraction in credit, the decline is almost twice as large and more persistent for non–banks. In what follows we analyze this pattern in greater detail and investigate the role of lending relationships in explaining the lending gap between banks and non-banks.

Table 2, column (1) shows that non–banks contract their lending by more than banks during borrower–country crises. In particular, the coefficient estimate for β in Equation (1) suggests a relatively stronger decline in lending by non–banks of 19.5%. Figure 1, panel (b) shows that there are no differential pre-trends between bank and non–bank lending. It plots coefficients and 90% confidence intervals for Equation (1) with the *non–bank* dummy interacted with dummies for the years before and after a crisis. The omitted year is $t-1$, ie the year before a financial crisis in the borrower country. Coefficient estimates are statistically and economically insignificant prior to a crisis. They are negative during and in the years after the crisis.

We next investigate the role of lending relationships. For banks, lending relationships reduce inefficiencies from information asymmetries (Ivashina and Kovner, 2011) and can benefit borrowers through better loan terms. Relationships are especially valuable when borrower transparency is low (Bharath et al., 2011), for example during periods of heightened uncertainty or crises: Sette and Gobbi (2015) and Beck et al. (2018) show that lending relationships alleviate borrowers' credit constraints during episodes of economic shocks. Bolton et al. (2016) argue that relationship banks offer credit at more favorable terms to firms than transaction banks in a crisis. Motivated by the prior literature that emphasizes the importance of relationships with banks during crises, we include an interaction term of different relationship measures with $crisis_{c,t}$ in Equation (1).

Columns (2)–(4) show that accounting for lending relationships significantly narrows the difference in credit provision between banks and non–banks during crises. Column (2) measures relationships via the lender–borrower relationship duration. In line with previous literature, the coefficient on the interaction term with the crisis dummy is positive and highly significant: having a previous relationship with a lender is on average associated with more credit during crises. Importantly, relative to the baseline result in column (1), the coefficient on the interaction term of *crisis* and *non–bank* declines by almost 50% in magnitude, to −0.103. It remains statistically significant at the 1% level. We obtain a similar picture when we measure relationships through interaction frequency in column (3), as well as when we include both relationship measures and their interactions with the crisis dummy in column (4). The differences in the estimates of our main coefficient of interest (*crisis* × *non–bank*) between columns (2)–(4) (including either or both relationship measures) and column (1) (including neither) are always statistically significant.[14]

Finally, columns (5) and (6) report results when we include triple interaction terms of *crisis*, *non–bank*, and *relationship* in Equation (1). For expositional clarity we plot the results in Figure 2. In the figure, each line shows the difference between bank and non–bank lending during a crisis on the y–axis for different values of the relationship strength on the x–axis. The black dashed and blue solid line plot values obtained from the log of frequency and duration as a measure of relationships. Among banks and non–banks with no existing relationship with a firm, non–banks reduce lending by 6.3% and 9.7% more than banks based on frequency and duration, respectively. The negative slope of both lines indicates that this difference widens with relationship strength: relative to banks, non–banks with a similarly strong relationship with a borrower reduce lending during crises by more the longer the relationship or the more frequent past interactions.

Unpacking these results in columns (5) and (6) in Table 2 shows the following. The negative coefficients on *non–bank*×*duration/frequency* suggest that relationships with non–banks provide less value than those with banks in general. At the same time, the positive coefficients on the triple interaction effects imply that the generally negative

[14]To gauge the statistical significance, we implement a Hausman–style test and report the respective t-statistics in the table footer. We compute the test statistic as $(\beta_{\text{base}} - \beta_{\text{full}})/\sqrt{se^2_{\text{base}} - se^2_{\text{full}}}$, where model "base" refers to the model represented in column (1) without the relationship measures, and model "full" refers to models represented in columns (2)–(4) including them. In the same spirit as the Hausman test, the correct asymptotic variance is the difference between the variances of the coefficients, as the regressions are estimated on the same sample (also see Gelbach (2016)).

effect of relationships with non–banks is attenuated during crises.

Together with the finding that controlling for lending relationships reduces the 'lending gap' between banks and non–banks during crises, this suggests that there are differences in both the quality and prevalence of relationships. First, relationships with non–banks are less valuable than those with banks, which implies that they provide less support during crises. Second, banks on average have stronger relationships with their borrowers, both in terms of duration and frequency (see Table 1), which is consistent with the interpretation that non–banks are less likely to form lasting relationships with borrowers.

What could explain the diminished role of relationships with non–banks? While further research is needed, one possible explanation could be that they have less borrower–specific soft information. Banks engage with borrowers in a number of ways, such as deposit and payments services or foreign exchange hedging, all of which create information and revenue. Another explanation is that non–banks are more likely to sell their loan shares, which could lower incentives to renegotiate. The latter could be exacerbated by leverage caps and diversification rules that may constrain non-banks' flexibility to amend or roll over loans during market–wide stress.

3.3 Loan Spreads

To further investigate the value of lending relationships we analyze how they affect the spread on syndicated loans during crises. Previous work has shown that lending relationships with banks mitigate the detrimental effects of crises on loan spreads (see Bharath et al. (2011), Sette and Gobbi (2015), or Bolton et al. (2016)). In light of the facts that non–banks are less likely to form lasting relationships and that lending relationships with non–banks appear to provide fewer benefits during crises than those with banks, we expect loan spreads during crises to increase by relatively more for firms connected to non–banks.

To analyze the link between loan spreads, crises, and non–banks, we estimate facility–

level regressions of the following form:

$$\begin{aligned} spread_{f(b,t)} = {} & \gamma_1\ crisis_{c,t} + \gamma_2\ non\text{-}bank\ dependence_{b,t-1} \\ & + \gamma_3\ crisis_{c,t} \times non\text{-}bank\ dependence_{b,t-1} \\ & + controls_f + \theta_c + \theta_i + \tau_t + \varepsilon_f. \end{aligned} \tag{2}$$

The dependent variable is the loan spread of facility f involving borrower b in year t. The crisis dummy takes on a value of one during financial crises in the borrower country. Non–bank dependence measures the borrower's dependence on non–bank lenders in year $t-1$. We explain the different measures we use to proxy this dependence below. To prevent our results from being driven by differences in the characteristics of the loan facilities, we control for the following variables: loan maturity (in months), the log of the facility amount, whether the loan is secured or not, and the loan type. Moreover, we include borrower country (θ_c) and borrower industry (θ_i) fixed effects as well as year fixed effects (τ_t). We also check the robustness of our findings to the inclusion of borrower country×borrower industry×year fixed effects, ie we compare spreads on loan facilities of borrowers in the same country and industry in a given year that differ in their non-bank dependence.

The variable $non\ bank\ dependence_{b,t-1}$ is either a dummy with a value of one if a firm received a loan from at least one non–bank in the year prior to the crisis and zero if it received loans from banks only ("nb connected"); the number of non–banks from which the firm was borrowing prior to the crisis ("# nb"); or the share of syndicated credit coming from non-banks over total syndicated credit the year prior to the crisis ("nb share").

Table 3 shows that loans spreads increase during banking crises in general, and especially if borrowers have relied more on non-bank funding. Columns (1)–(4) include controls for maturity, log loan amount, a dummy for secured loan, and loan type, as well as borrower country and borrower industry fixed effects. Columns (5)–(7) add borrower country×borrower industry×year fixed effects.

Column (1) shows that loan spreads increase by 33.9 bps during crisis years for the average borrower. Adding an interaction term with *nb connected* shows that the rise in spreads is substantially stronger for borrowers connected to at least one non–banks (column 2). Similar results are obtained when we use the number of lenders or the share of lending from non-bank lenders in columns (3) and (4). Accounting for potentially

confounding trends at the country–industry level through fixed effects in columns (5)–(7) confirms these results. The coefficients on all interaction terms are positive, statistically significant, and similar in magnitude across specifications.

In terms of economic significance, results in eg column (2) suggest that spreads on facilities increase by around 28.4 bp for borrowers with no connection to non–banks, but by 34.6 bp for those connected to non-banks (ie by about 6.2 bp or 20% more). For comparison, this relative increase is 6.4 bp in column (5) with borrower country×borrower industry×year fixed effects.

These results are consistent with the interpretation that borrowing from non-banks yields fewer benefits to borrowers during crises than borrowing from banks. Existing work has shown that having a stronger relationship with a lender is generally beneficial for access to credit during crises. But, as we established above, having a relationship with a non-bank provides fewer benefits compared to having one with a bank during a crisis. In addition, non-banks are less likely to form lasting relationship. An implication is hence that being connected to non–banks should lead to higher spreads during crises, which is consistent with the results in Table 3.

4. Real Effects and Robustness

In this section, we first analyze the real effects of the contraction in non-bank lending during crises. We then perform a series of robustness tests for our main result and examine lending along the extensive margin in more detail.

Real effects. To analyze whether exposure to non-banks has real effects in terms of firm employment and investment, we aggregate the data to the borrower-year level. If borrowers can easily substitute syndicated loans from non-banks with other forms of credit (eg bonds or trade credit), the substitution could offset the credit contraction of individual non-banks. Changes in non-banks' loan supply will only have real effects if firms can at most partially substitute the fall in non-bank credit. We run variants of the

following regression:

$$\begin{aligned}\Delta y_{b,t} = \delta_1\ crisis_{c,t} + \delta_2\ non\text{-}bank\ dependence_{b,t-1} \\ + \delta_3\ crisis_{c,t} \times\ non\text{-}bank\ dependence_{b,t-1} + \phi_b + \tau_t + \mathbf{X}_{b,t-1}\boldsymbol{\beta} + u_{b,t}.\end{aligned} \tag{3}$$

The dependent variable $\Delta y_{b,t}$ is either the log difference of total syndicated loan volume of borrowing firm b across all its lenders in year t, the change in its investment rate, or the change in employment. The crisis dummy ($crisis_{c,t}$) varies at the country level and equals one during banking crisis years in firm country c. The variable $non\text{-}bank\ dependence_{b,t-1}$ is either a dummy with a value of one if a firm received a loan from at least one non-bank in the year prior to the crisis and zero if it received loans from banks only ("nb connected"), the number of non-banks from which the firm was borrowing prior to the crisis ("# nb"), or the share of total credit coming from non-banks the year prior to the crisis ("nb share").

The variable ϕ_b denotes firm fixed effects and τ_t denotes year or country×industry×year fixed effects. We additionally control for firms' log of total assets, return on assets, long-term debt over total assets, short-term debt over total assets and leverage (captured in vector $\mathbf{X}$, lagged by one year). We cluster standard errors at the firm-country level, ie the level of the shock. A coefficient of $\delta_3 < 0$ would indicate that firms with greater non-bank dependence see a stronger fall in overall syndicated loan volume, investment or employment. In interpreting the results, an important caveat to keep in mind is that firm-level regressions do not allow us to fully control for unobservable confounding factors.

Table 4 shows that firms with higher non-bank dependence see a significantly stronger decline in loan volumes, investment, and employment. Column (1) shows that total syndicated loan volume for the average firm falls during financial crises. Column (2) adds interaction effects and shows that the fall in loan volume is stronger among non-bank connected firms. We account for potentially confounding trends at the country–industry level with borrower country×borrower industry×year fixed effects. For the investment rate, column (3) also shows a significant negative effect of non-bank exposure during crises. Column (4), in turn, shows that employment declines by relatively more for non-bank connected firms. Columns (5)–(7) repeat the analysis in columns (2)-(4) but use the number of non-banks lending to the firm as a measure of non-bank dependence, while columns (8)-(10) use the share of overall lending coming from non-banks as a measure of dependence. The coefficient estimates for δ_3 are all negative and, in all cases but one,

also statistically significant.

Taken together, these results provide suggestive evidence that firms are unable to perfectly substitute the fall in syndicated lending from non-banks with other sources. Firms with higher dependence on non-banks see significant reductions in investment and employment following crises in their countries.

Robustness. In Table 5 we test the robustness of our finding that non-banks cut lending by more than banks during crises and that relationships explain a sizeable share of this lending gap. First, we consider two alternative measures of relationships. For one, we use a measure of strength based on total lending by a given lender to a given borrower in the past five years, normalized by the borrower's total new borrowing over the same period (Bharath et al., 2011; Ivashina and Kovner, 2011). Second, we consider a measure based on the count of the number of years in which there is any new lending from a given lender to a given borrower over the past five years. Columns (1) and (2) show that the coefficient of the interaction between crisis and the non-bank indicator again decreases substantially in magnitude (relative to column (1) in Table 2), similar to our findings for measures based on duration and frequency.

The value of relationships may also play out differently depending on the type of loans. Insofar as a loan type is more likely to remain on balance sheet, as is the case for credit lines (Drucker and Puri, 2009), it could be more sensitive to relationships. Should accounting for lending relationships not narrow the lending gap between banks and non-banks among credit lines, it would thus speak against our argument. Columns (3) and (4) report results for Equation (1) with the log amount of credit lines as dependent variable. Also among credit lines, controlling for relationships reduces the lending gap by about 50%.

Next, we restrict the sample to U.S. borrowers as well as borrowers in countries with efficient financial systems.[15] Relationships may play a smaller role in countries with highly developed and very transparent lending markets (Beck et al., 2010), such as the U.S. Columns (5)–(8) provide support for this argument: the effect of lending relationships in narrowing the lending gap between banks and non-banks during the great financial

[15]To do so, we use Svirydzenka's (2016) financial development (FD) index, distributed by the IMF, which measures the efficiency and transparency of local financial markets. Borrower countries with efficient financial systems are those above the 90th percentile of the FD index. We focus on domestic syndicated loans in this exercise.

crises is less pronounced in the U.S. sub-sample (columns 5–6) as well as during crises in countries with a highly developed financial system (columns 7–8).

In columns (9) and (10), we examine two other potential determinants of the lending gap: lenders' industry specialization and their portfolio diversification. Paravisini et al. (2023) and Blickle et al. (2021) show that banks often specialize in narrow markets, and De Jonghe et al. (2020) find that banks' industry specialization can protect borrowers from shocks. Doerr and Schaz (2021) further establish that lenders with a geographically diversified loan portfolio supply more credit during borrower-country crises. To measure lenders' industry specialization, we compute the share of loans originated to borrowers in industry i out of lender l's total loan originations in year t. To measure geographic diversification, we construct a lender-year level Herfindahl-Hirschman index (HHI) of lenders' loan portfolio shares across countries and define diversification as one minus the HHI.[16]

Column (9) includes lenders' industry specialization as well as diversification, interacted with the crisis dummy. The magnitude of the coefficient on the interaction term of *non-bank* and *crisis* remains similar to our baseline estimate. The modest drop in the coefficient size suggests that these factors do not explain the differences in lending behavior between banks and non-bank during crises. This picture is reinforced when we include the interaction terms in the specification together with our baseline relationship measures in column (10).

Finally, column (11) further investigates the role of lenders' funding models in our global sample. Recall that our regressions already control for differences in funding models through time-varying fixed effects at the lender parent level. In addition, we follow Irani et al. (2021) to group non-banks into those with a stable and unstable funding structure. Insurance companies, pension funds, and other institutional investors such as trust companies are grouped as non-banks with stable liabilities, while those with unstable liabilities include investment banks, hedge funds, finance companies, and other investment funds. Results for Equation (1) show that the contraction in non-bank lending is stronger among non-banks with stable liabilities, but the estimated coefficient is not statistically different compared to that for non-banks with unstable liabilities (with an F-stat of 0.59).

[16]The industry share and diversification are defined as follows: $share_{l,i,t} = loans_{l,i,t}/loans_{l,t}$ and $diversification_{l,t} = 1 - \sum_{j=1}^{J^l} s_{l,c,t}^2$, where l is lender, i is industry, c is country and t time, and $s_{l,c,t}$ measures the share of a lender l's loans to borrowers in country c relative to its total loans in year t.

We provide additional robustness checks on the lending gap between non-banks and banks during crises in the Online Appendix. In Table **??**, we show that the results are robust for both public and private borrowers. The results are also robust to the exclusion of investment banks from our non-bank group, even if, in principle, investment banks could have close ties with banks. Similar findings are obtained when we restrict the sample to lenders from the major markets (U.S., Japan, and U.K.), or major lenders, defined as those who contribute more than $10 billion in 2012 prices over our sample period.

Extensive margin analysis. To account for the importance of the extensive margin, our baseline results construct lending based on a panel with loan amounts of zero in the years immediately before and after lender–borrower observations with positive credit amounts. This aims to strike a balance between considering the extensive margin of lending, which has been shown to be critical in syndicated lending, and keeping a manageable sample size that is not mostly populated with zero lending observations.[17] This could in principle introduce some selection bias, since borrower–lender–year observations are added or removed from the sample based on future or past lending.

To address this concern, we aggregate the data to the lender–borrower country–year level and focus on two samples. As aggregation prevents us from controlling for unobservable time–varying borrower characteristics through borrower×year fixed effects, we control for borrower country×year fixed effects.

First, in columns (1)–(3) of Table 6, we conduct our estimation using a continuous sample of active lenders and all borrower countries. We define lender l as being active in borrower country c in year t if year t is between the first year in our sample that l lends to c, and the year after the last loan originated from lender l to country c. As long as l is active in country c, we include $\log(1 + credit)_{l,c,t}$ in our regression sample for the extensive margin, even if there could be consecutive periods where the lender does not lend to the country. This differs from our baseline approach which only keeps the observations immediately before and after the year in which new lending takes place. In

[17] Among the 360,909 intensive margin observations in our sample (which contrast with 1,222,273 extensive margin observations), there are 122,310 instances where loan volumes of a given lender–borrower pair change from $t-1$ to t. In other words, focusing only on changes along the intensive margin ignores the lion's share of the variation in the data. If on the other hand we were to create a balanced lender–borrower panel for all years, with zeros for all years of no new origination, we would end up with over 10 million observations.

doing so, we obtain a continuous sample at the active lender–borrower country–year level. Across specifications with different fixed effects, we obtain a negative coefficient on the interaction term.

Second, in columns (4)–(6), we use an alternative measure of syndicated loan extension that combines changes along the extensive and intensive margins. Following Davis et al. (1996), we define credit growth as

$$\Delta credit_{l,c,t} = \frac{credit_{l,c,t} - credit_{l,c,t-1}}{credit_{l,c,t} + credit_{l,c,t-1}} \times 2$$

and compute the growth rate for all lender–borrower country–year combinations. The measure accommodates extensive margin adjustment by taking on a value of 2 when a lender enters a country and a value of -2 when a lenders exits a country. In between, the measure approximates log first differences in new credit.[18] Again, results confirm our baseline finding.

Taken together, the results obtained with alternative measures of extensive margins in Table 6 suggest that the bias in our baseline extensive margin estimates is likely small. In particular, the estimated interaction coefficient between crisis and non–bank lenders is close in magnitude to our baseline estimate at the lender–borrower–year level (–0.165, –0.189 versus –0.195).

5. Conclusion

The importance of non-bank financial institutions has steadily increased in recent decades. It has become a key objective of policy makers and academics to better understand their effects on credit supply, financial stability, and the real economy (Schnabel, 2021; Aramonte et al., 2022). Existing policy proposals have mostly focused on the need to monitor non-bank financial institutions due to their contribution to liquidity stress in money markets and securities markets (Quarles, 2020; Hauser, 2021; Hubbard et al.,

[18]The so–called 'DHS' measure has several desirable properties. It is a second order approximation of the log first difference, but it is bounded and thus mitigates the effects of outliers; moreover, it accommodates zeros in t or $t - 1/t + 1$ (exit and entry). The measure has been widely used in the literature and also underlies the construction of growth rates in the official Business Dynamic Statistics (BDS) of the Census Longitudinal Business Database (LBD). For related studies focusing on credit supply and using the DHS growth rate, see, for example, Chodorow-Reich (2014), Cortés et al. (2020), or Manaresi and Pierri (2024).

2021).

Our findings suggest that non-bank lending also warrants close attention. During borrower-country crises, non-banks cut lending and increase loan rates by substantially more than banks, and non-bank connected firms see stronger declines in their employment and investment. Non-banks' rising footprint could hence exacerbate the consequences of financial shocks for the real economy. Moreover, while post Great Financial Crisis regulation has arguably made banks more resilient, non-banks' greater presence might offset some of these gains during crises. Policy makers should take into account that risks may migrate across the financial system in response to tighter bank regulation, calling for a holistic perspective to financial regulation.

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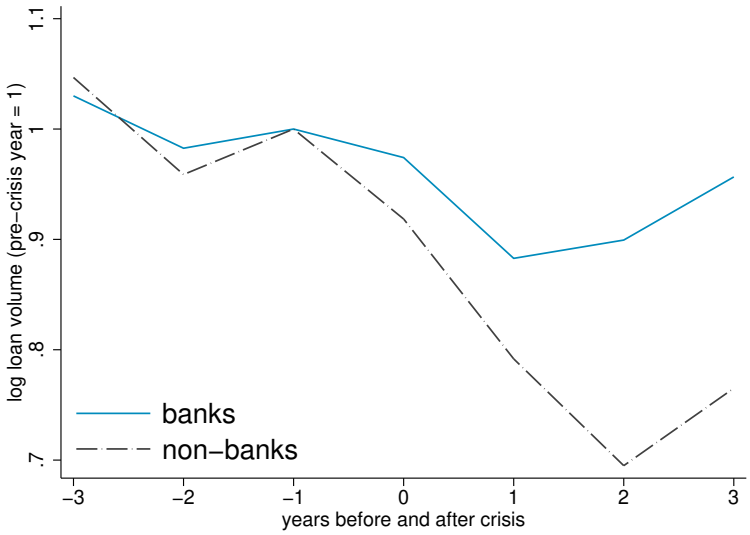
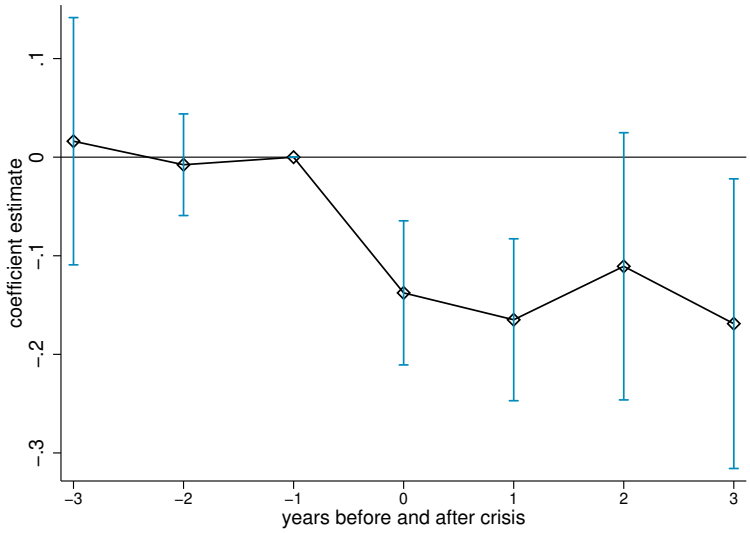
A. Figures and Tables

Figure 1: **Bank and non-bank lending during a crisis**

(a) Loan volumes

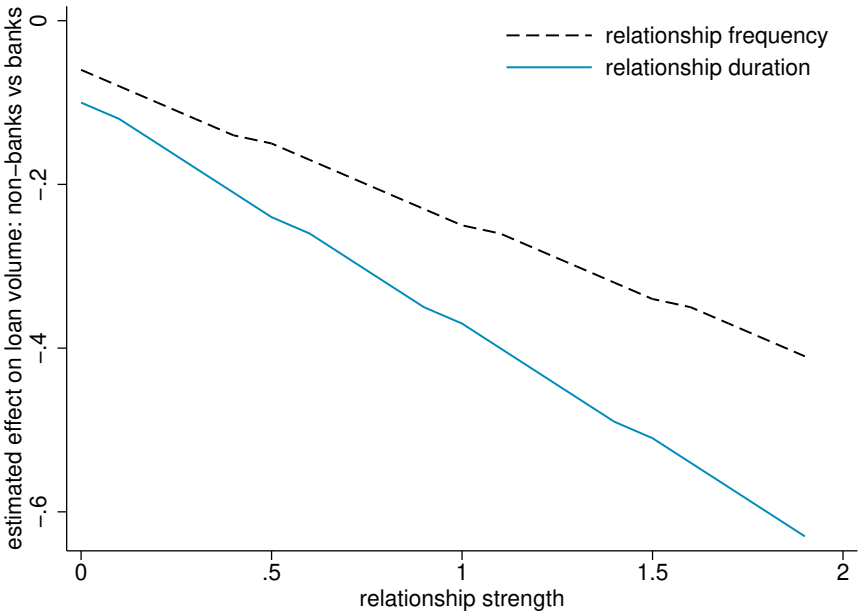


(b) Coefficient plot



Panel (a) plots the evolution of average new credit in logs in the years prior to, during (t=0), and after a financial crisis in a borrower country. Series are normalized to a value of one in the year before the onset of the crisis. The sample is split into lending by non-banks (black dashed line) and banks (blue solid line). Panel (b) presents coefficient estimates and 90% confidence bands obtained from estimating Equation (1) when we interact the non-bank dummy with dummies for the years before, during, and after a crisis. The omitted year is $t-1$, the year before a financial crisis in the borrower country.

Figure 2: **Loan volume during crises and relationship strength**



This figure plots the effect of a crisis on lending by banks vs. non-banks and how it varies with relationship strength. We estimate variants of Equation (1), but interact the variables *crisis* and *non-bank* with different relationship measures: $log(credit)_{l,b,t} = \delta_1\ relationship_{l,b,t} + \delta_2\ crisis_{c,t} \times non\ bank_l + \delta_3\ crisis_{c,t} \times relationship_{l,b,t} + \delta_4\ non\ bank_l \times relationship_{l,b,t} + \delta_5\ crisis_{c,t} \times non\ bank_l \times relationship_{l,b,t} + \phi_{l,b} + \psi_{L,t} + \tau_{b,t} + \varepsilon_{l,b,t}$. All regressions include lender×borrower, lender parent×year and borrower×year fixed effects. The coefficients entering the calculation are shown in columns (5) and (6) of Table 2. We compute the estimated effect of relationship strength on lending during crises, once for banks ($\delta_1+\delta_3$) and once for non-banks ($\delta_1+\delta_2+\delta_3+\delta_4+\delta_5$). Each line shows the difference between bank and non-bank lending (ie the value of $\delta_2+(\delta_4+\delta_5)\times relationship_{l,b,t}$) on the y-axis for different values of the relationship strength on the x-axis. We set the value of crisis to 1. The black dashed line plots values obtained from the log of relationship frequency as relationship measure, the blue solid line values from the log of the relationship duration.

Table 1: **Summary statistics**

(a) Full sample

Variable	Obs	Mean	Std. Dev.	Min	Max	P50
log(credit)	1222273	2.005	1.999	0	9.788	1.942
spread	231473	169.575	126.048	15	625	145.386
log maturity (month)	665786	3.813	.66	2.118	5.375	4.094
lending relation: duration (years)	1222273	2.081	3.193	0	29	1
lending relation: frequency (# loans)	1222273	1.591	1.993	0	46	1
industry lending share	1192719	.062	.154	0	1	.01
lender diversification	1222273	.407	.323	0	1	.421

(b) Split into banks and non-banks

	bank lender		**nonbank lender**		**mean diff.**
	mean	sd	mean	sd	t
log(credit)	2.02	(2.00)	1.92	(2.01)	16.64
spread	162.67	(120.65)	254.66	(156.39)	-94.29
log maturity (month)	3.79	(0.66)	4.00	(0.59)	-77.15
lending relation: duration (years)	2.18	(3.29)	1.18	(1.97)	105.44
lending relation: frequency (# loans)	1.63	(2.02)	1.28	(1.73)	58.31
industry lending share	0.06	(0.14)	0.11	(0.22)	-121.11
lender diversification	0.42	(0.32)	0.27	(0.31)	165.26
Observations	1097109		125164		1222273

This table reports summary statistics for the main variables in our analysis at the lender-borrower-year level. Panel (a) does so for the full sample, panel (b) separately for banks and non-bank lenders. The variable $log(credit)$ refers to the log of one plus new credit. Spread refers to all-in-drawn spread in basis points.

Table 2: **Non-banks supply less credit during crises**

VARIABLES	(1) log(credit)	(2) log(credit)	(3) log(credit)	(4) log(credit)	(5) log(credit)	(6) log(credit)
crisis × non-bank	-0.195***	-0.103***	-0.081***	-0.082***	-0.097***	-0.063***
	(0.058)	(0.032)	(0.019)	(0.021)	(0.032)	(0.018)
duration		-0.966***		0.270***	-0.933***	
		(0.051)		(0.032)	(0.046)	
crisis × duration		0.178***		0.040**	0.170***	
		(0.026)		(0.017)	(0.023)	
frequency			-1.188***	-1.317***		-1.167***
			(0.070)	(0.083)		(0.066)
crisis × frequency			0.154***	0.111*		0.150***
			(0.053)	(0.063)		(0.051)
non-bank × duration					-0.382***	
					(0.021)	
crisis × non-bank × duration					0.104***	
					(0.021)	
non-bank × frequency						-0.242***
						(0.024)
crisis × non-bank × frequency						0.060***
						(0.021)
Observations	1,220,491	1,220,491	1,220,491	1,220,491	1,220,491	1,220,491
R-squared	0.866	0.871	0.879	0.879	0.871	0.879
Lender*Borrower FE	✓	✓	✓	✓	✓	✓
Lender Parent*Year FE	✓	✓	✓	✓	✓	✓
Borrower*Year FE	✓	✓	✓	✓	✓	✓
t(diff=0)		-1.879	-2.083	-2.084	-2.001	-2.395

This table reports results at the lender-borrower-year level (see Equation (1)). The dependent variable is the log of one plus new credit extended each year to each borrower. Crisis is a dummy that takes on a value of one if the borrower country is experiencing a banking crisis. The dummy non-bank takes on a value of one if the lender is a non-bank. Columns (2)-(4) augment the baseline regression Equation (1) with measures of the strength of lending relationships (based on duration and frequency) in logs. Columns (5) and (6) add triple interaction terms between *crisis*, *non-bank*, and the relationship measures. Standard errors are clustered at the lender parent and borrower country level. The row "t(diff)=0" reports t-statistics associated with a Hausman-style test that determines whether one can reject the null hypothesis that the augmented model being estimated with the relationship measures features a significantly different "crisis×non-bank" coefficient from that in column (1). ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Table 3: **Non-banks, crises, and loan spreads**

VARIABLES	(1) spread	(2) nb connected spread	(3) # nb spread	(4) nb share spread	(5) nb connected spread	(6) # nb spread	(7) nb share spread
crisis	33.941***	28.434***	27.742***	28.572***			
	(5.416)	(5.799)	(5.858)	(5.619)			
nb dependence		19.817***	0.788***	91.892***	20.265***	0.684***	97.462***
		(4.226)	(0.132)	(4.633)	(2.835)	(0.111)	(2.257)
crisis × nb dependence		6.217***	2.783***	21.095**	6.448***	2.676***	33.700***
		(2.342)	(0.459)	(8.152)	(1.974)	(0.134)	(5.326)
Observations	50,213	50,213	50,213	50,213	44,791	44,791	44,791
R-squared	0.531	0.534	0.532	0.546	0.701	0.700	0.709
Borrower Ctry FE	✓	✓	✓	✓	-	-	-
Industry FE	✓	✓	✓	✓	-	-	-
Year FE	✓	✓	✓	✓	-	-	-
Borrower Ctry*Industry*Year FE	-	-	-	-	✓	✓	✓
Facility Controls	✓	✓	✓	✓	✓	✓	✓

This table reports results at the facility level (see Equation (2)). The dependent variable is the all-in-spread of facility f involving borrower b in year t. The *crisis* dummy takes on a value of one during financial crises in the borrower country. *Non-bank dependence* measures the borrower's dependence on non-bank lenders in year $t-1$. It is either a dummy with a value of one if a firm received a loan from at least one non-bank in the year prior to the crisis and zero if it received loans from banks only ("nb connected"); the number of non-banks from which the firm was borrowing prior to the crisis ("# nb"); or the share of syndicated credit coming from non-banks over total syndicated credit the year prior to the crisis ("nb share"). Control variables at the facility level include maturity, size, the type of loan (credit line, term loan, or other), and a secured loan indicator interacted with year. Standard errors are clustered at the borrower country-level. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Table 4: **Real effects of non-bank dependence**

VARIABLES	(1) loan volume	(2) nb connected loan volume	(3) nb connected investment	(4) nb connected employment	(5) # nb loan volume	(6) # nb investment	(7) # nb employment	(8) nb share loan volume	(9) nb share investment	(10) nb share employment
crisis	-0.1132**									
	(0.0425)									
nb dependence		-0.5502***	0.0003	0.0056	-0.0231***	0.0001**	-0.0005***	-0.0565	0.0020	-0.0254***
		(0.0348)	(0.0012)	(0.0052)	(0.0039)	(0.0000)	(0.0000)	(0.0508)	(0.0021)	(0.0037)
crisis × nb dependence		-0.1038**	-0.0145***	-0.0347**	-0.0352***	-0.0003***	-0.0004***	-0.0533	-0.0172***	-0.0366***
		(0.0421)	(0.0030)	(0.0145)	(0.0066)	(0.0001)	(0.0000)	(0.1960)	(0.0023)	(0.0122)
Observations	14,187	13,092	12,742	11,830	13,092	12,742	11,830	13,092	12,742	11,830
R-squared	0.1399	0.2508	0.3415	0.4276	0.2508	0.3406	0.4275	0.2321	0.3405	0.4276
Firm-level controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Borrower FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	-	-	-	-	-	-	-	-
Borrower Ctry*Year FE	-									
Borrower Ctry*Industry*Year FE	-	✓	✓	✓	✓	✓	✓	✓	✓	✓

This table reports results at the borrower-year level (see Equation (3)). It shows the effects of non-bank dependence during a crisis episode on firm-level outcome variables. The dependent variable in columns (1), (2), (5), and (8) is the annual change in the total volume of syndicated lending. Columns (3), (6), and (9) use the annual change in the investment rate, defined as the ratio between capital expenditure and fixed assets. Columns (4), (7), and (10) use the change in employment. The crisis dummy varies at the country level and equals one during banking crisis years in the firm country. The variable *non-bank dependence* varies across columns. In columns (2)-(4) it is a dummy with a value of one if a firm received a loan from at least one non-bank in the year prior to the crisis and zero if it received loans from banks only ("nb connected"); in columns (5)-(7) it is the number of non-banks from which the firm was borrowing prior to the crisis ("# nb"); and in columns (8)-(10) it is the share of total credit coming from non-banks the year prior to the crisis ("nb share"). Standard errors are clustered at the borrower country level. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Table 5: **Robustness tests**

VARIABLES	(1) log(credit)	(2) log(credit)	(3) credit line log(credit line)	(4) credit line + relation log(credit line)	(5) US log(credit)	(6) US log(credit)	(7) high FD log(credit)	(8) high FD log(credit)	(9) log(credit)	(10) log(credit)	(11) log(credit)
crisis × non-bank	-0.114***	-0.055***	-0.150***	-0.078***	-0.337***	-0.257***	-0.340***	-0.252***	-0.193***	-0.093***	
	(0.027)	(0.019)	(0.039)	(0.019)	(0.115)	(0.087)	(0.114)	(0.088)	(0.053)	(0.023)	
amount	-0.532***										
	(0.029)										
crisis × amount	0.043										
	(0.031)										
years		-1.665***									
		(0.098)									
crisis × years		0.245***									
		(0.087)									
duration				0.279***		1.200***		1.155***		0.291***	
				(0.047)		(0.177)		(0.149)		(0.032)	
crisis × duration				0.039**		0.245***		0.239***		0.036**	
				(0.017)		(0.060)		(0.060)		(0.017)	
frequency				-0.940***		-2.085***		-2.032***		-1.257***	
				(0.147)		(0.226)		(0.194)		(0.087)	
crisis × frequency				0.022		-0.242***		-0.232***		0.101*	
				(0.077)		(0.059)		(0.052)		(0.060)	
industry lending share									1.854***	1.613***	
									(0.172)	(0.148)	
crisis × industry lending share									0.067	0.061	
									(0.111)	(0.095)	
lender diversification									0.392***	0.349***	
									(0.111)	(0.091)	
crisis × lender diversification									-0.066	-0.097***	
									(0.047)	(0.036)	
crisis × non-bank (stable)											-0.309*
											(0.184)
crisis × non-bank (unstable)											-0.192***
											(0.057)
Observations	1,220,491	1,220,491	1,220,491	1,220,491	65,300	65,300	76,217	76,217	1,162,306	1,162,306	1,220,491
R-squared	0.878	0.880	0.877	0.884	0.889	0.902	0.894	0.906	0.869	0.880	0.866
Lender*Borrower FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Lender Parent*Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Borrower*Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
t(diff=0)	-1.558	-2.550		-2.171		-1.059		-1.205	-0.0525	-1.913	

This table reports robustness tests on the value of non-bank lending relationship during crises. The dependent variable is the log of one plus new credit extended each year to each borrower. The dummy non-bank takes on a value of one if the lender is a non-bank. Column (1) adds an alternative measure of bilateral lending relationship intensity: the amount borrowed by a given firm from a given lender normalized by the total amount borrowed by the firm over the past five years. Column (2) uses the total number of years in which at least one loan is extended over the past five years as another alternative measure of lending relationship intensity. Columns (3) and (4) focus on credit lines only. Columns (5) and (6) restrict the sample to U.S. borrowers and lenders around the Great Financial Crisis. Columns (7) and (8) focus on lenders from countries with a high degree (above 90-th percentile) of financial development defined by Svirydzenka (2016). Columns (9) and (10) investigate whether lender diversification and industry lending share narrow the lending gap between banks and non-banks. Column (11) classifies lenders into those with stable and unstable funding. The row "t(diff)=0" reports t-statistics associated with a Hausman-style test that determines whether one can reject the null hypothesis that the model being estimated with relationship measures features a significantly different "crisis×non-bank" coefficient from the corresponding model without relationship measures. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Table 6: **Lender–borrower country–level regressions**

VARIABLES	(1) active log(credit)	(2) active log(credit)	(3) active log(credit)	(4) Δ credit	(5) Δ credit	(6) Δ credit
crisis	-0.370***			-0.282***		
	(0.067)			(0.098)		
crisis × non-bank	-0.267***	-0.269***	-0.165***	-0.316***	-0.235***	-0.189***
	(0.068)	(0.066)	(0.063)	(0.080)	(0.065)	(0.068)
Observations	290,392	290,184	244,845	170,125	170,092	157,143
R-squared	0.425	0.475	0.562	0.019	0.199	0.346
Lender*Borrower Country FE	✓	✓	✓	✓	✓	✓
Lender Parent*Year FE	-	-	✓	-	-	✓
Borrower Country*Year FE	-	✓	✓	-	✓	✓

This table report results from regressions at the lender-borrower country-year level. For columns (1)–(3), the sample is a continuous sample that includes all active lenders for a borrower country. A lender is defined as active for a particular borrower country at year t, if t is between the first year that the lender lends to any firm in that country, and the year next to the last year the lender lends to any firm in that country. Columns (4)–(6) define credit growth so that it incorporates both extensive margin and intensive margin adjustments following Davis et al. (1996). Standard errors are clustered at the lender parent and borrower country level. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

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