Essays on Financial Intermediation

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Abstract

This thesis includes three essays investigating different aspects of financial intermediation.

Chapter 1 examines the impact of banks' collective liquidity mismatch policies on the stability of the financial sector. Using a novel identification strategy exploiting the presence of partially overlapping peer groups, I show that the liquidity created by individual banks is driven by the liquidity transformation activity of their peers. These correlated liquidity mismatch decisions are asymmetric and concentrated on the asset-side component of liquidity creation. Importantly, this strategic behavior increases both the default risk of individual institutions and overall systemic risk. From a macroprudential perspective, the results highlight the importance of explicitly regulating systemic liquidity risk.

Chapter 2 analyzes the credit supply and real sector effects of bank bail-ins by exploiting the unexpected failure of a major Portuguese bank and subsequent resolution. Using a matched firm-bank dataset on credit exposures and interest rates, we show that while banks more exposed to the bail-in significantly reduced credit supply at the intensive margin, affected firms compensated the tightening of overall credit with other sources of funding. Nevertheless, SMEs were subject to a binding contraction of funds available through credit lines and reduced investment and employment. These dampening effects are explained by the pre-shock internal liquidity position of smaller firms.

Finally, Chapter 3 examines the impact of a nationwide banking expansion program on access to finance as well as first-time borrowers' transition from microfinance institutions to the formal banking sector using microdata on the universe of loans to individuals from a developing country. We show that the program increased the likelihood of obtaining credit, particularly in areas with lower financial and economic development. The overall effect is driven by the newly set-up microfinance institutions (U-SACCOs), which grant loans to unbanked individuals and allow them to build credit history. Loan size increases and loan terms improve as the bank-borrower relationship matures, but these effects are weaker for U-SACCOs than for banks. Consistent with this evidence, a significant share of first-time borrowers switch to commercial banks, which cream-skim less risky borrowers from U-SACCOs after the program implementation and grant them cheaper, larger, and longer-term loans. These borrowers are not riskier and only initially receive smaller loans than similar individuals already in the formal banking sector. These results suggest that the microfinance sector, together with a credit reference bureau, plays an essential role in mitigating information frictions in credit markets.

Chapter 1

Strategic Liquidity Mismatch and Financial Sector Stability

1.1 Introduction

Banks have a unique ability to create liquidity by financing illiquid assets such as corporate loans with liquid liabilities such as demand deposits (Diamond and Dybvig, 1983). This combination of lending and deposit-taking activities protects firms and households against idiosyncratic and systematic liquidity shocks (Gatev and Strahan, 2006; Kashyap, Rajan, and Stein, 2002), and promotes economic growth (Bencivenga and Smith, 1991; Berger and Sedunov, 2017). However, the fundamental role of banks as liquidity providers has an inherent fragility problem. As exposed by the global financial crisis, excessive liquidity mismatch can lead to bank runs, breakdown of wholesale markets, and distressed asset sales

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that can threaten the solvency of individual banks and, more importantly, the financial system (Brunnermeier, 2009; Tirole, 2011). As recent theoretical literature emphasizes, the relationship between excessively high liquidity creation and financial instability can be further exacerbated when banks engage in strategic risk-taking behavior in the form of common portfolio choices (e.g., Farhi and Tirole, 2012).[1] Using a novel identification strategy exploiting the presence of peer groups that only overlap partially, this paper shows empirically that banks do take correlated portfolio decisions, and that such strategic behavior has a negative impact on both individual banks' default risk and overall systemic risk.

The incentive for banks to engage in collective risk-taking strategies can be rationalized on different grounds. Ratnovski (2009), Farhi and Tirole (2012), and Acharya, Mehran, and Thakor (2016) suggest that this behavior occurs due to the presence of bailout guarantees in case of generalized distress. This "too-many-to-fail" problem (Acharya and Yorulmazer, 2007, 2008; Brown and Dinç, 2011) leads to time-inconsistent and imperfectly targeted support to distressed banks to prevent contagion, and makes their balance-sheet choices strategic complements. Nonetheless, such correlated portfolio choices can also be driven by contractual features in the compensation of bank managers. Albuquerque, Cabral, and Guedes (2017) show that relying on relative performance evaluation (RPE) in compensation packages leads managers to disproportionately choose investments that are correlated with their peers. Similarly, Ozdenoren and Yuan (2016) predict that when agents have incentives to match industry average efforts, contractual externalities from RPE can generate excessive systemic risk-taking.[2] Ultimately, commonality in portfolio exposures and unreasonably high

[1]While in the subprime mortgage crisis the commonality of asset portfolios at banks was in the form of real estate loans, correlated portfolio choices during booms have been observed in various other forms in many crises throughout history (e.g., Reinhart and Rogoff, 2009).

[2]While public guarantees magnify this mechanism, RPE and associated correlated portfolio choices generate systemic risk even in the absence of bailout commitments by the lender of last resort (LOLR). Phelan (2017) and Morrison and Walther (2017) also show that correlated exposures may not necessarily be driven by distorted incentives due to bailout guarantees, but rather as a mechanism to provide ex-post incentives for enforcement and create market discipline. Common portfolio choices may also arise from learning motives (i.e., free-riding in information acquisition) which can lead to inefficient outcomes with fully rational agents (e.g., Banerjee, 1992). In such case, banks may rationally put more weight on the choices of others than on their own information, particularly when other banks are perceived as having greater expertise (Bikhchandani, Hirshleifer, and Welch, 1998). In a different framework, Thakor (2016) shows that periods of

liquidity transformation activity may have a tremendous adverse impact on financial stability due to higher correlation of defaults, inefficient contagious liquidations, and amplification of the impact of liquidity shocks (Acharya and Naqvi, 2012; Acharya and Thakor, 2016; Allen, Babus, and Carletti, 2012). This can sow the seeds for costly crises associated with sharp recessions and severe distributional consequences (Reinhart and Rogoff, 2009).[3]

While theoretically intuitive, identifying peer effects is empirically challenging because strategic reactions are intrinsically simultaneous (i.e., the reflection problem), and due to potential correlated effects where all banks in the same local network are subject to unobserved shocks which lead to similar policies (Manski, 1993). To counter these issues, I use an identification strategy based on Bramoullé, Djebbari, and Fortin (2009) and De Giorgi, Pellizzari, and Redaelli (2010) where a structure of connections resembling a social network can be used to solve the reflection problem and construct a valid IV to account for potential correlated effects. The key feature I exploit is that large cross-border bank holding companies tend to manage liquidity on a global scale and coordinate their risk-management policies within the group (e.g., Anginer, Cerutti, and Martinez Peria, 2017; Cetorelli and Goldberg, 2012a,b). Thus, while not part of the *direct* peer group of a domestic bank i for liquidity mismatch decision-making, the policies of a foreign bank-holding group should still influence *indirectly* those of the domestic bank i if the former has a subsidiary a that operates in the same country of bank i and that is part of i's local network. Such type of decision network structure, in line with the theoretical literature on the potential drivers of banks' collective risk-taking strategies (e.g., Albuquerque, Cabral, and Guedes, 2017; Farhi and Tirole, 2012), generates "peers-of-peers" that act as exclusion restrictions to solve the reflection problem. In addition, the policies of such indirect peers can be used as a valid instrument that is orthogonal to the domestic banks peers' liquidity policies.

sustained profitability lead all agents to assign relatively high estimates to bankers' skills. This lowers credit spreads and encourages banks to invest in increasingly risky and correlated assets.

[3] Analyzing 17 advanced economies from 1870 to 2013, Jordà, Richter, Schularick, and Taylor (2017) find that credit growth on the asset-side of banks' balance sheet and liquidity indicators such as the loan-to-deposit ratio are better predictors of systemic financial crises than solvency indicators such as capital ratios.

Using a sample of 1,612 commercial banks operating in 32 OECD countries from 1999 to 2014 and the Berger and Bouwman (2009) liquidity creation measure to capture banks' liquidity transformation activity, I first show that financial intermediaries do follow the liquidity mismatch policies of their respective peers when determining their own. The estimates indicate that the economic impact is large and consistent with a coordinated behavior where each bank adjusts to each other's decisions. Specifically, a one standard deviation increase in peer banks' average liquidity creation leads to a 5–9 percentage point increase in the liquidity created by individual banks, corresponding to a 17–29 percent increase relative to the mean.

Further, banks' liquidity mismatch choices are driven in large part by direct responses to the corresponding decisions of their respective peers and, to a much lesser extent, their other characteristics e.g., competitors' size, capitalization, profitability or credit risk. In fact, their joint effect is economically small and not robust, suggesting the results are not likely to be determined by shared characteristics between banks and their peers, and that any bias due to omitted characteristics of competitors is likely to be is small. Importantly, these findings are robust to battery of alternative tests, including different peer group definitions, the inclusion of country-year fixed effects to address any remaining omitted variables concerns, the use of the Basel III NSFR (BCBS, 2014) as an alternative funding liquidity risk measure, as well as an alternative instrument based on market data following Leary and Roberts (2014).

Given the importance of off-balance-sheet liquidity creation through loan commitments, standby letters of credit and other claims to liquid funds (e.g., Boot, Greenbaum, and Thakor, 1993; Kashyap, Rajan, and Stein, 2002), I also consider a more granular quarterly sample of 597 commercial banks operating in the US from 1999Q1 to 2014Q4.[4] The reported coefficients remain economically and statistically significant, as well as remarkably similar in magnitude across the liquidity creation measures with and without off-balance-sheet exposures. This

[4] Using data from US "Call Reports" further ensures that the results are not driven by potential problems in Bankscope in terms of different definitions of certain items across countries, preserves homogeneity in terms of regulatory framework, accounting standards and macroeconomic conditions, and allows testing whether the results on peer influence are sensitive to the use of higher frequency data.

suggests that competitors have a negligible impact in the liquidity created by banks off the balance-sheet. In fact, when decomposing aggregate liquidity creation into its individual components as in Berger, Bouwman, Kick, and Schaeck (2016a), I find that peer effects are generally concentrated in the asset-side component of liquidity creation, of which lending is a key element. This result, present in either sample, is therefore consistent with previous evidence of herding behavior in bank lending policies (e.g., Rajan, 1994; Uchida and Nakagawa, 2007).

In terms of cross-sectional heterogeneity, I show that peer effects in liquidity creation decisions are generally concentrated in less profitable and more risky banks with lower capital, lower deposit share, lower liquidity ratios, and higher non-interest revenue share. These findings are in line with collective risk-taking being driven by the incentive of improving profitability (e.g., Farhi and Tirole, 2012; Ratnovski, 2009), and indicate that higher levels of liquidity risk are not being compensated with higher capital ratios that could increase a bank's probability of survival during a crisis (Berger and Bouwman, 2013). Further, such collective risk-taking behavior is not present within banks with high capital, a result consistent with theory showing that high levels of capital strengthens banks' monitoring incentives (Mehran and Thakor, 2011) and lowers asset-substitution moral hazard (Morrison and White, 2005). In line with the procyclical nature of banks' risk management (e.g., Acharya, Shin, and Yorulmazer, 2011; Thakor, 2016), I also show that strategic liquidity mismatch choices are more prevalent in non-crisis years, though still present after the 2007-2009 global financial crisis.

Finally, I find that strategic complementarity in banks' liquidity mismatch policies affect considerably the stability of the financial system. In order to examine the direction in which these peer effects operate, I first show that the response of individual banks to the liquidity mismatch choices of competitors is asymmetric. In other words, individual banks mimic their peers strongly when they are increasing funding liquidity risk rather than decreasing it. I then show explicitly that, consistent with theoretical predictions (e.g., Acharya and Naqvi, 2012; Allen, Babus, and Carletti, 2012), correlated liquidity transformation activities increase both individual banks' default risk and overall systemic risk. This effect is economically significant: a change in the peer effect in liquidity creation from one standard deviation below the mean to one standard deviation above the mean is associated with a decrease in banks' distance-to-default of 8–12 percent, and a 7–8 and 15–31 percent increase from the mean Marginal Expected Shortfall and SRISK, respectively. These results are robust across multiple model specifications, alternative funding liquidity risk indicators, and for various financial stability measures.

The main contribution of this paper is to empirically show that financial intermediaries engage in strategic and correlated portfolio decisions and that such behavior has a negative impact on financial stability. Despite the extensive literature on this issue (e.g., Acharya, Mehran, and Thakor, 2016; Albuquerque, Cabral, and Guedes, 2017; Farhi and Tirole, 2012; Ozdenoren and Yuan, 2016; Vives, 2014), most conclusions are based on theoretical results that lack empirical support. In fact, while there is some evidence of peer effects in banks' lending policies (Uchida and Nakagawa, 2007) and liquidity risk-management decisions (Bonfim and Kim, 2018), previous studies are not able to disentangle whether this behavior is driven by banks simply facing common unobserved shocks or sharing common characteristics which lead them to choose similar policies. More importantly, to the best of my knowledge no study empirically examines the impact of banks' correlated liquidity mismatch decisions on the stability of the financial system. This issue is particularly relevant after the 2007-2009 global financial crisis, with both academics and policymakers questioning the efficacy of the recently proposed liquidity regulatory reforms (e.g., Allen, 2014; Calomiris, Heider, and Hoerova, 2015; Diamond and Kashyap, 2016; Segura and Suarez, 2017).[5]

While broadly consistent with the literature analyzing the effect of bailout guarantees on the risk of individual banks (e.g., Dam and Koetter, 2012), the results in this paper also show that moral-hazard is not necessarily restricted to banks exogenously engaging in excessive

[5]As distinctly argued by Allen (2014), "with capital regulation there is a huge literature but little agreement on the optimal level of requirements. With liquidity regulation, we do not even know what to argue about". Ultimately, the Basel III liquidity requirements may only play a limited role in reducing the likelihood of a system-wide liquidity strain as these requirements target individual banks and abstract from the additional risk of simultaneous liquidity shortfalls due to interconnections between them (IMF, 2011).

risk-taking. Instead, as theoretically conjectured by Farhi and Tirole (2012), banks also create aggregate (systemic) risk by mimicking each other's balance-sheet structures and behaving strategically. Besides, unlike Gropp, Hakenes, and Schnabel (2011) and Bonfim and Kim (2018), the identification framework I use does not restrict collective risk-taking behavior to be just driven by distorted incentives due to the presence of a LOLR. Instead, consistent with the theoretical predictions of Albuquerque, Cabral, and Guedes (2017) and Ozdenoren and Yuan (2016), the results suggest that contractual features in the bank managers' compensation schemes can also play an important role.

1.2 Identification Strategy

Empirical Model. Let a given bank i operating in country j at time t be part of a specific local network $N_{i,j,t}$ containing a total of $n_{i,j,t}$ peers. Let $y_{i,j,t}$ be the liquidity mismatch position of bank i, and $X_{i,j,t}$ and $Z_{j,t}$ a set of observed bank and country characteristics, respectively. Following Manski (1993) standard linear-in-means model, bank i's outcome $y_{i,j,t}$ can be expressed as a function of (i) the mean outcome of its peer group $\bar{y}_{-i,j,t}$, (ii) average characteristics of its peer group $\bar{X}_{-i,j,t-1}$, and (iii) bank i's and country j's characteristics:

$$y_{i,j,t} = \mu_i + \beta \bar{y}_{-i,j,t} + \lambda' \bar{X}_{-i,j,t-1} + \gamma' X_{i,j,t-1} + \delta' Z_{j,t-1} + v_t + \varepsilon_{i,j,t} \tag{1.1}$$

where,

$$\bar{y}_{-i,j,t} = \frac{\sum_{c \in N_{\mathrm{i,j,t}}} y_{c,j,t}}{n_{i,j,t}}; \bar{X}_{-i,j,t-1} = \frac{\sum_{c \in N_{\mathrm{i,j,t-1}}} X_{c,j,t-1}}{n_{i,j,t-1}}$$

The coefficient β captures the *endogenous effect* this paper aims to document i.e., the influence of peers' liquidity mismatch choices on the respective decisions of bank i. Following Leary and Roberts (2014), the equally-weighted average liquidity mismatch decisions of competitors ($\bar{y}_{-i,j,t}$) is defined as a contemporaneous measure since it limits the amount of time for banks to respond to one another, thus making it more difficult to identify mimicking behavior.[6] It also mitigates the scope for confounding effects by reducing the

[6]The results are robust to the use of a lagged measure - see Panel C of Appendix Table 1.7.

likelihood of other bank structure changes.[7] The *contextual effects* in $\bar{X}_{-i,j,t-1}$ capture the propensity of bank i to change its liquidity transformation policy in response to changes in other characteristics of the peer group e.g., leverage, profitability, credit risk. Peer, bank and country-level controls are lagged by one period to mitigate concerns of reverse causality. Bank and time fixed effects are represented by μ_i and v_t, respectively.

Identification Problem. Identifying peer effects is notoriously difficult because of two well-known issues: (i) the reflection problem, a particular case of simultaneity, and (ii) correlated or common group effects (Manski, 1993).

First, in standard linear-in-means models where peer groups are fixed, reflection arises because all agents in a local network N_{ijt} affect and are affected by all other agents. Therefore, one cannot disentangle if bank i's decision is the cause or the effect of its peers' respective choices. This simultaneity in the behavior of interacting agents due to perfectly overlapping peer groups introduces collinearity between the mean outcome of the peer group (endogenous effect) and their mean characteristics (contextual effects). This issue alone prevents separately identifying these two effects, even in the absence of unobserved correlated shocks. In contrast, under a structure resembling a social network, peer groups are individual-specific and partially overlap. This feature guarantees the existence of "peers of peers" i.e., agents who are not in the peer group of another agent, but that are included in the group of one of the peers of this agent. Such indirect peers generate within-group variation in $\bar{y}_{-i,j,t}$ and thus solve the reflection problem (Bramoullé, Djebbari, and Fortin, 2009).

While the presence of network structure with partially overlapping peer groups allows to isolate the endogenous effect of interest, it does not necessarily estimate the causal effect of peers' influence on individual banks' behavior. Specifically, the estimation results might still be biased due to the presence of group-specific unobservable factors affecting both the behavior of individual agents and their peers. This can result in agents within the same local

[7] Since bank i is excluded, $\bar{y}_{-i,j,t}$ varies not only across countries and over time, but also across banks within each country-year combination.

network behaving similarly because they face a common environment or common shocks, rather than due to actual strategic behavior. In other words, even if reflection is perfectly solved, the presence of correlated effects may still impede $\bar{y}_{-i,j,t}$ from being identified.

Identification Strategy. I use a novel identification strategy based on Bramoullé, Djebbari, and Fortin (2009) and De Giorgi, Pellizzari, and Redaelli (2010) generalized linear-in-means model where the architecture of a social network can be exploited not only to solve the reflection problem, but also to construct a valid IV for the endogenous effect and thus account for correlated effects. In detail, the presence of partially overlapping peer groups allows to use the policies of "peers of peers" as a relevant instrument. By construction, the decision of a certain bank who is not part of bank i's peer group, but included in the group of one of i's peers, is uncorrelated with bank i's peer group fixed-effect, and correlated with the mean outcome of i's group through endogenous interactions (De Giorgi, Pellizzari, and Redaelli, 2010). Such an instrument is therefore orthogonal to the bank i peers' liquidity policies, extracting the exogenous part of its variation and identifying all the relevant parameters.

Importantly, the network effects can only be identified if there are banks operating in the same country that have different direct contacts affecting their liquidity mismatch decisions. Such a rich structure of connections is likely to exist in the banking sector since, as shown by Cetorelli and Goldberg (2012a,b), large cross-border banking groups tend to manage liquidity on a global scale. As a result, it is reasonable to assume that in addition to the liquidity choices of its direct competitors, a foreign-owned subsidiary also takes into consideration the overall liquidity transformation policies of its parent bank-holding group when determining its own. In such case, the sets of peers of two given banks do not perfectly coincide if one of them is a foreign-owned subsidiary and the other a domestic bank. This notion is also consistent with Anginer, Cerutti, and Martinez Peria (2017) who find a positive and robust association between parent banks and foreign subsidiaries default risk, even when accounting for the default risk of other banks and firms in the home and host countries, as well as global factors. This relationship is partially driven by managers of subsidiaries who are rarely independent from their parents, thus suggesting that their risk-management policies tend to be coordinated.

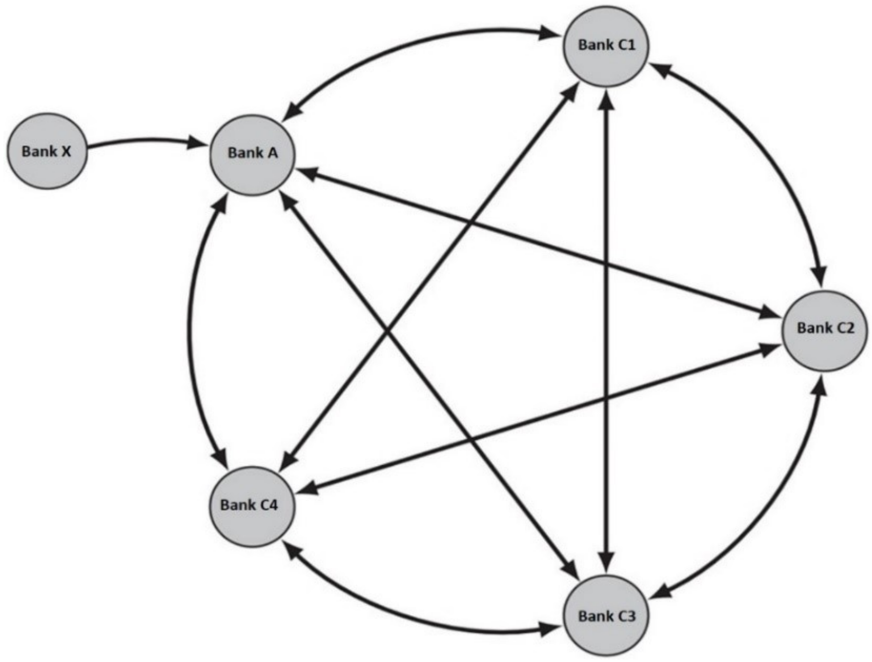
To illustrate, consider the simple network presented in Figure 1.1. Bank A, a foreign-owned subsidiary of a Bank X, competes in country j at time t with domestic Banks C1, C2, C3 and C4. They interact as follows: (i) Bank A's peer group includes Bank X, its parent bank-holding company, and Banks C1, C2, C3 and C4 which operate in the same country and have similar size and business models; (ii) Banks C1, C2, C3 and C4 peer groups include only their respective domestic competitors i.e., Bank A and the remaining C banks, but not the foreign parent X. Thus, one can use the liquidity mismatch position of Bank X (the indirect peer) as an instrument for the liquidity choice of the (direct) peers of Banks C1, C2, C3, and C4.[8] This instrument satisfies both the relevance and exclusion restrictions. First, the liquidity mismatch policy of Bank X is relevant for the respective decision of the peers of Banks C1, C2, C3 and C4 since it should influence directly the liquidity choice of Bank A. Finally, the exclusion restriction is also satisfied if the liquidity decision of Bank X is exogenous to that of Banks C1, C2, C3 and C4 own choice.

Identifying Assumption and Definition of Peer Groups. Following Figure 1.1, the key identifying assumption is that the foreign parent bank-holding group X only affects the decisions of domestic banks Cs indirectly through the average outcome of peers due to the presence of X's subsidiary. In other words, under such network structure a certain domestic bank should have little incentive to *directly* mimic the liquidity mismatch policies of a bank-holding group based in a different country. In this setting, this seems plausible.

First, within-country banks are expected to have higher incentives to mimic their domestic competitors since they share the same LOLR and are more likely to be exposed to the same set

[8] In the case of having only one foreign-owned subsidiary in a peer group, there is no instrument for the liquidity created by subsidiary A's peers, so A must be dropped from the analysis. If there are two or more distinct foreign-owned subsidiaries within the same peer group (e.g., banks A1 and A2 that are owned by foreign bank-holding groups X and Y, respectively), I keep both foreign-owned subsidiaries A1 and A2 in the estimation if the two parents are located in different countries. In such case, parent Y can identify A1, parent X can identify A2, and for the remaining banks C1-C4 the instrument is the average of parents X and Y liquidity creation. This follows Bramoullé, Djebbari, and Fortin (2009) framework that only requires that some of the indirect peers not be direct peers of the bank in question. Thus, one can use the (average) characteristics of all the indirect peers as instruments for peer bank behavior. Nevertheless, the results are similar in statistical and economic terms when dropping all foreign-owned subsidiaries from the estimations (see Panel A of Appendix Table 1.7).

Figure 1.1: Example of a simple network of banks



The figure shows a network of banks operating in country j in period t under a complete market structure (e.g., Allen and Gale, 2000) but with the presence of a bank holding company based in country p (Bank X) that affects the decisions of its foreign-owned subsidiary (Bank A). The different institutions interact as follows: (i) Bank A's peer group includes Bank X (its foreign parent bank holding company) and Banks C1, C2, C3 and C4 (its domestic competitors which have similar size and business model); (ii) Banks C1, C2, C3 and C4 respective peer groups include each other and Bank A, but not bank X e.g., Bank C1's peer group consists of Banks A, C2, C3 and C4.

of shocks and (correlated) investment opportunities (e.g., Farhi and Tirole, 2012; Ratnovski, 2009).[9] Second, peer influence for learning motives (e.g., Banerjee, 1992) is also more likely to occur within countries since banks share a similar regulatory framework and economic environment, and information for managers of small banks is more accessible. Finally, studies examining the usage of explicit RPE in incentive contracts show that firms select peers narrowly to filter out common exogenous shocks to performance e.g., based on membership in the same local market index, size, industry, and correlation of stock returns (e.g., Bizjak,

[9]It is important to note that in addition to banks generally facing a higher likelihood of being bailed-out in case of distress when compared to other industries (and thus having higher incentives to engage in collective risk-taking strategies), the regulatory framework specific to banks' liquidity mismatch policies provides a unique setting to study peer influence. In fact, overall maturity and liquidity mismatch decisions remain, to a large extent, unregulated until the Basel III NSFR rules come into force in 2018. This makes it more likely for social multiplier effects to occur as there are no boundaries or thresholds on what banks can do.

Kalpathy, Li, and Young, 2017).[10] Given that this evidence may be specific to industries other than the banking sector, Appendix Table 1.2 reports the composition of peer groups for the largest US banks in 2016 as reporting of this information in proxy statements is mandatory. The reported banks suggest that financial intermediaries indeed choose peers based in the same country and of similar size for benchmarking purposes.[11]

To incorporate further heterogeneity in peer group composition, I also introduce a size criterion when forming peer groups.[12] In detail, the peer group of a certain commercial bank i is defined as other commercial banks with similar size operating in the same country j in the same year t. To ensure that the results are not driven by a particular choice of peer group size, I report results throughout the paper based on size groups of a maximum of 10, 20 and 30 banks i.e., each bank operating in a certain country in a certain year has 9, 19 and 29 competitors, respectively.[13]

In fact, unlike small banks, large banks face both a higher idiosyncratic probability of a bailout during a crisis because they are too-big-to-fail, and a separate incentive to herd due to a "too-many-to-fail effect" (Acharya and Yorulmazer, 2007; Brown and Dinç, 2011; Farhi and Tirole, 2012). Both are driven by LOLR bailout guarantees which may lead to excessive

[10] Albuquerque (2009) also argues that relevant peers include not only firms in the same industry, but also those of similar size.

[11] Citigroup, for instance, changed in 2016 the peer group that is officially used to determine executive pay "due to the increasing challenges associated with comparing executive compensation at U.S. financial services firms to pay at firms headquartered outside the U.S. that are subject to different regulatory environments". This modification of Citi's peer group included the removal of 3 non-US banks (Barclays, Deutsche Bank, and HSBC) and inclusion of 8 US financial services firms to create a peer group of 13 US institutions. Consistent with the size criteria I use throughout the paper, the proxy statement also states that "in selecting peers, the Compensation Committee used size-based metrics as primary screening criteria among financial services firms". Source: Citigroup Inc. Notice of Annual Meeting and Proxy Statement. April 25, 2017.

[12] The Federal Financial Institutions Examination Council (FFIEC) in the US also differentiates banks according to asset size and splits them into more than 10 different peer groups.

[13] The same set of criteria to define peer groups is also proposed by Berger and Bouwman (2015) that suggest a benchmarking exercise to executives and financial analysts in which a bank would compare its liquidity creation to that of its peers to increase performance. The choice of peer group size (between 10 and 30 banks) is also consistent with Bizjak, Lemmon, and Nguyen (2011) and Kaustia and Rantala (2015). The former study finds that the average size of the peer group when setting executive compensation is around 17.3 for S&P 500 firms and 15.8 for non-S&P firms. The latter computes peer groups based on analyst-following, three-digit SIC codes and six-digit GICS codes to study peer effects in stock split decisions, and indicates that the average peer group size is of 11.7, 15.8 and 23.5 firms, respectively, when looking at NYSE-listed entities.

risk-taking in the form of excessive liquidity mismatch and correlated risk. Brown and Dinç (2011) also show empirically that the "too-many-to-fail" effect is stronger for larger banks. In addition, free-riding in information acquisition is likely to be driven by a leader-follower model where small banks' liquidity mismatch choices are affected by the decisions of large banks, but not the vice-versa. This type of behavior has been shown empirically by Leary and Roberts (2014) for non-financial listed firms in the US. Finally, the probability of RPE adoption and thus of correlated portfolio choices also increases with bank size (Albuquerque, Cabral, and Guedes, 2017; Ilic, Pisarov, and Schmidt, 2016).[14]

1.3 Sample and Descriptive Statistics

Data. To gauge the relationship between banks' strategic liquidity mismatch policies and financial stability, I combine data from several sources and compile (i) a cross-country OECD sample with annual frequency covering banks' financial and ownership information, and (ii) a more granular dataset with quarterly bank-level data for the US.

The main cross-country sample includes 1,612 commercial banks operating in 32 OECD countries from 1999 to 2014.[15] The data on banks' balance-sheet and income statements is obtained from the BvD/Fitch Bankscope. To have information at the most disaggregated level and avoid double-counting within the same institution, I discard consolidated entries if banks report unconsolidated data.[16] Thus, as in Gropp, Hakenes, and Schnabel (2011),

[14] More generally, within-country banks with different size differ significantly in terms of loan portfolio and funding composition. While larger banks tend to use riskier wholesale funding and are more likely to engage in informationally transparent lending, smaller banks rely more on stable deposits and engage in informationally opaque lending to small bank-dependent firms (Berger, Bouwman, and Kim, 2017; Song and Thakor, 2007). Berger and Bouwman (2009) also find that liquidity creation differs significantly across large, medium and small US banks.

[15] Out of 34 OECD members, Iceland and Israel are not included in the sample due to the limited number of foreign-owned banks, if any, that would not allow to identify the peer effects of interest.

[16] I go to great lengths to (i) identify duplicate observations in each country/year and thus avoid capturing spurious peer effects; and (ii) check whether the bank specialization reported in Bankscope is accurate i.e., if a commercial bank is indeed engaged in financial intermediation activities. First, besides discarding consolidated entries if banks report information at the unconsolidated level, I also look for banks having the same address, nickname, website or phone and drop the respective duplicates e.g., banks reporting information with different financial standards in the same year. Second, I cross-check the specialization codes in Bankscope with those reported in Claessens and Van Horen (2015) and adjust them accordingly.

domestic and foreign subsidiaries are included as separate entities. While most bank-specific variables are expressed in ratios, all variables in levels (e.g., total assets) are also adjusted for inflation and converted into millions of US dollars.[17] Stock prices and number of shares outstanding are collected from Thomson Reuters Datastream and matched with Bankscope using the International Securities Identification Number (ISIN) for listed banks.

Ownership information for all commercial banks in the OECD sample is manually collected from the BvD ownership database, banks and national central banks' websites, and newspaper articles obtained from Factiva. The data is further cross-checked with the Claessens and Van Horen (2015) bank ownership database. Compared to the latter, however, the database I compile is unique in several aspects. First, while the Claessens and Van Horen (2015) database indicates whether a certain bank is foreign-owned and the respective home country of the parent bank, I obtain information on who the actual owner of this foreign-owned bank is, and its respective Bankscope identifier.[18] Further, while Claessens and Van Horen (2015) report the country of ownership based on direct ownership, I obtain information and consider throughout the paper the ultimate bank owner based on a 50% threshold. While limited to OECD countries, the data used in this paper is therefore considerably more detailed and provides a novel source of information.

With respect to the country-level variables, I collect information on GDP per capita, GDP growth, imports and exports of goods and services, and the Consumer Price Index (CPI) from the World Bank's WDI database and the Federal Reserve Bank (FRB) of St. Louis

Finally, to further ensure that the sample only includes commercial banks - typically defined as institutions that make commercial loans and issue transaction deposits - I exclude banks with customer deposits not exceeding 5% of liabilities and with loans not exceeding 5% of total assets.

[17] The sample is also restricted to the largest 100 banks in each country, thus excluding smaller (mostly regional) banks in the US and Japan and limiting the over-representation of these two countries. In practice, a bank is excluded if and only if it is not in the Top 100 in terms of assets in the country it operates in *all* the years it is active. I also exclude branches of foreign banks since they generally do not report individual information and are not covered by the LOLR of the country where they operate.

[18] Consider the US as an example. While the Claessens and Van Horen (2015) bank ownership database only indicates the home country of the majority shareholder of HSBC Bank USA (i.e., UK), the database I construct specifies who the owner is (HSBC Holdings Plc) and its Bankscope identifier. With this information and using a parallel Bankscope dataset with information at the consolidated level, one can compute the liquidity created by the foreign parent bank holding company and construct the main instrument.

Economic Data. The date of inception of explicit deposit insurance schemes is obtained from Demirgüç-Kunt, Kane, and Laeven (2015), while the country-level measure of macroprudential regulation intensity (i.e., cumulative sum of changes over time in the usage intensity of capital buffers, interbank exposure limits, concentration limits, LTV ratio limits and reserve requirements) is from Cerutti, Correa, Fiorentino, and Segalla (2017). Banking sector equity market indices are provided by FTSE Russell.

Finally, the quarterly bank-level US sample is obtained from the FFIEC/FRB of Chicago "Call Reports" and includes 597 commercial banks from 1999Q1 to 2014Q4. These reports containing balance sheet, off-balance sheet and income statement information are combined with on and off balance-sheet liquidity creation data available from Christa Bowman's website and constructed following the Berger and Bouwman (2009) methodology. I also obtain stock price data from CRSP and use the CRSP-FRB Link provided by the FRB of New York to match each regulatory bank identifier (RSSD) with a unique PERMCO. The sample includes not only individually traded banks but also those that are part of a traded bank holding company. Nonetheless, to ensure that the liquidity is being created by the sample banks, I follow Berger and Bouwman (2009) and exclude banks that are not individually traded which account for less than 90% of the holding assets.[19]

Liquidity mismatch measures. Given that banks hold liquidity on their asset side and provide liquidity through their liabilities, liquidity management is ultimately a joint decision over both assets and liabilities (Cornett, McNutt, Strahan, and Tehranian, 2011; Donaldson, Piacentino, and Thakor, 2018; Gatev, Schuermann, and Strahan, 2009). In this regard, I build on the work of Berger and Bouwman (2009) and use their measure of liquidity creation to capture banks' liquidity mismatch policies.[20] By considering the different asset, liability

[19]The CRSP identifiers are further matched with PERMNOs and merged with CoVaR data available up to 2013Q2 (Adrian and Brunnermeier, 2016). I thank Allen Berger and Christa Bouwman for sharing the liquidity creation data, and Tobias Adrian and Markus Brunnermeier for providing the CoVaR data.

[20]In robustness tests I also consider the distinct, though complementary, Basel III Net Stable Funding Ratio (NSFR). This regulatory requirement is expected to enter into effect in January 2018 and aims to encourage banks to hold more stable and longer term funding against their less liquid assets, thus reducing liquidity transformation risk. It is defined as the ratio of the available amount of stable funding (ASF) to the required

and equity components of a bank's balance-sheet, this structural indicator provides a broad picture of the overall funding mismatch of each financial institution.

In detail, the liquidity creation measure is defined as the weighted sum of all bank balance-sheet items as a share of total assets. Liquidity weights are assigned based on the ease, cost and time it takes for banks to dispose of their obligations to meet a sudden demand for liquidity, and for customers to use liquid funds from banks. Since banks create liquidity by transforming illiquid assets (e.g., corporate loans) into liquid liabilities (e.g., demand deposits), both iliquid assets and liquid liabilities are given a positive liquidity weight of $1/2$. Similarly, since banks destroy liquidity when they transform liquid assets (e.g., cash and government securities) into iliquid liabilities (e.g., long-term funding) or equity, liquid assets, iliquid liabilities and equity are given a negative liquidity weight of $-1/2$. An intermediate weight of 0 is applied to assets and liabilities that are neither liquid nor iliquid. Since the granularity of the data is different in Bankscope and the US Call Reports used in Berger and Bouwman (2009), I adapt the classifications and weights following those of the authors - see Appendix Table 1.1.[21]

Financial stability indicators. Following the literature standard (e.g., Beck, De Jonghe, and Schepens, 2013; Boyson, Fahlenbrach, and Stulz, 2016; Dam and Koetter, 2012), I use the Z-score (distance-to-default) to capture individual bank's default risk. This measure can be interpreted as the number of standard deviations by which returns would have to fall from the mean to eliminate all the equity of a bank i.e., a lower Z-score implies a higher probability of default. In detail, the Z-score of bank i at time t is defined as the sum of return-on-assets (ROA) and the equity to assets ratio, all divided by the standard deviation of the ROA. I

amount of stable funding (RSF) over a one-year horizon. Banks will have to meet a regulatory minimum of 100%. I use the inverse of the NSFR throughout (i.e., NSFRi = RSF/ASF) so that this indicator is directly comparable to the Berger and Bouwman (2009) liquidity creation measure. While liquidity creation is an indicator of current iliquidity, the NSFR captures what iliquidity would be under a stress scenario (Berger and Bouwman, 2015).

[21] The weights to compute the NSFR are also presented in Appendix Table 1.1. These are given according to the final calibrations provided by the Basel Committee (BCBS, 2014) but also adapted to the granularity of Bankscope data. Where applicable, items are treated relatively conservatively e.g., all loans are assumed to have a maturity of more than 1 year and hence a RSF weight of 85 percent.

use a three and five-year rolling window to compute the standard deviation of ROA. This approach avoids the variation in Z-scores within banks over time to be exclusively driven by variation in levels of profitability and capital. Furthermore, by not relying on the full sample period, the denominator is no longer computed over different window lengths for different banks. Given that the Z-score is highly skewed, I use its natural logarithm to allow for a more uniform distribution.

From a regulatory perspective of ensuring the financial sector stability, the contribution of each bank to the risk of the financial system as a whole is increasingly more relevant than the absolute level of risk of any individual institution. As a result, I also consider two different measures to capture systemic risk. The first, Marginal Expected Shortfall - MES (Acharya, Pedersen, Philippon, and Richardson, 2017) is defined as bank i's expected equity loss (in %) in year t conditional on the market experiencing one of its 5% lowest returns in that given year. MES is computed using the opposite of returns such that the higher a bank's MES is, the higher its systemic risk contribution. The market is defined as the country-specific banking sector equity market. The second measure, Systemic Capital Shortfall - SRISK (Brownlees and Engle, 2017), corresponds to the expected bank i's capital shortage (in billion USD) during a period of system distress and severe market decline. Following Acharya, Engle, and Richardson (2012), the long-run MES is approximated as 1-exp(-18*MES) where MES is the one day loss expected if market returns are less than -2%. Unlike MES, SRISK is a also function of the bank's book value of debt, its market value of equity and a minimum capital ratio that bank firm needs to hold. To ensure comparability across countries, I follow Engle, Jondeau, and Rockinger (2015) and set the prudential capital ratio to 4% for banks reporting under IFRS and to 8% for all other accounting standards, including US GAAP.

Table 1.1 reports descriptive statistics for the main variables in the cross-country sample. The average bank is creating liquidity (0.32), both on the asset (0.17) and liability (0.15) side of the balance-sheet. If in place, it would comply with the regulatory NSFR (101%). It has a distance-to-default (ln[Z-score]) of 3.36 to 3.70, marginal expected shortfall (MES) of 2.42%, and expected capital shortage of 3.17 billion USD in case of system distress (SRISK). 16.4% of the observations in the sample correspond to listed banks. Bank-level characteristics include size (ln[total assets]), capital ratio (equity/assets), ROA (net income/assets), deposit share (customer deposits/assets) and NPL provisions (loan loss provisions/assets), all winsorized at the 1st and 99th percentile levels. Country-level indicators include the log of GDP per capita, GDP growth volatility (standard deviation of GDP growth rate over the past 5 years), local market concentration (Herfindahl index) and the Cerutti, Correa, Fiorentino, and Segalla (2017) prudential regulation intensity measure. The bank and country-level controls are comparable in terms of magnitude to those in previous studies consistently showing their important for banks' financial decisions (e.g., Beck, De Jonghe, and Schepens, 2013; Beltratti and Stulz, 2012; Ellul and Yerramilli, 2013). For completenesses, Appendix Table 1.3 presents summary statistics for all the peer banks' characteristics considered (e.g., peers' average liquidity creation, size or capitalization), as well as additional bank and country characteristics that are used to minimize omitted variables concerns.[22] Finally, Appendix Table 1.4 reports the summary statistics for the quarterly US sample of listed banks. While the average US bank is larger when compared to the OECD sample, the liquidity mismatch indicators and remaining bank-level characteristics are relatively similar across the two samples.

1.4 Results

1.4.1 Peer effects in Banks' Liquidity Mismatch Decisions

Table 1.2 reports the benchmark set of results gauging whether the liquidity mismatch decisions of a specific bank are determined by the respective choices of its competitors. The table presents 2SLS coefficient estimates of model (1.1) using the Berger and Bouwman (2009) liquidity creation measure as dependent variable and, exploiting the presence of partially overlapping peer groups, the liquidity policy of "peers of peers" as a relevant instrument

[22]These include the liquidity ratio (liquid assets/total assets), non-interest revenue share (non-interest income/total income), cost-to-income ratio, global integration (imports plus exports of goods and service to GDP), deposit insurance (a dummy variable that equals 1 if an explicit deposit insurance scheme is in place in country j in year t, and 0 otherwise), and a dummy variable that equals 1 if IFRS is in place in country j in year t to account for potential reporting jumps at the time of a bank's accounting standards change.

Table 1.1: Summary statistics

Variables	N	Mean	SD	P25	P50	P75
Liquidity mismatch indicators:						
Liquidity Creation	14,438	0.316	0.235	0.173	0.342	0.471
LC Asset-side	14,438	0.170	0.219	0.032	0.226	0.344
LC Liability-side	14,438	0.146	0.147	0.037	0.144	0.255
NSFRi	14,438	0.995	0.522	0.738	0.890	1.078
Bank-level characteristics:						
Size	14,438	8.279	2.119	6.684	8.103	9.706
Capital Ratio	14,438	0.100	0.079	0.056	0.080	0.116
ROA	14,438	0.006	0.013	0.002	0.006	0.011
Deposit Share	14,438	0.586	0.222	0.444	0.619	0.761
NPL Provisions	14,438	0.005	0.008	0.000	0.002	0.005
Country-specific characteristics:						
GDP per Capita	14,438	10.42	0.554	10.37	10.53	10.71
GDP Growth Volatility	14,438	0.019	0.012	0.010	0.016	0.025
Concentration	14,438	0.187	0.133	0.094	0.151	0.251
Prudential Regulation Intensity	14,438	0.553	2.247	-1.000	0.000	1.000
Financial stability indicators:						
Ln(Z-score)$_{3y}$	12,390	3.700	1.333	2.896	3.668	4.482
Ln(Z-score)$_{5y}$	9,411	3.361	1.143	2.688	3.389	4.045
Marginal Expected Shortfall (%)	2,374	2.423	2.212	0.781	1.952	3.422
S-RISK (bil USD)	2,374	3.172	13.845	0.000	0.040	1.013

This table presents summary statistics for the main variables in the cross-country sample comprised of 1,612 commercial banks operating in 32 OECD countries from 1999 to 2014. Liquidity Creation (LC) is the Berger and Bouwman (2009) "cat nonfat" measure i.e., on-balance-sheet liquidity creation divided by total assets. NSFRi is the inverse of the Net Stable Funding Ratio. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing both measures. Bank-level characteristics include size (ln[total assets]), capital ratio (equity/assets), ROA (net income/assets), deposit share (customer deposits/assets), and NPL provisions (loan loss provisions/assets). Country-level characteristics include the log of GDP per capita, GDP growth volatility (standard deviation of GDP growth rate over the past 5 years), local market concentration (Herfindahl index) and prudential regulation intensity (cumulative sum of changes over time in the usage intensity of capital buffers, interbank exposure limits, concentration limits, LTV ratio limits and reserve requirements). Z-score is defined as the sum of capital over total assets and return-on-assets (ROA), divided by the 3 or 5-year rolling standard deviation of ROA. Marginal Expected Shortfall (MES) corresponds to bank i's expected equity loss (in %) in a given year conditional on the market experiencing one of the 5% lowest returns in that year. Systemic Capital Shortfall (S-RISK) is the bank-specific expected capital shortage (in billion USD) during a period of system distress and severe market decline.

(Bramoullé, Djebbari, and Fortin, 2009; De Giorgi, Pellizzari, and Redaelli, 2010). The row at the top of the table reports the peer effect of interest i.e., the estimated coefficient on the instrumented peer banks' average liquidity creation. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10 (columns 1-2), 20 (columns 3-4) and 30 banks (columns 5-6) according to their size. The regressions in columns 1, 3, 5 control for the standard set of bank, peer average and country characteristics used throughout the paper, while those in columns 2, 4 and 6 include additional covariates to minimize omitted variable concerns. All specifications include year and bank fixed-effects, and the t-statistics in parentheses are robust to heteroskedasticity and within peer group dependence.[23]

Consistent with the theoretical predictions of Farhi and Tirole (2012) and Albuquerque, Cabral, and Guedes (2017), among others, the results across all specifications in Table 1.2 show that the liquidity created by individual banks is significantly and positively affected by the liquidity transformation activity of its respective competitors. To ease the interpretation of magnitudes and ensure comparability across different samples, all coefficients are scaled by the corresponding variable's standard deviation. Thus, a one standard deviation increase in peers' average liquidity creation leads to a 5.2–9.1 percentage point increase in bank i's liquidity creation, corresponding to a 17–29 percent increase relative to the mean.[24]

While bank-specific liquidity mismatch decisions are mostly driven by direct responses to the respective policies of its competitors, some other peer characteristics such as their average capital and non-interest revenue share also matter for its determination. Nevertheless, their joint effect on individual banks' liquidity decisions is economically small and not robust. This suggests that (i) the results are not likely to be driven by shared characteristics between banks and their respective peers, and that (ii) any bias due to omitted characteristics of competitors that are relevant for bank i's liquidity choices is likely to be small.

Identifying assumptions. The relevance condition requires the IV to be significantly correlated with peer banks' average liquidity creation (the endogenous variable), and the results in Table 1.2 show that this is indeed the case. The instrument is always significant at

[23]Following the example in Figure 1.1, the peer group that includes Banks C1-C4 constitutes the relevant cluster to build inference since there is no variation in the instrument across them. In other words, given that the liquidity created by Bank X (the foreign parent bank-holding group that owns the foreign subsidiary A) should be positively correlated with that of C1, C2, C3 and C4 through the effect on A's liquidity creation, and since banks C1-C4 become identified using the characteristics of the same Bank X as an instrument, standard errors should be clustered at the peer group level. Nevertheless, the results are also robust to using the bank as the unit for clustering - see Panel B of Appendix Table 1.7.

[24]The unscaled coefficient estimates can be retrieved by dividing each coefficient with the corresponding variable's standard deviation presented in Appendix Table 1.3. The results in Appendix Table 1.5 show that this effect is still significant, though underestimated, when using OLS regressions.

Table 1.2: Peer effects in banks' liquidity mismatch decisions

Dep Var: Liquidity Creation	(1)	(2)	(3)	(4)	(5)	(6)
Peers' Liquidity Creation	0.058***	0.052***	0.064***	0.056***	0.091***	0.085***
	(3.337)	(2.913)	(4.053)	(3.271)	(4.443)	(3.540)
Peers' Size	0.008	0.010	0.009	0.011	0.009	0.009
	(0.994)	(1.353)	(1.206)	(1.376)	(0.820)	(0.921)
Peers' Capital Ratio	0.005	0.004	0.012*	0.010	0.019***	0.017***
	(1.018)	(0.925)	(1.789)	(1.591)	(2.724)	(2.773)
Peers' ROA	0.001	0.000	-0.001	-0.001	0.004	0.005
	(0.410)	(0.037)	(-0.297)	(-0.390)	(0.977)	(1.520)
Peers' Deposit Share	0.001	0.003	-0.003	-0.001	0.003	0.003
	(0.415)	(0.810)	(-0.516)	(-0.173)	(0.588)	(0.636)
Peers' NPL Provisions	0.000	0.000	-0.001	-0.001	0.002	0.003
	(0.037)	(-0.007)	(-0.467)	(-0.302)	(0.741)	(1.187)
Peers' Liquidity Ratio		0.006		0.006		0.012**
		(1.362)		(1.123)		(2.017)
Peers' Cost-to-Income		-0.001		-0.001		0.003
		(-0.453)		(-0.310)		(0.543)
Peers' Non-Interest Revenue		0.008***		0.008**		0.010***
		(2.764)		(2.302)		(2.841)
Peer Group Size	10	10	20	20	30	30
No. Observations	12,066	12,066	13,887	13,887	14,438	14,438
No. Banks	1,483	1,483	1,566	1,566	1,612	1,612
No. Peer Groups	143	143	80	80	59	59
Bank and Country Controls	Y	Y	Y	Y	Y	Y
Additional Controls	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
First-Stage F-stat	30.59	26.77	19.75	17.99	13.61	11.27
First-Stage Instrument	0.017***	0.016***	0.019***	0.017***	0.016***	0.013***
	(5.531)	(5.174)	(4.445)	(4.242)	(3.690)	(3.357)
Mean of Dep. Variable	0.307	0.307	0.314	0.314	0.316	0.316

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bouwman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. All coefficients are scaled by the corresponding variable's standard deviation and *t*-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. The bank-specific (size, capital ratio, ROA, deposit share and NPL provisions) and country-level characteristics (GDP per capita, GDP growth volatility, concentration and prudential regulation intensity) are all defined in Table 1.1. Additional bank and country controls include banks' liquidity ratio (liquid assets/total assets), non-interest revenue share (non-interest income/total income) and cost-to-income ratio, as well as global integration (imports plus exports of goods and service to GDP), deposit insurance and IFRS (dummy variables equal to 1 if an explicit deposit insurance scheme and IFRS, respectively, is in place in country j in year t, and 0 otherwise). Peer banks' average characteristics comprise the same set of bank-specific controls in a given specification, but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. First-Stage F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic testing for weak instruments. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

the 1% level in the 1st stage of the 2SLS estimation in all specifications and the cluster-robust Kleibergen and Paap (2006) F-statistic also rejects the hypothesis of a weak IV.

Together with the relevance condition, the exclusion restriction implies that the only role the instrument plays in influencing the outcome variable is through its effect on the endogenous variable. In other words, this identification strategy only solves the endogeneity problem if the foreign parent bank-holding group does not *directly* influence the liquidity mismatch decisions of a domestic bank i. Thus, the estimates may be biased if the liquidity created by the foreign parent is correlated with either an omitted characteristic of peer banks that is relevant for bank i's liquidity policy, or an omitted bank i liquidity creation determinant. While the results discussed above suggest a limited role of the former, the latter concern is addressed as follows.

First, columns (1) to (3) of Table 1.3 report the results of an extended version of model (1.1) with country*year fixed-effects for country-year pairs with more than one peer group. Despite slightly smaller in magnitude, the estimated coefficients are still economically and statistically significant, with estimates ranging from 4.1 to 5.9 percentage point increase in bank i's liquidity creation as a result of a one standard deviation increase in the liquidity created by their respective competitors. This result corroborates the previous findings and helps ruling out alternative explanations such as the effect being driven by changes in regulations or supervisory effort that the model is not able to perfectly control for.

Second, to mitigate potential concerns that the results may still be biased due to omitted time-varying bank characteristics, I apply the methodology developed by Altonji, Elder, and Taber (2005) to quantify the relative importance of any remaining omitted variable bias. Coefficient stability is computed as the ratio between each coefficient estimate including controls as reported in Table 1.2 (numerator), and the difference between the latter and the coefficient derived from a regression with the same number of observations but without any controls (denominator). The results suggest that to explain the full effect of peers' liquidity creation, the covariance between unobserved factors and peers' liquidity creation would need to be between 3.82 to 9.32 times as high as the covariance of the included controls – in comparison, Altonji, Elder, and Taber (2005) estimate a ratio of 3.55 which they interpret as evidence that unobservables are unlikely to explain the effect they analyze. Accordingly, one can conclude that the likelihood that unobserved heterogeneity explains the documented peer effects is likely to be small.

Finally, the identifying assumption may still not be satisfied if the country where the foreign parent bank-holding company is headquartered and the country where the domestic banks operate were subject to similar shocks that could influence the liquidity they both create. To further address this concern, I repeat the analysis with an alternative IV where, instead of using the raw liquidity creation of the foreign parent bank-holding group as an instrument, the common variation in this measure (e.g., time-varying shocks common to all countries or country-specific) is purged as follows. First, I regress the liquidity created by the foreign parent with (i) observed country-level characteristics and country and time fixed-effects, or with (ii) country×time fixed effects. Then, the estimated residuals from each of these two models are used to instrument for peer firms' liquidity mismatch choices: $\hat{\varepsilon}_{p,j,t} = \widehat{LC}_{p,j,t} - \hat{\tau}' Z_{j,t-1} - \widehat{\omega_j} - \widehat{\upsilon_t}$, and $\hat{\upsilon}_{p,j,t} = \widehat{LC}_{p,j,t} - \widehat{m}_{tj}$, respectively. Such residuals should better capture the idiosyncratic nature of the foreign parents' liquidity transformation risk-management policies and thus offer a useful robustness test for identifying exogenous variation. In line with the results in Table 1.2, the coefficient estimates reported in Panel A of Appendix Table 1.6 remain significant.

Robustness tests. I also conduct a battery of additional tests to ensure that previous findings are robust. First, to ensure that the results are not being driven by the choice of instrument used to identify peer banks' liquidity creation choices, Columns (4) to (6) of Table 1.3 show that the previous estimates are robust to the use of an alternative IV based on market data. In detail, following the identification strategy in Leary and Roberts (2014), the liquidity mismatch decisions of competitors are now instrumented with the lagged idiosyncratic component of peer banks' equity returns. Intuitively, one extracts the idiosyncratic variation in stock returns using a traditional asset pricing model augmented by a factor to purge common variation among peers. The residual from this model is then lagged by one year and used to extract the exogenous variation in peer banks' liquidity choices – see a detailed description of the methodology in Appendix A. Due to the bank-specific nature of idiosyncratic stock returns and the vast asset pricing literature aimed at isolating this component, the instrument is unlikely to affect individual bank's liquidity decisions directly. Besides, stock returns are relatively free from manipulation and impound most, if not all, value-relevant events (Leary and Roberts, 2014). Finally, the instrument must be correlated with liquidity decisions of peers and there is a substantial literature linking banks' funding policies to stock returns e.g., Beltratti and Stulz (2012). Compared to the main identification strategy used in this paper, however, this instrument only allows to identify the sub-set of publicly-listed banks in the sample. Nevertheless, the main results remain unchanged.

Second, given that in the benchmark case each bank i in country j in year t belongs to a certain peer group of up to 30 banks based on their size, bank 30 and 31 in a size rank, for instance, would never interact with each other as they belong to different peer groups. Besides, bank 30 would give equal weight to the liquidity profile of banks 1, 2,. . . , 29, even if there is a substantial difference between the size of bank 1 and bank 29. To address this issue, I construct peer weighted-averages based on the size similarity (inverse of the Euclidean distance) between all banks operating in country j in year t i.e., the smaller the distance between two banks in terms of size, the more weight the relationship has. The peer influence weight between bank i and p operating in the same country in the same year is defined as:

$$Weight_{Size-Similarity_{i-p,j,t}} = \frac{\max(TA_{j,t}) - |TA_{i,j,t} - TA_{p,j,t}|}{\sum_{p=1}^{N} \max(TA_{j,t}) - |TA_{i,j,t} - TA_{p,j,t}|} \tag{1.2}$$

where $\max(TA_{j,t}) - |TA_{i,j,t} - TA_{p,j,t}|$ is the inverse of the Euclidean distance between the size of bank i and p in country j in year t, and $\sum\limits_{p=1}^{N} \max(TA_{j,t}) - |TA_{i,j,t} - TA_{p,j,t}|$ is the sum of all the inverse size distances in country j in year t. By construction, the sum of weights in each country in each year is equal to 1. The estimate presented in Column (7) of Table 1.3 is not only economically and statistically significant, but also in line in terms of magnitude with the coefficients reported in Table 1.2.

Third, columns (8) to (10) of Table 1.3 present the results of a falsification test where the analysis is conducted under the assumption that individual commercial banks follow other financial institutions of similar size and business model, but irrespective the country where they operate. This test is particularly important to ensure peer groups are defined correctly. In practice, I first rank all banks operating in the 32 OECD countries according to their size (total assets), group them into peer groups of 10, 20 or 30 banks according to the size rank in a given year, and then construct the peer averages for each bank accordingly while excluding bank i. The reported estimates show no statistically significant results for the coefficient of interest no matter how peer groups are defined. In other words, individual banks liquidity mismatch policies are not sensitive to those of banks of similar size that operate abroad. This is consistent with the a priori assumption when forming peer groups that within-country banks are expected to have higher incentives to mimic their peers.

Fourth, the identifying assumption that a foreign-owned subsidiary considers the liquidity mismatch policy of its parent bank-holding company (in addition to those of its domestic peers) may be more appropriate when the subsidiary is not too small or not too large relative to its parent. On the one hand, when a subsidiary is only an insignificant part of the foreign parent, their liquidity mismatch policies may be very different due to the considerable dissimilarity in terms of size. On the other hand, if the subsidiary is a large part of the parent, there may be little difference between the subsidiary and the parent's liquidity creation decisions - even when removing the balance-sheet characteristics of the former from the latter when computing the IV. While there are not many of these extreme cases in the sample, the results reported in Panel B of Appendix Table 1.6 show that the results are robust to the exclusion of foreign parent bank-holding groups for identification purposes (i.e., as part of the IV) in which their respective subsidiaries are more the 25% or less than 0.5% of the parents' size, or more the 50% or less than 5% of the parents' size. In this case, foreign-owned subsidiaries operating in OECD countries only enter in the specifications through the computation of the average liquidity creation of the peers of domestic banks, and the parents of the foreign-owned subsidiaries (that can be based in any country) only enter in the regressions through the IV used to identify the average liquidity creation of the peers of domestic banks.[25]

Fifth, the conclusions also do not change when considering the inverse of the NSFR (NSFRi) an alternative, though complementary, liquidity mismatch indicator i.e., while liquidity creation is an indicator of current iliquidity, the NSFR captures what iliquidity would be under a stress scenario (Berger and Bouwman, 2015). Appendix Table 1.8 follows the same structure of Table 1.2 and the reported 2SLS estimated coefficients corroborate the previous findings: (i) the first-stage regression coefficient estimates and the Kleibergen and Paap (2006) F-statistic show that the instrument is relevant and not weak; (ii) the estimates on the coefficient of interest, Peers' NSFRi, indicate that the relationship between the liquidity transformation risk of bank i and those of its peers is both positive and highly statistically significant in all specifications.

US evidence. As a final robustness test, I reiterate the previous analysis when considering a quarterly sample of banks operating in the US. Restricting the analysis to a panel of US banks serves multiple purposes. First, using data from "Call Reports" ensures that the results are not driven by potential problems in Bankscope in terms of different definition of certain B/S categories across countries. Second, it preserves homogeneity in terms of regulatory framework, accounting standards and macroeconomic conditions. Third, it allows testing whether the results on peer influence are sensitive to the use of higher frequency data. Finally, since the information provided in "Call Reports" is considerably more granular, it also allows using the Berger and Bouwman (2009) on-and-off-balance-sheet ("catfat") liquidity creation measure as dependent variable. The latter is particularly relevant given the extensive literature highlighting the importance of off-balance-sheet liquidity creation through loan commitments, standby letters of credit and other claims to liquid funds (e.g.,

[25] The main findings also remain unchanged (i) when excluding all foreign-owned subsidiaries from the estimations; (ii) when using the lagged peer banks' liquidity creation (instead of a contemporaneous measure) as the main explanatory variable; (iii) without winsorizing any of the control variables; and (iv) when removing from the sample banks with asset growth above 75% in any of the years they are active since these may have been involved in mergers and acquisitions - see Appendix Table 1.7.

Boot, Greenbaum, and Thakor, 1993; Kashyap, Rajan, and Stein, 2002). In the US, for instance, this accounts for almost half of all liquidity created (Berger and Bouwman, 2009).

In detail, Table 1.4 reports two-stage least squares estimates of model (1.1) using both the Berger and Bouwman (2009) "catfat" (columns 1 to 3) and "catnonfat" (columns 4 to 6) liquidity creation measures as dependent variables i.e., on-and-off-balance-sheet and on-balance-sheet liquidity creation divided by total assets, respectively. Since there are no corresponding quarterly-level data for most parents of foreign-owned subsidiaries operating in the US, it is not possible to use here this paper's main identification strategy based on Bramoullé, Djebbari, and Fortin (2009) and De Giorgi, Pellizzari, and Redaelli (2010). Besides, the relatively small number of smaller, mostly regional, foreign-owned subsidiaries would not allow to identify a large proportion of domestic US banks in the sample. To counter this issue, I follow Leary and Roberts (2014) and, as in columns (4)-(6) of Table 1.3, use as IV the lagged peer bank average equity return shock. In this case, standard errors are clustered at the bank level since the instrument varies across banks and over time. The estimated coefficients are still significant as well as remarkably similar in terms of magnitude across the liquidity creation measures with and without off-balance-sheet exposures. This suggests that peer banks may have a negligible impact in the liquidity created by individual banks off the balance-sheet. I explore this in more detail in the following.

1.4.2 Mechanisms and Heterogeneity

Asset vs. liability-side of liquidity creation. The results so far show that competitors play an significant role in determining variations in liquidity mismatch policies of individual banks. However, peer influence can be concentrated or at least affect in a dissimilar way the liquidity created on the asset and liquidity sides of banks' balance-sheets. Berger, Bouwman, Kick, and Schaeck (2016a), for instance, show that capital support measures reduce banks' asset-side liquidity creation while increasing by a similar magnitude the liquidity created on their liability-side. To better examine the mechanisms through which these adjustments operate, I decompose aggregate liquidity creation into its individual elements (i.e., asset-side, liability-side and off-balance-sheet liquidity creation - all normalized by bank assets) and regress each of them on peer banks' corresponding component of liquidity creation.

The results reported in Table 1.5 indicate that peer effects in liquidity creation decisions are concentrated on the asset-side of banks' balance-sheets. Specifically, Panel A considers the cross-country OECD sample with annual frequency as in Table 1.2 where the instrument is defined as the foreign subsidiary's parent asset or liability-side liquidity creation within each peer group, and with standard errors robust to heteroskedasticity and within peer group dependence. Instead, Panel B focuses on the US sample with quarterly frequency as in Table 1.4 where the instrument is the lagged peer bank average equity return shock, and with standard errors clustered at the bank-level. The reported estimates show no statistically significant results for liability-side liquidity creation, a result robust irrespective of the sample, identification strategy and peer group definition used. Appendix Table 1.9 presents the results with total liquidity creation further decomposed into its off-balance-sheet component when using the quarterly US sample. As with liability-side liquidity creation, the findings indicate that competitors' influence also does not operate via liquidity created off the balance-sheet. Overall, consistent with the evidence in Rajan (1994) and Uchida and Nakagawa (2007), the results suggest this type of collective risk-taking behavior is driven by liquidity created on the asset-side, of which lending is a key component.

Heterogeneity. What type of banks mimic their competitors? Although the results so far focused on estimating average coefficients, the strength of the effect is likely to vary with bank characteristics (e.g., capital, profitability) and over the business cycle. Table 1.6 presents the results exploiting the heterogeneity in the coefficient β from model (1.1) by interacting the main explanatory variable of interest, peers' liquidity creation, with indicator variables identifying (i) the lower, intermediate and upper thirds of each interaction variable's distribution within a country-year, and (ii) the pre-crisis (1999-2006), crisis (2007-2009), and post-crisis periods (2010-2014). To avoid redundancy, the results reported are based on the benchmark peer group definition as in specification (3) of Table 1.2 where competitors are defined as other commercial banks operating in the same country in the same year, and grouped into a network of 20 banks according to their size.

Table 1.3: Peer effects in banks' liquidity mismatch decisions – additional tests

	Country-Year Fixed Effects			Alternative Instrument			Weighted Peer Avg.	Peer Groups Defined Globally		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Peers' Liquidity Creation	0.041***	0.040**	0.059*	0.096***	0.087***	0.088***	0.088**	0.009	0.002	0.008
	(2.656)	(2.340)	(1.918)	(3.217)	(3.532)	(3.528)	(2.338)	(0.638)	(0.077)	(0.382)
Peer Group Size	10	20	30	10	20	30	-	10	20	30
No. Observations	11,674	11,631	10,382	3,007	3,007	3,007	15,529	14,937	15,819	15,923
No. Banks	1,447	1,348	1,192	293	293	293	1,680	1,652	1,677	1,689
No. Peer Groups	139	68	42	36	25	22	-	126	64	43
Bank Characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Peers Avg. Characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country Controls	-	-	-	Y	Y	Y	Y	Y	Y	Y
Year FE	-	-	-	Y	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country-Year FE	Y	Y	Y	N	N	N	N	N	N	N
First-Stage F-stat	28.32	15.37	9.68	18.88	31.14	29.28	24.91	14.35	7.535	4.427
First-Stage Instrument	0.016***	0.016***	0.010***	0.007***	0.010***	0.009***	0.010***	0.008***	0.004***	0.004**
	(5.322)	(3.920)	(3.111)	(4.345)	(5.580)	(5.411)	(4.991)	(3.788)	(2.745)	(2.104)
Mean of Dep. Variable	0.3037	0.3074	0.3142	0.389	0.389	0.389	0.318	0.322	0.322	0.323

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bouwman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. Bank-specific (size, capital ratio, ROA, deposit share and provisions) and country-level characteristics (GDP per capita, GDP growth volatility, liquidity regulation, deposit insurance and concentration) are all defined in Table 1.1. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. First-Stage F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic testing for weak instruments. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

Table 1.4: Peer effects in banks' liquidity mismatch decisions – US quarterly sample

	"Catfat" Liquidity Creation			"Catnonfat" Liquidity Creation		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' Liquidity Creation	0.042**	0.046*	0.054**	0.037***	0.045**	0.054***
	(2.287)	(1.840)	(2.250)	(2.591)	(2.332)	(2.585)
Peer Group Size	10	20	30	10	20	30
No. Observations	16,784	16,784	16,784	16,784	16,784	16,784
No. Banks	597	597	597	597	597	597
Bank Characteristics	Y	Y	Y	Y	Y	Y
Peers Avg. Controls	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
First-Stage F-stat	28.08	27.02	38.48	35.07	39.41	47.24
First-Stage Instrument	-0.003***	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***
	(-5.299)	(-5.198)	(-6.203)	(-5.922)	(-6.278)	(-6.873)
Mean of Dep. Variable	0.398	0.398	0.398	0.305	0.305	0.305

The table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bouwman (2009) "catfat" and "catnonfat" Liquidity Creation measures as dependent variables i.e., on-and-off-balance-sheet and on-balance-sheet liquidity creation divided by total assets, respectively. The quarterly bank-level Liquidity Creation data is obtained from Christa Bouwman's website and the remaining bank balance-sheet information is collected from the US Call Reports. Summary statistics are presented in Appendix Table 1.4. The instrument is the Leary and Roberts (2014) lagged peer bank average equity return shock. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within bank dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. Bank-specific characteristics include log total assets, capital ratio, ROA, deposit share and NPL provisions. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i's observation. All control variables are lagged by one quarter. First-Stage F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic testing for weak instruments. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

The results in columns (1) to (5) of Table 1.6 show that peer effects in banks' liquidity creation decisions are concentrated in less profitable and more risky banks with lower capital, lower deposit share, lower liquidity ratios, and higher non-interest revenue share. These findings are consistent with collective risk-taking being driven by the incentive of improving profitability (e.g., Farhi and Tirole, 2012; Ratnovski, 2009), and indicate that higher levels of funding liquidity risk are not being compensated with higher capital ratios that could increase a bank's probability of survival during the crisis (Berger and Bouwman, 2013). In fact, the peer effects of interest are not statistically significant for banks with high capital, a result consistent with theory showing that higher capital strengthens banks' monitoring

Table 1.5: Asset vs. liability side of liquidity creation

	Asset-Side Liq. Creation			Liability-Side Liq. Creation		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cross-country sample (annual frequency)						
Peer Banks' Asset-Side LC	0.040***	0.041**	0.061**			
	(3.152)	(2.007)	(2.335)			
Peer Banks' Liability-Side LC				0.013	0.014	-0.014
				(0.430)	(0.641)	(-0.079)
Peer Group Size	10	20	30	10	20	30
No. Observations	12,109	13,940	14,491	12,109	13,940	14,491
No. Banks	1,483	1,566	1,612	1,483	1,566	1,612
Bank, Peer and Country Controls	Y	Y	Y	Y	Y	Y
Year and Bank FE	Y	Y	Y	Y	Y	Y
First-Stage F-stat	39.23	10.82	10.67	5.64	4.72	0.09
Mean of Dep. Variable	0.145	0.164	0.168	0.152	0.148	0.146
Panel B: US sample (quarterly frequency)						
Peer Banks' Asset-Side LC	0.034***	0.029**	0.049**			
	(2.688)	(2.293)	(2.406)			
Peer Banks' Liability-Side LC				-0.055	-0.025	0.059
				(-0.297)	(-1.013)	(1.089)
Peer Group Size	10	20	30	10	20	30
No. Observations	16,784	16,784	16,784	16,784	16,784	16,784
No. Banks	597	597	597	597	597	597
Bank and Peer Controls	Y	Y	Y	Y	Y	Y
Quarter and Bank FE	Y	Y	Y	Y	Y	Y
First-Stage F-stat	43.37	83.73	58.24	0.13	7.09	2.18
Mean of Dep. Variable	0.121	0.121	0.121	0.185	0.185	0.185

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the asset and liability-side components of the Berger and Bouwman (2009) Liquidity Creation measure (both divided by total assets) as dependent variables. Panel A shows the results when using the sample of 32 OECD countries with annual frequency as in Table 1.2 - where the instrument is defined as the foreign subsidiary's parent asset or liability-side liquidity creation within each peer group. Panel B reports the results when using the US sample with quarterly frequency as in Table 1.4 - where the instrument is the Leary and Roberts (2014) lagged peer bank average equity return shock. All coefficients are scaled by the corresponding variable's standard deviation. t-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence in Panel A, and to heteroskedasticity and within bank dependence in Panel B. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific (size, capital ratio, ROA, deposit share and NPL provisions) and country-level characteristics (GDP per capita, GDP growth volatility, liquidity regulation, deposit insurance and concentration) are all defined in Tables 1 (OECD sample) and Appendix Table 1.4 (US sample). Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i's observation. All control variables are lagged by one period. First-Stage F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic testing for weak instruments. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

incentives (Mehran and Thakor, 2011) and lowers asset-substitution moral hazard (Morrison and White, 2005). Similarly, in the theoretical framework of Albuquerque, Cabral, and Guedes (2017), the incentive to use more relative performance evaluation and thus invest in correlated projects leading to systemic risk also increases with bank leverage.

In line with the prevalent view in the literature suggesting that banks' risk management tends to be procyclical with more aggressive risk-taking during economic booms and more conservative investments in downturns (e.g., Acharya and Naqvi, 2012; Acharya, Shin, and Yorulmazer, 2011; Thakor, 2016), the coefficient estimates in Column (6) show that strategic liquidity mismatch choices are more prevalent in non-crisis years. Despite not statistically significant in crisis years, such type of risk-taking behavior is still present after 2007-2009 global financial crisis, thus highlighting the need for a new macroprudential framework allowing for a more efficient systemic liquidity risk management.[26]

Table 1.7 examines more directly the potential channels driving the correlated balance-sheet exposures. In detail, banks are first classified as small, medium or large by splitting the within country-year distribution of banks' total assets into these three groups. The middle third of the distribution is excluded for each of the regressions to allow for a more accurate comparison between smaller and larger banks. The peer averages are then constructed based on the following scenarios: (i) small banks mimicking small banks and large banks mimicking large banks; (ii) small banks mimicking large banks and large banks mimicking small banks; (iii) small banks mimicking small banks; (iv) large banks mimicking large banks. This analysis is particularly useful to shed light on the potential mechanisms behind this type of coordinated behavior (e.g., bailout guarantees, managers' compensation structures, learning motives), and understand whether these decisions are indeed likely to be strategic.

[26] This result is unlikely to be driven by changes in prudential regulations introduced after the crisis or by the Basel's first guidelines on the new liquidity regulations issued in 2010. First, changes in the intensity of capital requirements, interbank exposure limits, concentration limits, LTV ratios limits and reserve requirements are explicitly controlled for in the model following Cerutti, Correa, Fiorentino, and Segalla (2017). Second, these changes in regulation would imply that all banks adjust their portfolio towards reducing liquidity transformation risk. However, as I show later in the paper (see Table 1.8), such collective strategic behavior is asymmetric i.e., individual banks mimic their respective peers strongly when competitors are increasing risk-taking rather than decreasing it.

Table 1.6: Cross-sectional heterogeneity and the business cycle

Dep Var: Liquidity Creation	Capital Ratio	ROA	Deposit Share	Non-Interest Revenue	Liquidity Ratio	Business Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' Liq. Creation × I_{low}	0.088***	0.050*	0.076***	0.042	0.059***	
	(3.568)	(1.915)	(3.699)	(1.472)	(2.933)	
Peers' Liq. Creation × I_{medium}	0.036***	0.045***	0.041***	0.021*	0.033***	
	(3.089)	(3.489)	(3.586)	(1.811)	(3.215)	
Peers' Liq. Creation × I_{high}	0.012	0.019**	0.012	0.037***	0.020*	
	(1.131)	(2.222)	(1.160)	(3.869)	(1.785)	
Peers' Liq. Creation × $I_{pre-crisis}$						0.079***
						(5.221)
Peers' Liq. Creation × I_{crisis}						0.045
						(1.094)
Peers' Liq. Creation × $I_{post-crisis}$						0.116*
						(1.795)
No. Observations	13,887	13,887	13,887	13,887	13,887	13,887
No. Banks	1,566	1,566	1,566	1,566	1,566	1,566
Bank, Peer and Country Controls	Y	Y	Y	Y	Y	Y
Year and Bank FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Variable	0.314	0.314	0.314	0.314	0.314	0.314

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bouwman (2009) "catnonfat" Liquidity Creation measure as dependent variable (i.e., on-balance-sheet liquidity creation divided by total assets), and when interacting the main explanatory variable of interest, peers' liquidity creation, with indicator variables identifying (i) the lower, intermediate and upper thirds of each interaction variable's distribution within a country-year, and (ii) the pre-crisis (1999-2006), crisis (2007-2009), and post-crisis periods (2010-2014). To avoid redundancy, the results reported are based on the benchmark peer group definition as in specification (3) of Table 1.2 where competitors are defined as other commercial banks operating in the same country in the same year, and grouped into a network of 20 banks according to their size. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Bank-specific (size, capital ratio, ROA, deposit share and provisions) and country-level characteristics (GDP per capita, GDP growth volatility, liquidity regulation, deposit insurance and concentration) are all defined in Table 1.1. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

The results confirm that the size of competitors is a crucial determinant for individual banks' decision-making. Specifically, the coefficient estimates in columns (1) and (2) indicate that large and small banks' liquidity mismatch decisions are only sensitive to the choices of their respective counterparts.[27] In other words, as predicted by the theoretical literature on

[27] While peer groups in column (1) of Table 1.7 are defined in a different manner than in the main analysis (i.e., banks within a country-year pair split into three size groups, where each group can have any number of banks vs. banks within a country-year pair grouped into groups of 10, 20 and 30 banks of similar size), it is reassuring that the coefficient estimate is similar in magnitude to those in Table 1.2.

Table 1.7: Bank size and coordinated behavior

Dep Var: Liquidity Creation	(1)	(2)	(3)	(4)
	S → S & L→ L	S → L & L → S	S → S	L → L
Peers' Liquidity Creation	0.086***	0.025	0.051**	0.090***
	(6.105)	(0.675)	(2.421)	(5.810)
No. Observations	8,453	8,593	4,132	4,295
No. Banks	1,173	1,181	638	546
Bank, Peer and Country Controls	Y	Y	Y	Y
Year and Bank FE	Y	Y	Y	Y
Mean of Dep. Variable	0.299	0.302	0.259	0.338

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bouwman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets, and when classifying banks as small, medium or large by splitting the within country-year distribution of banks' total assets into these three groups. The middle third of the distribution is excluded for each of the regressions to allow for a more accurate comparison between smaller (S) and larger banks (L). The peer averages are then constructed based on the following scenarios: (i) small banks mimicking small banks and large banks mimicking large banks; (ii) small banks mimicking large banks and large banks mimicking small banks; (iii) small banks mimicking small banks; (iv) large banks mimicking large banks. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Bank-specific (size, capital ratio, ROA, deposit share and provisions) and country-level characteristics (GDP per capita, GDP growth volatility, liquidity regulation, deposit insurance and concentration) are all defined in Table 1.1. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

collective moral-hazard due to the LOLR bailout commitment i.e., the "too-many-to-fail" problem (e.g., Acharya and Yorulmazer, 2007; Farhi and Tirole, 2012), larger banks tend to mimic other larger banks, while smaller banks follow other smaller banks.[28] Nonetheless, in line with risk-taking being driven by the presence of RPE in compensation schemes that tends to be more prevalent in larger banks (Albuquerque, Cabral, and Guedes, 2017; Ilic,

[28]The results therefore suggest that learning (i.e., free-riding in information acquisition) is unlikely to play a major role in this setting since small banks' liquidity choices do not seem to be affected by the respective decisions of large banks. This differs from the findings of Leary and Roberts (2014) that consider a sample of listed non-financial US firms and show that peer firm relevance is driven by a leader–follower model in which small firms are sensitive to large firms, but not vice-versa. In contrast with other industries, however, the institutional framework (e.g., existence of government guarantees) and regulatory environment (e.g., strict regulations and guidelines on what the banks can and should do) in the banking sector make it less likely for such rational "herding" behavior driven by uncertainty regarding the optimal liquidity policy to occur.

Pisarov, and Schmidt, 2016), the results reported in columns (3) and (4) also show that such mimicking behavior is stronger in larger banks.[29]

1.4.3 Collective Risk-taking and Financial Sector Stability

While the previous results highlighted that individual banks do take into consideration their respective competitors' liquidity mismatch decisions when determining their own, I now examine the consequences of such behavior explicitly i.e., whether these correlated balance-sheet exposures have an adverse effect on both individual banks' default risk and overall systemic risk. Despite the theoretical literature being clear on the direction one should expect (e.g., Allen, Babus, and Carletti, 2012), to the best of my knowledge this is the first study that analyzes this issue empirically.

Asymmetric responses. In order to investigate the direction in which these peer effects operate, I start by examining whether the response of individual banks to the funding liquidity choices of competitors is asymmetric. In other words, this analysis aims to understand if this type of mimicking behavior is stronger when peers are on average increasing liquidity transformation risk rather than decreasing it. If banks' follow competitors with the same intensity when they are decreasing and increasing risk, the impact of such coordinated behavior on financial stability is likely to be small. To answer this question, I interact the main explanatory variable capturing the average liquidity created by competitors with (i) a dummy variable equal to 1 if peers' average liquidity creation decreased from periods *t-1* to *t*, a 0 otherwise; and (ii) a dummy variable equal to 1 if peers' average liquidity creation increased from periods *t-1* to *t*, and 0 otherwise.

Table 1.8 reports the findings. The results in columns (1) to (3) show that correlated liquidity transformation activities work asymmetrically, thus suggesting that this behavior is indeed strategic. In specific, individual banks mimic their respective peers strongly when

[29]It is important to note that, while insightful, these results do not aim to verify or reject a particular theory per se. The objective of this paper is not to take a definite view on what may be driving this type of mimicking behavior – this issue is left for future research. Rather, it stresses the importance of these peer effects for the stability of the financial system and the need of a regulatory tool to counter this issue.

Table 1.8: Asymmetric behavior

	Liquidity Creation			Δ Liquidity Creation		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' Liq. Creation × $I_{LCdecreased}$	0.068***	0.054**	0.086***			
	(3.140)	(2.373)	(3.496)			
Peers' Liq. Creation × $I_{LCincreased}$	0.076***	0.087***	0.109***			
	(3.154)	(4.072)	(4.441)			
Δ Peers' Liq. Creation × $I_{LCdecreased}$				-0.015	-0.057	-0.068
				(-0.369)	(-1.235)	(-0.737)
Δ Peers' Liq. Creation × $I_{LCincreased}$				0.070*	0.051***	0.059*
				(1.770)	(3.386)	(1.691)
Peer Group Size	10	20	30	10	20	30
No. Observations	12,066	13,887	14,438	9,511	11,572	12,035
No. Banks	1,483	1,566	1,612	1,218	1,337	1,358
Bank, Peer and Country Controls	Y	Y	Y	Y	Y	Y
Year and Bank FE	Y	Y	Y	Y	Y	Y
Mean Dep. Var	0.306	0.314	0.316	0.001	0.001	0.001

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bouwman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets, and when interacting the main explanatory variable capturing the average liquidity created by competitors with (i) a dummy variable equal to 1 if peers' average liquidity creation decreased from periods *t-1* to *t*, a 0 otherwise; and (ii) a dummy variable equal to 1 if peers' average liquidity creation increased from periods *t-1* to *t*, and 0 otherwise. All coefficients are scaled by the corresponding variable's standard deviation and *t*statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Bank-specific (size, capital ratio, ROA, deposit share and provisions) and country-level characteristics (GDP per capita, GDP growth volatility, liquidity regulation, deposit insurance and concentration) are all defined in Table 1.1. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank *i*. All control variables are lagged by one period. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

these competitors are increasing funding liquidity risk rather than decreasing it. Columns (4) to (6) show that the same conclusion holds when re-running the regressions in first differences i.e., in changes from one period to the next period.[30] These findings are therefore the first indication that the peer effects may in fact lead to lower financial stability due to increased maturity transformation risk in the banking system. Diamond and Rajan (2001) and Allen and Gale (2004), for instance, argue that banks' liquidity transformation activities are a fundamental driver of financial instability and suggest that bank failures are more likely to

[30]While using either bank fixed effects or first differences removes time-invariant bank-specific unobservables, the latter focuses on deviations of variables from their previous period values instead of deviations from the bank-level mean which may introduce look-ahead bias (Berger, Bouwman, Kick, and Schaeck, 2016a; Ellul and Yerramilli, 2013).

occur when the level of liquidity creation is high. Rajan (1994) and Acharya and Naqvi (2012) find that banks creating excessive liquidity also tend to engage in lending practices leading to asset bubbles, which ultimately result in future financial instability. Berger and Bouwman (2017) also show that banking crises in the US have been preceded by periods of abnormal liquidity creation, while Hong, Huang, and Wu (2014) show that systemic liquidity risk as measured by TED spreads was a major predictor of bank failures in 2009 and 2010.

Impact on financial stability: empirical model. Based on the identification strategy described in section 2 used to adequately deal with the reflection and correlated effects problems, I use the following regression specification as a first step to capture time and country-varying peer effects in liquidity decisions:

$$y_{i,j,t} = \mu_i + \beta_{j,t}\bar{y}_{-i,j,t} + \lambda'\bar{X}_{-i,j,t-1} + \gamma' X_{i,j,t-1} + \delta' Z_{j,t-1} + v_t + \varepsilon_{i,j,t} \tag{1.3}$$

where the indices i, j, and t correspond to bank, country, and year, respectively. Compared to model (1.1), the relationship between liquidity of bank i and liquidity of its peers, $\beta_{j,t}$, is now allowed to vary not only across countries, but also over time since degree of mimicking in bank risk exposures should vary as a function of the availability of correlated projects (e.g., Albuquerque, Cabral, and Guedes, 2017). As before, the dependent variable $y_{i,j,t}$ is a measure of bank's liquidity mismatch activity, $\bar{y}_{-i,j,t}$ denotes the peer banks' average liquidity excluding bank i in year t within country j, and $\bar{X}_{-i,j,t-1}$, $X_{i,j,t-1}$ and $Z_{j,t-1}$ are average peer banks' characteristics, bank-specific factors, and country-level controls, respectively.

In practice, I make use of the panel structure of the data and estimate model (1.3) for each country-year combination by shocking the average peer effect in the overall sample with two indicator variables specifying the country and year such that:

$$y_{i,j,t} = \mu_i + [\beta_0 + (\beta_1 \times I_{country} \times I_{year})]\bar{y}_{-i,j,t} + \lambda'\bar{X}_{-i,j,t-1} + \gamma' X_{i,j,t-1} + \delta' Z_{j,t-1} + v_t + \varepsilon_{i,j,t} \tag{1.4}$$

In the second step, the estimated coefficient on the peer effect of interest in model (1.3), $\hat{\beta}_{j,t}$, is used to run the following specification to gauge the impact of peer effects in liquidity choices on financial stability:

$$STA_{i,j,t} = \kappa + \delta\hat{\beta}_{j,t} + \gamma' X_{i,j,t-1} + \eta' Z_{j,t-1} + \mu_i + v_t + u_{i,j,t} \quad (1.5)$$

where the dependent variable $STA_{i,j,t}$ is a measure of default risk or contribution to systemic risk of bank i, $\hat{\beta}_{j,t}$ is the country and time-varying peer effect estimated in (1.3), and $X_{i,j,t-1}$ and $Z_{j,t-1}$ contain lagged bank and country-specific characteristics, respectively. As before, I also include bank and year fixed-effects in the model to control for unobserved heterogeneity and account for average differences across banks and time not captured by the other exogenous variables.

Impact on financial stability: results. Tables 1.9 and 1.10 analyze the main question of this paper directly by looking at the impact of peer effects in liquidity mismatch decisions on financial stability, both from a idiosyncratic and systemic risk perspective. The dependent variable in Table 1.9 is the distance-to-default (ln[Z-score]) when using a 3-year (columns 1-3) and 5-year (columns 4-6) window to compute the standard deviation of ROA. This measure captures the default (solvency) risk of individual institutions so that a lower Z-score implies a higher probability of default. I employ a set of firm-specific and country-level controls that previous literature (e.g., Beck, De Jonghe, and Schepens, 2013; Ellul and Yerramilli, 2013) consistently show to impact bank risk. These include bank-specific measures of size (total assets), deposit share (deposits-to-assets), credit risk (NPL provisions-to-assets), liquid asset holdings (liquid assets-to-total assets), efficiency (cost-to-income) and funding structure (non-interest revenue share), as well as country-level indicators of economic development (GDP per capita), economic stability (GDP growth volatility), concentration (Herfindahl index), and prudential regulation intensity.

As initially hypothesized, peer effects in liquidity mismatch choices are strongly negatively (positively) associated with Z-scores (banks' default risk). Importantly, this effect is both statistically and economically significant. For instance, a change in the peer effect in liquidity creation from one standard deviation below the mean to one standard deviation above the mean is associated with a decrease in the lnZscore$_{3y}$ of 8 to 12 percent. In other words, the number of standard deviations profits would have to drop before capital is depleted is

Table 1.9: Peer effects in banks' liquidity mismatch decisions and default risk

	$\ln\text{Zscore}_{3y}$			$\ln\text{Zscore}_{5y}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Effect:	-0.387***	-0.373***	-0.384***	-0.225***	-0.261***	-0.519***
Liq. Creation - $\widehat{\beta_{j,t}^{LC}}$	(-5.424)	(-4.441)	(-4.095)	(-3.176)	(-3.003)	(-4.539)
Peer Group Size	10	20	30	10	20	30
No. Observations	10,328	11,904	12,390	7,869	9,100	9,411
No. Banks	1,351	1,426	1,463	1,125	1,196	1,227
Adj. R-squared	0.478	0.477	0.477	0.623	0.623	0.624
Bank characteristics	Y	Y	Y	Y	Y	Y
Country controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Variable	3.687	3.693	3.700	3.357	3.363	3.361

This table reports coefficient estimates of model (1.5) using ln(Z-Score) as dependent variable. Z-score is defined as the sum of equity capital over total assets (E/A) and return on assets (ROA), divided by the 3 or 5-year rolling standard deviation of ROA. The peer effects in liquidity mismatch decisions are estimated with model (1.3) using liquidity creation as dependent variable ($\widehat{\beta_{j,t}^{LC}}$), where the relationship between the liquidity of bank i and the liquidity of its peers is allowed to vary across countries and over time. Liquidity Creation is the Berger and Bouwman (2009) "cat nonfat" measure i.e., on-balance-sheet liquidity creation divided by total assets. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific characteristics include bank size, deposit share, NPL provisions, liquid assets, non-interest income revenue share and cost-to-income ratio, while country-level controls comprise GDP per capita, GDP growth volatility, local market concentration and prudential regulation intensity. All controls are lagged and defined in Tables 1 and Appendix Table 1.3. Robust standard errors clustered at the bank level are in parentheses. *, ** and *** designate that the test statistic is significant at the 10%, 5% and 1% levels.

reduced by 8 to 12 percent in such case. The conclusions do not change and the estimates are both quantitatively and economically similar when using the inverse of the NSFR (NSFRi) to capture liquidity transformation risk - see Appendix Table 1.10. In short, this economically significant increase in the default risk of individual banks provides evidence of the distressing effects of correlated balance-sheet exposures and liquidity mismatch decisions.

Tables 1.10 reports the results when looking at consequences of peer effects in liquidity creation choices on systemic risk as measured by the Marginal Expected Shortfall (columns 1-3) and Systemic Capital Shortfall (columns 4-6). Consistent with the previous findings on the negative effects of such strategic behavior for individual banks' default risk, the estimated coefficients indicate that peer effects in liquidity mismatch policies are also positively and significantly associated with overall systemic risk. As before, the results are robust across multiple model specifications, and when using NSFRi to capture liquidity risk (Appendix Table 1.11). The magnitude of the estimates also suggests that this effect is economically large: a change in the peer effect in liquidity creation from one standard deviation below the mean to one standard deviation above the mean is associated with an increase in MES of 0.17 to 0.21, and increase in SRISK of 0.47 to 1.19. This represents approximately a 7–8 and 15–31 percent increase from the mean MES and SRISK, respectively.

It is important to note, however, that both MES and SRISK are based on market data and therefore the sample size is significantly reduced when compared to Table 1.9. While the estimated coefficients are still significant at conventional levels notwithstanding the potential power issues in the regressions, I also perform an out-of-sample test to ensure the results are robust. In detail, Appendix Table 1.12 reports results when using instead the US quarterly sample of listed banks as in Table 1.4 and CoVaR (Adrian and Brunnermeier, 2016) as the dependent variable using data made available by the authors until 2013Q2. The main conclusions remain the same, even when analyzing a higher frequency bank-level dataset with a different measure to capture systemic risk.

In short, the results in Tables 1.9, 1.10, and Appendix Table 1.10-Appendix Table 1.12 together provide robust and novel empirical evidence that strategic complementarity in banks' liquidity mismatch policies decrease the stability of the financial system. Irrespective of the multitude of channels that have been put forward to explain such type of risk-taking behavior, these findings highlight the need of having a macroprudential tool that minimizes the propensity for banks to create excessive liquidity and collectively underprice liquidity risk. Such a binding requirement would allow for a more efficient systemic liquidity risk management that would ultimately reduce the potential taxpayer burden.

1.5 Conclusion

The global financial crisis distinctly exposed the negative implications of excessive liquidity transformation on financial stability and the macroeconomy. This outcome was achieved in part through banks' correlated exposures. Ultimately, liquidity mismatch decisions of

Table 1.10: Peer effects in banks' liquidity mismatch decisions and systemic risk

	MES			SRISK		
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Effect:	0.620*	0.915***	0.930**	3.123***	2.537***	1.773**
Liq. Creation - $\widehat{\beta_{j,t}^{LC}}$	(1.820)	(2.770)	(2.552)	(2.626)	(2.635)	(2.137)
Peer Group Size	10	20	30	10	20	30
No. Observations	1,783	2,197	2,374	1,783	2,197	2,374
No. Banks	244	273	290	244	273	290
Adj. R-squared	0.711	0.690	0.693	0.806	0.802	0.802
Bank characteristics	Y	Y	Y	Y	Y	Y
Country controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Variable	2.544	2.498	2.423	3.835	3.360	3.172

This table reports coefficient estimates of model (1.5) using the Marginal Expected Shortfall (MES) and Systemic Capital Shortfall (S-RISK) as dependent variables. MES is defined as bank i's expected equity loss (in %) in year t conditional on the market experiencing one of its 5% lowest returns in that given year. SRISK corresponds to the expected bank i's capital shortage (in billion USD) during a period of system distress and severe market decline. The peer effects in liquidity mismatch decisions are estimated with model (1.3) using liquidity creation as dependent variable ($\widehat{\beta_{j,t}^{LC}}$), where the relationship between the liquidity of bank i and the liquidity of its peers is allowed to vary across countries and over time. Liquidity Creation is the Berger and Bouwman (2009) "cat nonfat" measure i.e., on-balance-sheet liquidity creation divided by total assets. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific characteristics include bank size, capital ratio, ROA, deposit share, NPL provisions, liquid assets, non-interest income revenue share and cost-to-income ratio, while country-level controls comprise GDP per capita, GDP growth volatility, local market concentration and prudential regulation intensity. All controls are lagged and defined in Tables 1 and Appendix Table 1.3. Robust standard errors clustered at the bank level are in parentheses. *, ** and *** designate that the test statistic is significant at the 10%, 5% and 1% levels.

individual banks spilled over to other institutions and markets, contributing to further losses and exacerbating overall liquidity stress. Such systemic liquidity risk was, judging by the extent of government intervention, clearly undervalued by both the private and public sectors.

In this regard, this paper empirically examines the extent to which banks' liquidity transformation activities are affected by the respective choices of competitors, and the impact of these strategic funding liquidity policies on the stability of individual banks and the financial system. Using a novel identification strategy exploiting the presence of partially overlapping peer groups, and incorporating a large sample of commercial banks operating in 32 OECD countries from 1999 to 2014, I find that financial institutions do take into consideration their peers' liquidity mismatch decisions when determining their own. Such collective risk-taking behavior is driven by liquidity created on the asset-side, of which lending is a key component, and concentrated in less profitable and more risky banks with lower capital, lower deposit share, lower liquidity ratios, and higher non-interest revenue share.

With respect to the consequences of this strategic behavior for the financial system, I first show that the response of individual banks to the funding liquidity choices of competitors is asymmetric: individual banks mimic their respective peers strongly when competitors are increasing liquidity transformation risk rather than decreasing it. I then show explicitly that peer effects in financial institutions' liquidity mismatch policies increase both individual banks' default risk and overall systemic risk. This effect is both statistically and economically significant which, from a macroprudential perspective, highlights the importance of dealing and properly regulating the systemic component of funding liquidity risk.

In fact, while the Basel III liquidity requirements, combined with improved supervision, should help to strengthen individual banks' funding structure and thus enhance banking sector stability, these liquidity standards are fundamentally micro-prudential in nature.[31] Despite recent proposals for macroprudential liquidity regulation such as time-varying LCR and NSFR ratios or a macroprudential liquidity buffer where each bank would be required to hold systemically-liquid assets (IMF, 2011), policymakers and regulators have yet to establish a concise macroprudential framework that mitigates the possibility of a simultaneous liquidity need by financial institutions. Since information spillovers are a defining characteristic of panics due to financial agents' imperfect knowledge regarding cross-exposures, and given that, as shown in this paper, these information spillovers between banks do occur, a static and time-invariant microprudential liquidity requirement that mainly depends on individual banks' idiosyncratic risk (rather than system-wide conditions) may not be suited to prevent a

[31]Most developed economies have also recently introduced formal bank resolution and bail-in regimes that involve the participation of bank creditors in bearing the costs of restoring a distressed bank and include heavy restrictions on taxpayer support. Despite the potential negative but limited short-term costs for the real economy (Beck, Da-Rocha-Lopes, and Silva, 2018a), the move from bailouts to credible bail-in frameworks represents an important step to mitigate the incentives for collective risk-taking behavior. On the other hand, while the Financial Stability Board (FSB) issued in 2009 the core principles for the design of pay structures currently being implemented in different countries, Albuquerque, Cabral, and Guedes (2017) argue that these largely omit the role that RPE plays in creating systemic risk.

systemic liquidity crisis. As argued by Dewatripont, Rochet, and Tirole (2010), "a 1 percent probability of failure means either that 1 percent of the banks fail every year or, alternatively, that the whole banking system fails every hundred years - quite distinct outcomes. Therefore it is crucial for regulators to find ways of discouraging herding behavior by banks, or at least penalizing excessive exposure to the business cycle".

Appendix 1.A. Computation of the Stock Return Shock

To extract the idiosyncratic component of stock returns, I follow Leary and Roberts (2014) by using, in addition to the market factor traditional in asset pricing models, an industry factor to remove any common variation in returns across the same peer group. The model is specified as follows:

$$R_{i,j,t} = \alpha_{i,j,t} + \lambda_{i,j,t}\left(RM_{j,t} - Rf_{j,t}\right) + \phi_{i,j,t}\left(\bar{R}_{-i,j,t} - Rf_{j,t}\right) + \epsilon_{i,j,t} \tag{1.6}$$

where $R_{i,j,t}$ refers to the stock return for bank i in country j over period t, $(RM_{j,t} - Rf_{j,t})$ is the excess market returns (i.e., market factor) and $\left(\bar{R}_{-i,j,t} - Rf_{j,t}\right)$ is the excess return on an equally-weighted portfolio excluding bank i's return (i.e., industry factor). The intercept $\alpha_{i,j,t}$ measures the mean monthly abnormal return. I use the one-month US T-Bill Rate to proxy for the risk-free rate and the Morgan Stanley Capital International (MSCI) equity market index of each country to proxy for their respective market factor. The model is estimated for each bank in a rolling regression using a minimum of 24 and a maximum of 60 past monthly returns. In detail, to compute expected and idiosyncratic returns of bank i in month m of year t, I first estimate equation (1.6) using monthly returns from month m of year $t-5$ to month $m+12$ of year $t-1$. Using the estimated coefficients and the factor returns from bank i in month m of year t, the idiosyncratic return component, $\hat{\eta}_{i,j,t}$, is computed as the difference between the actual return $R_{i,j,t}$ and the expected return $\hat{R}_{i,j,t}$:

$$\hat{R}_{i,j,t} = \hat{\alpha}_{i,j,t} + \hat{\lambda}_{i,j,t}\left(RM_{j,t} - Rf_{j,t}\right) + \hat{\phi}_{i,j,t}\left(\bar{R}_{-i,j,t} - Rf_{j,t}\right) \tag{1.7}$$

and,

$$\hat{\eta}_{i,j,t} = R_{i,j,t} - \hat{R}_{i,j,t} \tag{1.8}$$

The idiosyncratic return obtained from the above model is therefore the return of the bank after removing all known sources of systematic variation i.e., exposure to market and industry. Thus, the residuals obtained from (1.6) should be purely bank specific and hence, free from any commonalities across the bank. In order to ensure consistency with the frequency of accounting data, I compound the monthly idiosyncratic return component to have an annual measure. This quantity is then averaged over the peer banks for each country j in each year t, and the exogenous source of variation for peer banks' liquidity choices is the lagged average peer bank equity return shock.

Appendix 1.B. Additional Results

Appendix Table 1.1: Liquidity Creation and NSFR weights

Assets	***Liq.***	***Creation***	***RSF***	**Liabilities**	***Liq.***	***Creation***	***ASF***
Loans				**Interest-bearing Liabilities**			
Residential Mortgage Loans	0	Semi-liquid	85%	Customer Deposits – Current	0.5	Liquid	90%
Other Mortgage Loans	0.5	Illiquid	85%	Customer Deposits – Savings	0.5	Liquid	95%
Other Consumer/Retail Loans	0	Semi-liquid	85%	Customer Deposits – Term	0	Semi-liquid	95%
Corporate & Commercial Loans	0.5	Illiquid	85%	**Total Customer Deposits**			
Other Loans	0.5	Illiquid	85%	Deposits from Banks	0.5	Liquid	0%
Gross Loans				Other Deposits & ST Borrowings	0	Semi-liquid	0%
Less: Reserves for Impaired Loans/NPLs	0.5	Illiquid	100%	Long Term Funding	-0.5	Illiquid	100%
Net Loans				Derivatives	0.5	Liquid	0%
Other Earning Assets				Trading Liabilities	0.5	Liquid	0%
Loans and Advances to Banks	0	Semi-liquid	15%	**Total Funding**			
Government Securities	-0.5	Liquid	5%	**Non-interest Bearing Liabilities**			
Derivatives	-0.5	Liquid	50%	Other liabilities	-0.5	Illiquid	0%
Held to Maturity Securities	-0.5	Liquid	100%	**Total Liabilities**			
At-equity Investments in Associates	0.5	Iliquid	100%				
Trading Securities	-0.5	Liquid	50%				
Other Securities	-0.5	Liquid	50%				
Other Earning Assets	0.5	Illiquid	100%				
Total Earning Assets							
Non-earning Assets							
Cash and Due From Banks	-0.5	Liquid	0%	**Equity**			
Fixed Assets	0.5	Illiquid	100%	Common Equity	-0.5	Illiquid	100%
Other Non-earning Assets	0.5	Illiquid	100%	Other Equity	-0.5	Illiquid	100%
Total Assets				**Total Equity**			

This table presents the weights assigned to each bank balance sheet item to construct the Liquidity Creation and NSFRi measures. Liquidity Creation is the Berger and Bowman (2009) "catnonfat" measure i.e., on-balance-sheet liquidity creation divided by total assets. NSFRi (inverse of the Net Stable Funding Ratio) is defined as the ratio of the required amount of stable funding (RSF) to the available amount of stable funding (ASF). "Other Non-earning Assets" includes Foreclosed Real Estate, Goodwill, Other Intangibles, Current Tax Assets, Deferred Tax Assets and Discontinued Operations. "Other liabilities" comprises Credit Impairment Reserves and Other Reserves, Fair Value Portion of Debt, Deferred Liabilities, Discontinued Operations, Insurance Liabilities and Current Tax Liabilities. "Long-Term Funding" includes Senior Debt Maturing after 1 Year, Subordinated Borrowing and Pref. Shares and Hybrid Capital accounted for as Debt. "Other Equity" consists of Non-controlling Interest, Securities Revaluation Reserves, Foreign Exchange Revaluation Reserves, Fixed Asset Revaluations and Other Accumulated OCI and Pref. Shares and Hybrid Capital accounted for as Equity. "Other Securities" includes Trading Securities and at FV through Income, Available for Sale Securities and Other Securities. "Other Earning Assets" comprises Investments in Property, Insurance Assets and Other Earning Assets.

Appendix Table 1.2: Reported peer groups of largest US banks

	Wells Fargo	JPMorgan Chase	Citigroup	U.S. Bancorp	PNC	BNY Mellon	State Street	Capital One
American Express	X	X	X					X
Bank of America	X	X	X	X	X			X
BNY Mellon	X		X				X	
BB&T	X			X	X			X
Capital One	X		X		X		X	
Citigroup	X	X						X
Fifth Third	X			X	X			X
Goldman Sachs	X	X	X				X	
JPMorgan Chase	X		X	X	X	X	X	X
KeyCorp	X			X	X			
Morgan Stanley	X	X	X			X	X	
PNC	X		X	X		X	X	X
Regions	X			X	X			X
State Street	X					X		
SunTrust	X			X	X			X
U.S. Bancorp	X		X		X	X	X	X
Wells Fargo		X	X	X	X	X	X	X
AIG			X					
MetLife			X					
Prudential			X			X		
M&T Bank					X			
BlackRock						X	X	
Franklin Resources						X	X	
Charles Schwab						X		
Northern Trust						X	X	
Ameriprise							X	
Discover								X
Total No. Peers	16	6	13	9	11	11	12	12

This table presents the peer groups of the largest US banks in 2016 as reported in their publicly-available 2017 proxy statements. These comprise both (i) financial performance peers, which include other banks most directly competing for financial capital and customers, and that match the respective bank's scope, scale, business model/mix, and geography; and (ii) labor market peers, which also includes other banks of similar scope and scale but that directly compete for executive talent (e.g., Wells Fargo 2017 Proxy Statement).

Appendix Table 1.3: Additional summary statistics – OECD sample

Variables	N	Mean	SD	P25	P50	P75
Additional bank and country characteristics:						
Liquidity Ratio	14,438	0.078	0.097	0.015	0.039	0.103
Non-Interest Income Revenue	14,438	0.369	0.233	0.203	0.336	0.500
Cost-to-Income Ratio	14,438	0.633	0.285	0.502	0.622	0.744
Global Integration	14,438	0.823	0.623	0.501	0.614	0.962
Deposit Insurance	14,438	0.984	0.124	1.000	1.000	1.000
IFRS	14,438	0.201	0.400	0.000	0.000	0.000
Peer Averages (peer group size: 10 banks)						
Peers' Liquidity Creation	12,066	0.301	0.136	0.223	0.310	0.391
Peers' NSFRi	12,066	1.014	0.265	0.835	0.969	1.148
Peers' Size	12,066	8.143	1.980	6.596	7.931	9.555
Peers' Capital Ratio	12,066	0.104	0.049	0.069	0.095	0.126
Peers' ROA	12,066	0.006	0.007	0.003	0.006	0.010
Peers' Deposit Share	12,066	0.570	0.120	0.487	0.575	0.658
Peers' NPL Provisions	12,066	0.004	0.004	0.001	0.003	0.006
Peers' Liquid Asets	12,066	0.080	0.062	0.033	0.060	0.111
Peers' Banks' Cost to Income	12,066	0.636	0.162	0.557	0.638	0.722
Peers' Non-Interest Revenue	12,066	0.377	0.128	0.291	0.371	0.452
Peer Averages (peer group size: 20 banks)						
Peers' Liquidity Creation	13,887	0.309	0.120	0.237	0.320	0.389
Peers' NSFRi	13,887	1.009	0.220	0.854	0.982	1.120
Peers' Size	13,887	8.231	1.865	6.786	8.255	9.561
Peers' Capital Ratio	13,887	0.103	0.042	0.076	0.097	0.122
Peers' ROA	13,887	0.006	0.006	0.003	0.006	0.010
Peers' Deposit Share	13,887	0.577	0.110	0.500	0.577	0.653
Peers' NPL Provisions	13,887	0.005	0.004	0.002	0.003	0.006
Peers' Liquid Asets	13,887	0.079	0.058	0.035	0.058	0.110
Peers' Cost to Income	13,887	0.635	0.141	0.573	0.639	0.714
Peers' Non-Interest Revenue	13,887	0.370	0.113	0.294	0.372	0.440
Peer Averages (peer group size: 30 banks)						
Peers' Liquidity Creation	14,438	0.311	0.114	0.241	0.322	0.388
Peers' NSFRi	14,438	1.004	0.205	0.867	0.979	1.102
Peers' Size	14,438	8.250	1.753	7.056	8.210	9.553
Peers' Capital Ratio	14,438	0.103	0.039	0.076	0.097	0.122
Peers' ROA	14,438	0.006	0.006	0.003	0.006	0.010
Peers' Deposit Share	14,438	0.581	0.108	0.505	0.578	0.654
Peers' NPL Provisions	14,438	0.004	0.004	0.002	0.003	0.006
Peers' Liquid Asets	14,438	0.078	0.057	0.036	0.058	0.108
Peers' Cost to Income	14,438	0.636	0.133	0.577	0.644	0.711
Peers' Non-Interest Revenue	14,438	0.369	0.107	0.296	0.376	0.432

This table presents summary statistics for all the additional variables in the cross-country sample used in this study. These include the liquidity ratio (liquid assets/total assets), non-interest revenue (non-interest income/total income), cost-to-income ratio, global integration (imports plus exports of goods and service to GDP), deposit insurance (a dummy variable that equals 1 if an explicit deposit insurance scheme is in place in country j in year t, and 0 otherwise) and a dummy variable that equals 1 if IFRS is in place in country j in year t to account for potential reporting jumps at the time of a bank's accounting standards change. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. The full sample consists of 14,438 bank-year observations corresponding to 1,612 commercial banks operating in 32 OECD countries from 1999 to 2014.

Appendix Table 1.4: Summary statistics – US sample

Variables	N	Mean	SD	P25	P50	P75
Liquidity mismatch indicators:						
Liquidity Creation ("catfat")	16,784	0.398	0.150	0.300	0.402	0.498
Liquidity Creation ("catnonfat")	16,784	0.305	0.125	0.229	0.311	0.390
LC Asset-side	16,784	0.121	0.120	0.045	0.128	0.203
LC Liability-side	16,784	0.185	0.069	0.139	0.183	0.231
LC Off-balance-sheet	16,784	0.092	0.055	0.055	0.080	0.113
Bank-level characteristics:						
Size	16,784	14.58	1.435	13.54	14.26	15.26
Capital Ratio	16,784	0.095	0.022	0.080	0.092	0.106
ROA	16,784	0.005	0.007	0.003	0.005	0.009
Deposit Share	16,784	0.778	0.082	0.728	0.791	0.839
NPL Provisions	16,784	0.011	0.005	0.008	0.010	0.012

This table presents summary statistics for the main variables in the quarterly bank-level US sample. The latter is obtained from the FFIEC/FRB of Chicago "Call Reports" and which includes 597 commercial banks from 1999Q1 to 2014Q4. Liquidity Creation (LC) is either the Berger and Bouwman (2009) "cat nonfat" or "catnonfat" measure i.e., on-and-off-balance-sheet and on-balance-sheet liquidity creation divided by total assets, respectively. This information is collected from Christa Bowman's website. Bank-level characteristics include size (ln[total assets]), capital ratio (equity/assets), ROA (net income/assets), deposit share (customer deposits/assets), and NPL provisions (loan loss provisions/assets).

Appendix Table 1.5: Peer effects in banks' liquidity mismatch decisions – OLS estimates

Dep Var: Liquidity Creation	(1)	(2)	(3)	(4)	(5)	(6)
Peers' Liquidity Creation	0.031***	0.030***	0.048***	0.046***	0.056***	0.054***
	(7.990)	(7.904)	(8.934)	(9.294)	(8.431)	(8.933)
Peers' Size	0.008	0.010	0.009	0.011	0.007	0.009
	(0.963)	(1.332)	(0.990)	(1.268)	(0.615)	(0.821)
Peers' Capital Ratio	0.002	0.002	0.009	0.009	0.013*	0.013*
	(0.426)	(0.441)	(1.361)	(1.347)	(1.731)	(1.910)
Peers' ROA	0.001	-0.000	-0.001	-0.001	0.005	0.005
	(0.321)	(-0.037)	(-0.126)	(-0.299)	(1.003)	(1.319)
Peers' Deposit Share	-0.000	0.002	-0.004	-0.002	-0.000	0.002
	(-0.071)	(0.503)	(-0.844)	(-0.293)	(-0.002)	(0.319)
Peers' NPL Provisions	-0.000	-0.000	-0.001	-0.001	0.002	0.003
	(-0.092)	(-0.137)	(-0.354)	(-0.238)	(0.622)	(0.951)
Peers' Liquid Assets		0.003		0.004		0.006
		(0.847)		(0.933)		(1.256)
Peers' Cost to Income		-0.002		-0.001		0.002
		(-0.489)		(-0.336)		(0.293)
Peers' Non-Interest Revenue		0.008***		0.008**		0.009**
		(2.706)		(2.199)		(2.503)
Peer Group Size	10	10	20	20	30	30
No. Observations	12,066	12,066	13,887	13,887	14,438	14,438
No. Banks	1,483	1,483	1,566	1,566	1,612	1,612
Bank and Country Controls	Y	Y	Y	Y	Y	Y
Additional Controls	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Variable	0.307	0.307	0.314	0.314	0.316	0.316

This table reports OLS estimates of model (1.1) using the Berger and Bowman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroscedasticity and within bank dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. Bank-specific (size, capital ratio, ROA, deposit share and NPL provisions) and country-level characteristics (GDP per capita, GDP growth volatility, liquidity regulation, deposit insurance and concentration) are all defined in Table 1. Additional bank-specific controls include the share of wholesale funding (share of money market funding in money market funding and total deposits), cost to income ratio and non-interest revenue share (non-interest income in total income). The additional country-level controls are global integration (imports plus exports of goods and service divided GDP) and IFRS (dummy variable accounting for potential reporting jumps at the time of a bank's accounting standards change). Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

Appendix Table 1.6: Peer effects in banks' liquidity mismatch decisions – additional tests

Dep Var: Liquidity Creation	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Alternative IVs						
(i) Idiosyncratic liquidity creation of foreign parent: $\hat{\varepsilon}_{p,j,t} = \widehat{LC}_{p,j,t} - \hat{\tau}' Z_{j,t-1} - \widehat{\omega_j} - \hat{\upsilon}_t$						
Peers' Liquidity Creation	0.106***	0.107***	0.099***	0.099***	0.110***	0.115***
	(3.504)	(3.306)	(6.966)	(6.955)	(5.061)	(4.618)
	[12,066]	[12,066]	[13,887]	[13,887]	[14,438]	[14,438]
(ii) Idiosyncratic liquidity creation of foreign parent: $\hat{\upsilon}_{p,j,t} = \widehat{LC}_{p,j,t} - \widehat{m}_{tj}$						
Peers' Liquidity Creation	0.073**	0.078**	0.086***	0.085***	0.090***	0.089***
	(2.130)	(2.253)	(5.434)	(5.619)	(4.262)	(4.306)
	[12,066]	[12,066]	[13,887]	[13,887]	[14,438]	[14,438]
Panel B: Do not consider a foreign parent if its subsidiary is too small or too large						
(i) Exclude foreign parents if subsidiary is less than 0.5% or more than 25% of its size						
Peers' Liquidity Creation	0.057***	0.053**	0.073***	0.068***	0.091***	0.086***
	(2.856)	(2.505)	(4.638)	(3.945)	(5.754)	(4.748)
	[10,332]	[10,332]	[12,966]	[12,966]	[13,895]	[13,895]
(ii) Exclude foreign parents if subsidiary is less than 1% or more than 50% of its size						
Peers' Liquidity Creation	0.066***	0.062***	0.074***	0.070***	0.093***	0.089***
	(3.572)	(3.120)	(4.290)	(3.891)	(5.884)	(5.080)
	[9,495]	[9,495]	[12,049]	[12,049]	[13,702]	[13,702]
Peer Group Size	10	10	20	20	30	30
Bank and Country Controls	Y	Y	Y	Y	Y	Y
Additional Controls	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bowman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets. Panel A considers an alternative IV where, instead of using the raw liquidity creation of the foreign parent bank-holding group as an instrument, I first regress the liquidity created by the foreign parent with (i) observed country-level characteristics and country and time fixed-effects, or with (ii) country×time fixed effects. Then, the estimated residuals from each of these two models are used to instrument for peer firms' liquidity mismatch choices. Panel B excludes foreign parent bank-holding groups for identification purposes (i.e., as part of the IV) if their subsidiaries are too small or too large. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. The bank and country controls, and additional bank and country controls are the same as in Table 2. Peer banks' average characteristics comprise the same set of bank-specific controls in a given specification, but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

Appendix Table 1.7: Peer effects in banks' liquidity mismatch decisions – additional tests

Dep Var: Liquidity Creation	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Exclude all foreign-owned subsidiaries						
Peers' Liquidity Creation	0.041**	0.030*	0.054***	0.040**	0.066***	0.045*
	(2.404)	(1.707)	(3.114)	(2.176)	(2.790)	(1.691)
	[9,662]	[9,662]	[11,255]	[11,255]	[11,761]	[11,761]
Panel B: Standard errors clustered at the bank-level						
Peers' Liquidity Creation	0.058***	0.052***	0.064***	0.056***	0.091***	0.085***
	(3.689)	(3.197)	(4.332)	(3.593)	(4.701)	(3.615)
	[12,066]	[12,066]	[13,887]	[13,887]	[14,438]	[14,438]
Panel C: Lagged peers banks' liquidity creation						
Peers' Liquidity Creation	0.048**	0.043**	0.051**	0.041*	0.090***	0.081***
	(2.313)	(1.979)	(2.411)	(1.733)	(4.263)	(3.390)
	[12,066]	[12,066]	[13,887]	[13,887]	[14,438]	[14,438]
Panel D: No winsorizing of control variables						
Peers' Liquidity Creation	0.059***	0.053***	0.066***	0.059***	0.096***	0.091***
	(3.543)	(3.111)	(4.237)	(3.445)	(4.699)	(3.803)
	[12,066]	[12,066]	[13,887]	[13,887]	[14,438]	[14,438]
Panel E: Drop banks with asset growth above 75% in any of the years						
Peers' Liquidity Creation	0.057***	0.053***	0.062***	0.053***	0.076***	0.065**
	(3.295)	(2.937)	(3.681)	(2.936)	(3.261)	(2.440)
	[9,214]	[9,214]	[10,630]	[10,630]	[11,085]	[11,085]
Peer Group Size	10	10	20	20	30	30
Bank and Country Controls	Y	Y	Y	Y	Y	Y
Additional Controls	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the Berger and Bowman (2009) "catnonfat" Liquidity Creation measure as dependent variable i.e., on-balance-sheet liquidity creation divided by total assets. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within bank dependence in Panel B, and to heteroskedasticity and peer group dependence in all other panels. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. The bank and country controls, and additional bank and country controls are the same as in Table 2. Peer banks' average characteristics comprise the same set of bank-specific controls in a given specification, but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

Appendix Table 1.8: Peer effects in banks' liquidity mismatch decisions – NSFR

Dep Var: NSFRi	(1)	(2)	(3)	(4)	(5)	(6)
Peers' NSFRi	0.157***	0.156***	0.106***	0.099**	0.112**	0.107**
	(2.681)	(2.675)	(2.653)	(2.373)	(2.495)	(2.274)
Peers' Size	0.020	0.024	0.041*	0.045**	0.021	0.021
	(0.934)	(1.189)	(1.948)	(2.139)	(1.135)	(1.196)
Peers' Capital Ratio	0.011	0.012	0.015	0.013	0.018	0.017
	(0.813)	(0.903)	(1.375)	(1.194)	(1.578)	(1.566)
Peers' ROA	0.006	0.007	0.002	0.003	0.009*	0.01*
	(1.085)	(1.229)	(0.374)	(0.491)	(1.886)	(1.689)
Peers' Deposit Share	0.043**	0.045**	0.044**	0.044**	0.056**	0.055**
	(2.170)	(2.332)	(2.090)	(2.157)	(2.005)	(2.069)
Peers' NPL Provisions	0.002	0.003	-0.001	0.001	0.005	0.007
	(0.421)	(0.781)	(-0.164)	(0.235)	(1.192)	(1.369)
Peers' Liquid Assets		0.008		0.002		0.007
		(1.136)		(0.245)		(0.700)
Peers' Cost to Income		0.004		0.003		0.002
		(0.606)		(0.378)		(0.258)
Peers' Non-Interest Revenue		0.002		0.008		0.003
		(0.296)		(1.332)		(0.521)
Peer Group Size	10	10	20	20	30	30
No. Observations	12,066	12,066	13,887	13,887	14,438	14,438
No. Banks	1,483	1,483	1,566	1,566	1,612	1,612
No. Peer Groups	143	143	80	80	59	59
Bank and Country Controls	Y	Y	Y	Y	Y	Y
Additional Controls	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
First-Stage F-stat	13.50	13.43	15.72	15.48	10.67	10.34
First-Stage Instrument	0.018***	0.018***	0.019***	0.019***	0.014***	0.013***
	(3.674)	(3.665)	(3.965)	(3.934)	(3.266)	(3.216)
Mean of Dep. Variable	1.000	1.000	0.998	0.998	0.995	0.995

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the NSFRi (inverse of the Net Stable Funding Ratio) as dependent variable. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. All coefficients are scaled by the corresponding variable's standard deviation and t-statistics (in parentheses) are robust to heteroskedasticity and within peer group dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size. The bank-specific (size, capital ratio, ROA, deposit share and NPL provisions) and country-level characteristics (GDP per capita, GDP growth volatility, concentration and prudential regulation intensity) are all defined in Table 1. Additional bank and country controls include banks' liquidity ratio (liquid assets/total assets), non-interest revenue share (non-interest income/total income) and cost-to-income ratio, as well as global integration (imports plus exports of goods and service to GDP), deposit insurance and IFRS (dummy variables equal to 1 if an explicit deposit insurance scheme and IFRS, respectively, is in place in country j in year t, and 0 otherwise). Peer banks' average characteristics comprise the same set of bank-specific controls in a given specification, but are computed as the average across all banks within a certain peer group, excluding bank i. All control variables are lagged by one period. First-Stage F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic testing for weak instruments. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

Appendix Table 1.9: Peer effects in off-balance-sheet liquidity creation decisions

Dep Var: Off-Balance-Sheet Liquidity Creation	(1)	(2)	(3)
Peers' Off-Balance-Sheet Liquidity Creation	-1.611	0.024	0.029
	(-0.003)	(0.659)	(0.385)
Peer Group Size	10	20	30
No. Observations	16,784	16,784	16,784
No. Banks	597	597	597
Bank and Peer Controls	Y	Y	Y
Quarter and Bank FE	Y	Y	Y
First-Stage F-stat	0.000	5.397	1.188
Mean of Dep. Variable	0.092	0.092	0.092

This table reports two-stage least squares (2SLS) estimates of model (1.1) using the off-balance-sheet component of the Berger and Bowman (2009) Liquidity Creation measure (divided by total assets) as dependent variable, and the US sample with quarterly frequency as in Table 4 - where the instrument is the Leary and Roberts (2014) lagged peer bank average equity return shock. All coefficients are scaled by the corresponding variable's standard deviation. t-statistics (in parentheses) are robust to heteroskedasticity and within bank dependence. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific (size, capital ratio, ROA, deposit share and provisions) are all defined in Appendix Table 1.4. Peer banks' average characteristics comprise the same set of bank-specific controls but are computed as the average across all banks within a certain peer group, excluding bank i's observation. All control variables are lagged by one period. First-Stage F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic testing for weak instruments. Statistical significance at the 10%, 5% and 1% levels is denoted by *, ** and ***, respectively.

Appendix Table 1.10: Peer effects in liquidity mismatch decisions and default risk – NSFR

	$\ln\text{Zscore}_{3y}$			$\ln\text{Zscore}_{5y}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Effect:	-0.570**	-0.581**	-0.659**	-0.517***	-0.432*	-0.348
NSFRii - $\widehat{\beta^{NSFRi}_{j,t}}$	(-2.487)	(-2.109)	(-2.351)	(-2.679)	(-1.838)	(-1.495)
Peer Group Size	10	20	30	10	20	30
No. Observations	10,328	11,904	12,390	7,869	9,100	9,411
No. Banks	1,351	1,426	1,463	1,125	1,196	1,227
Adj. R-squared	0.476	0.476	0.476	0.623	0.622	0.622
Bank characteristics	Y	Y	Y	Y	Y	Y
Country controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Variable	3.687	3.693	3.700	3.357	3.363	3.361

This table reports coefficient estimates of model (1.5) using ln(Z-Score) as dependent variable. Z-score is defined as the sum of equity capital over total assets (E/A) and return on assets (ROA), divided by the 3 or 5-year rolling standard deviation of ROA. The peer effects in liquidity mismatch decisions are estimated with model (1.3) using NSFRi (inverse of the Net Stable Funding Ratio) as dependent variable ($\widehat{\beta^{NSFRi}_{j,t}}$), where the relationship between the liquidity of bank i and the liquidity of its peers is allowed to vary across countries and over time. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific characteristics include bank size, deposit share, NPL provisions, liquid assets, non-interest income revenue share and cost-to-income ratio, while country-level controls comprise GDP per capita, GDP growth volatility, local market concentration and prudential regulation intensity. All controls are lagged and defined in Tables 1 and Appendix Table 1.3. Robust standard errors clustered at the bank level are in parentheses. *, ** and *** designate that the test statistic is significant at the 10%, 5% and 1% levels.

Appendix Table 1.11: Peer effects in liquidity mismatch decisions and systemic risk – NSFR

	MES			SRISK		
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Effect:	1.890**	2.130**	2.043**	10.462**	9.889**	8.495*
NSFRi - $\widehat{\beta_{j,t}^{NSFRi}}$	(2.222)	(2.153)	(2.317)	(2.092)	(2.334)	(1.805)
Peer Group Size	10	20	30	10	20	30
No. Observations	1,783	2,197	2,374	1,783	2,197	2,374
No. Banks	244	273	290	244	273	290
Adj. R-squared	0.712	0.690	0.693	0.806	0.802	0.803
Bank characteristics	Y	Y	Y	Y	Y	Y
Country controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Variable	2.544	2.498	2.423	3.835	3.360	3.172

This table reports coefficient estimates of model (1.5) using the Marginal Expected Shortfall (MES) and Systemic Capital Shortfall (S-RISK) as dependent variables. MES is defined as bank i's expected equity loss (in %) in year t conditional on the market experiencing one of its 5% lowest returns in that given year. SRISK corresponds to the expected bank i's capital shortage (in billion USD) during a period of system distress and severe market decline. The peer effects in liquidity mismatch decisions are estimated with model (1.3) using the NSFRi (inverse of the Net Stable Funding ratio) as dependent variable ($\widehat{\beta_{j,t}^{NSFRi}}$), where the relationship between the liquidity of bank i and the liquidity of its peers is allowed to vary across countries and over time. Appendix Table 1.1 presents the weights given to the different balance-sheet items when computing this measure. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific characteristics include bank size, capital ratio, ROA, deposit share, NPL provisions, liquid assets, non-interest income revenue share and cost-to-income ratio, while country-level controls comprise GDP per capita, GDP growth volatility, local market concentration and prudential regulation intensity. All controls are lagged and defined in Tables 1 and Appendix Table 1.3. Robust standard errors clustered at the bank level are in parentheses. *, ** and *** designate that the test statistic is significant at the 10%, 5% and 1% levels.

Appendix Table 1.12: Peer effects and systemic risk – US sample and CoVaR

	CoVaR					
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Effect:	1.186***	6.590***	12.181***			
Liq. Creation ("catfat") - $\beta_{j,t}^{\widehat{LC_{catfat}}}$	(3.121)	(3.356)	(5.226)			
Peer Effect:				1.131***	3.216**	6.304***
Liq. Creation ("catnonfat") - $\beta_{j,t}^{\widehat{LC_{catnonfat}}}$				(2.774)	(2.411)	(3.763)
Peer Group Size	10	20	30	10	20	30
No. Observations	14,221	14,221	14,221	14,221	14,221	14,221
No. Banks	501	501	501	501	501	501
Adjusted R-squared	0.863	0.863	0.863	0.907	0.907	0.907
Bank characteristics	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y

This table reports coefficient estimates of model (1.5) using the conditional value-at-risk (CoVaR) of Adrian and Brunnermeier (2016) calculated at the 95% levels as dependent variable, and the quarterly US sample of listed banks as in Table 4. The peer effects in liquidity mismatch decisions are estimated with model (1.3) using the Berger and Bouwman (2009) "catfat" and "catnonfat" Liquidity Creation measures as dependent variables (i.e., on-and-off-balance-sheet and on-balance-sheet liquidity creation divided by total assets, respectively), where the relationship between the liquidity of bank i and the liquidity of its peers is allowed to vary across countries and over time. Peer groups are defined as commercial banks operating in the same country in the same year grouped into a maximum of 10, 20 or 30 banks according to their size (total assets). Bank-specific characteristics include log total assets, capital ratio, ROA, deposit share and NPL provisions. All controls are lagged by one quarter and defined in Tables 1 and Appendix Table 1.3. Robust standard errors clustered at the bank level are in parentheses. *, ** and *** designate that the test statistic is significant at the 10%, 5% and 1% levels.

Chapter 2

Sharing the Pain? Credit Supply and Real Effects of Bank Bail-ins

2.1 Introduction

The recent financial crisis highlighted the pressing need for a robust and consistent mechanism to resolve banks in distress. Absent a viable alternative to bankruptcy that could lead to

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contagion and a credit crunch, policymakers around the world opted to bail-out financial intermediaries using public funding. In Europe, for instance, taxpayers have covered more than two-thirds of the cost of recapitalizing and resolving banks (Philippon and Salord, 2017). These interventions were often accompanied by significant government losses and austerity programs associated with political frictions and distributional problems. To counter this pervasive issue, most developed economies have recently introduced formal bail-in regimes that involve the participation of bank creditors in bearing the costs of restoring a distressed bank and include severe restrictions on taxpayer support.

An effective bank resolution framework should minimize the trade-off between imposing market discipline and reducing the external costs of a potential bank failure (Beck, 2011). In fact, previous evidence has shown both the detrimental impact of public guarantees on bank risk-taking (e.g., Dam and Koetter, 2012; Gropp, Hakenes, and Schnabel, 2011) and the negative effects of bank failures on real outcomes (e.g., Ashcraft, 2005; Bernanke, 1983; Calomiris and Mason, 2003). The recently introduced bail-in regime reduces moral-hazard due to creditors' expectation of being bailed-in in case of distress (Schäfer, Schnabel, and Weder, 2016), and should in principle also minimize the negative economic effects since the healthy part of the bank can continue functioning. However, there is little to no empirical evidence on the effects this new resolution mechanism may have on credit provision or the real economy. Our study fills this gap in the literature by examining the credit supply and real effects of bank bail-ins using a unique dataset combining firm-bank matched data on credit exposures and interest rates from the Portuguese credit register with balance-sheet information for the firms and their lenders.

In detail, we exploit the unexpected collapse of a major bank in Portugal (Banco Espírito Santo - BES) in August 2014 that was coined "one of Europe's biggest financial failures" (FT, 2014). The institution was resolved through a bail-in and split into a "good" and "bad" bank, protecting taxpayers and depositors but leaving shareholders and junior bondholders holding the toxic assets. The costs of this intervention fell not only on the bank's creditors, but also indirectly on other resident banks that had to provide additional funding to the Bank Resolution Fund. Importantly, the bank failure was unrelated to fundamental risks in a generalized group of borrowers or in the Portuguese banking sector. Instead, the collapse was due to large risky exposures to a limited number of firms that were also owned by the Espírito Santo family (Economist, 2014). These reflected the "practice of management acts seriously detrimental" to the bank and non-compliance with determinations issued by the Portuguese central bank "prohibiting an increase in its exposure to other entities of the Group" (Banco de Portugal, 2014a). From an identification perspective, exploiting this (exogenous) shock is therefore particularly attractive since the bank's failure was purely idiosyncratic.

We start the analysis by examining over 115,000 bank-firm lending relationships and running a within-firm difference-in-differences specification comparing changes in credit supply to the same borrower across banks exposed differently to the bail-in i.e., the bailed-in bank itself, other banks that provided ad-hoc financing to the Resolution Fund, and banks that were exempt from making additional contributions. By exploiting the widespread presence of Portuguese firms with multiple bank relationships, this approach allows us to control for changes in observable and unobservable firm characteristics such as credit demand, quality, and risk (Khwaja and Mian, 2008). In this regard, we show that the supply of credit from banks more exposed to the bail-in declined significantly to existing borrowers as a result of the shock. In detail, comparing lending to the same firm by banks one standard deviation apart in terms of exposure to the bail-in, we find that more exposed banks reduced total credit and granted credit lines by 3.0 and 5.7 percent more than banks exposed less. This credit supply contraction was concentrated in firms that had the bailed-in bank as their main lender prior to the shock and was less pronounced for SMEs as well as firms with lower profitability and non-performing loans in the pre-period.[1]

A fundamental follow-up question is whether more exposed firms could compensate the credit supply tightening by accessing funds from other banks less affected by the shock, and if

[1]We confirm our findings when using the complete sample of borrowing firms in Portugal (i.e., including single-bank-relationship firms) in a model that replaces firm fixed-effects with location-size-sector fixed-effects as in De Jonghe, Dewachter, Mulier, Ongena, and Schepens (2018).

there were any real effects associated with the intervention.[2] Importantly, following Abowd, Kramarz, and Margolis (1999) and Cingano, Manaresi, and Sette (2016), we are also able to control for loan demand when looking at the cross-section of firms by including in the regressions the vector of estimated firm-level fixed effects from the within-firm specification. We find at the cross-sectional level that firms more exposed to the bail-in did not suffer a reduction of overall credit after the intervention when compared to firms exposed less. However, when isolating granted credit lines from total committed credit and focusing on firms with credit lines at multiple banks, we show that SMEs more exposed to the resolution were subject to a binding contraction in quantity of funds available through lines of credit, an essential component for corporate liquidity management (Jiménez, Lopez, and Saurina, 2009; Sufi, 2009). Specifically, a one standard deviation increase in firm exposure to the bail-in is associated with a 2.2 percent binding decrease in granted credit lines to SMEs.

Our results show that the resolution also came at the cost of moderately higher interest rates for more exposed firms. In detail, a one standard deviation increase in firm exposure to the shock is associated with a relative increase of 20 basis points in the interest rate charged on credit lines for the average firm. We also observe a relative increase in interest rates on new credit operations (though only for large firms more exposed to the shock), as well as a moderate relative decrease in the maturity of new credit and increase in the share of collateralized credit after the shock across both firm types.

Finally, regarding the effect of the bank failure and subsequent bail-in on real sector outcomes, we find evidence of a negative adjustment of investment and employment policies at SMEs borrowing from more exposed banks prior to the resolution. This effect is economically significant: a one standard deviation increase in firm exposure to the shock leads to a relative drop in investment and employment of up to 2.0 and 1.5 percent, respectively. These dampening effects of the bank resolution are driven by a response to increased liquidity

[2] This issue is particularly important in the context of SMEs which usually find it difficult to substitute credit from other sources because they are more opaque and thus mainly rely on existing banking relationships. This is still a source of great concern among academics, regulators and policy-makers, particularly in Europe (Giovannini, Mayer, Micossi, Di Noia, Onado, Pagano, and Polo, 2015)

risk by firms with lower ex-ante internal liquidity. Consistent with the argument that the option for firms to access liquidity from credit lines should be more valuable when internal liquidity is scarce (e.g., Campello, Giambona, Graham, and Harvey, 2011), we find that the negative real effects are concentrated on illiquid SMEs more exposed to the resolution that responded to the funding shock by increasing cash holdings while decreasing investment and employment.[3] Instead, in line with precautionary cash savings being important in times of dislocation in markets for external finance (e.g., Duchin, Ozbas, and Sensoy, 2010), more exposed SMEs with high liquidity before the bail-in were able to use their available internal cash holdings to compensate for the binding contraction in granted credit lines and thus maintain employment and investment.[4]

This paper contributes to the literature examining how distressed banks should be resolved. Kahn and Winton (2004) suggest that a "good-bank-bad-bank" split may be beneficial as it reduces risk-shifting incentives in the healthy bank and increases its incentive to screen and monitor the "good" loans. More recent work, however, has mostly focused in describing the potential benefits and costs of the different bank resolution mechanisms (e.g., Avgouleas and Goodhart, 2015; Conlon and Cotter, 2014; Dewatripont, 2014; Philippon and Salord, 2017) and examining the interaction between bail-ins and bail-outs from a theoretical perspective (e.g., Bernard, Capponi, and Stiglitz, 2017; Colliard and Gromb, 2017; Keister and Mitkov, 2017; Klimek, Poledna, Farmer, and Thurner, 2015; Walther and White, 2017). Our paper contributes to this literature by assessing the effects of a bank bail-in on credit supply and real sector outcomes using detailed bank-, firm- and loan-level data. To the best of our knowledge, this is the first empirical study examining this issue.

[3]This result is not explained by differences in anticipated growth opportunities across SMEs with low and high levels of internal liquidity prior to the bank resolution.

[4]In a separate but related exercise, we gauge whether the bail-out of four Portuguese banks in 2012 resulted in similar negative effects. We find no significant differences between borrowers of bailed-out and non-bailed-out banks in terms of credit supply, investment or employment. This points to rather sharp differences between different bank resolution policies, although we caution that the macroeconomic situation was considerably different during these two episodes and that the public intervention in 2012 was more systemic in nature.

We also contribute to the literature analyzing bank failures and the associated negative real effects. Bernanke (1983) and Calomiris and Mason (2003) highlight the economic repercussions of bank failures in the 1920s and 1930s, while Ashcraft (2005) links the decrease in lending following the closure of a large (solvent) affiliate in a regional bank holding company in Texas in the 1990s to a decline in local GDP. Slovin, Sushka, and Polonchek (1993) show that firms that were the main customers of Continental Illinois in the US saw their share prices negatively affected by its bankruptcy. Our paper shows that even when a bank partly continues operating because of a more efficient bank resolution mechanism, there are still negative repercussions for certain borrower groups.

Finally, our paper also contributes to the empirical corporate finance literature on firm's liquidity management and its importance for the transmission of credit supply shocks to the real economy. Under the precautionary demand for cash theory, firms hold cash as a buffer as protection against adverse cash flow shocks. This is particularly valuable for firms that are financially constrained (Almeida, Campello, and Weisbach, 2004), and following a credit crunch (Duchin, Ozbas, and Sensoy, 2010). Directly relevant for our work, Berg (2018) shows that while liquid SMEs are able to absorb credit supply shocks by using existing cash buffers, their illiquid counterparts increase cash holdings when a loan application is rejected, cutting non-cash assets by more than the requested loan amount, and thus investment and employment. While Berg (2018) uses discontinuities in credit scores comparing accepted and rejected loan applicants at a single German bank, we use an exogenous bank shock for identification and the entire set of banks operating in Portugal.[5]

[5]This paper is also part of an expanding literature using loan-level data to explore the effect of regulatory, liquidity and solvency shocks on credit supply and real outcomes. Khwaja and Mian (2008) and Schnabl (2012) gauge the effect of exogenous liquidity shocks on banks' lending behavior in Pakistan and Peru, respectively. Jiménez, Ongena, Peydró, and Saurina (2012, 2014b) use Spanish credit register data to explore the effect of monetary policy on credit supply and banks' risk-taking. Cingano, Manaresi, and Sette (2016) analyze the transmission of bank balance sheet shocks to credit and its effects on investment and employment in Italy. Ivashina and Scharfstein (2010) and Chodorow-Reich (2014) examine the effects of the crisis in the US. Iyer, Peydró, Da-Rocha-Lopes, and Schoar (2014), Alves, Bonfim, and Soares (2016) and Blattner, Farinha, and Rebelo (2018) use the same credit register data from Portugal as we do to investigate the effect of the liquidity freeze in European interbank markets on credit supply, the role of the ECB as lender of last resort in avoiding the collapse of the Portuguese financial system during the European sovereign debt crisis, and the impact of bank capital adequacy on productivity, respectively.

2.2 Background

After a rapid series of events including the disclosure of hefty losses of €3.6 billion in the first-half of 2014 arising from exposures to the parent family-controlled group of companies, the Portuguese central bank decided to apply a resolution measure to Banco Espírito Santo (BES) on August 3, 2014. The bank was classified as a significant credit institution by the European Central Bank (World Bank, 2016) and was the third largest bank in Portugal with a market share of 19 percent of credit granted to non-financial corporations (Banco de Portugal, 2014a). The scale of the losses came as a surprise to the Bank of Portugal, which suggested that these "reflected the practice of management acts seriously detrimental" and "noncompliance with the determinations issued prohibiting an increase in its exposure to other entities of the Group" (Banco de Portugal, 2014a).

The resolution of the bank involved the transfer of sound activities and assets to a bridge bank or "good bank" designated as Novo Banco (New Bank). In contrast, shareholders and junior bondholders were left with the toxic assets that remained in a "bad bank" which is in the process of liquidation.[6] The €4.9 billion of capital of the newly-created bank was fully provided by Portugal's Bank Resolution Fund established in 2012 and financed by contributions of all the country's lenders.[7] Since the Fund did not yet have sufficient enough resources to fully finance such a large operation, it had to take a loan from a group of eight of its (largest) member banks (€0.7 billion) and another from the Portuguese government (€3.9 billion). The government ensured the deal would have no direct or indirect costs for taxpayers since the loan was made to the Bank Resolution Fund (i.e., not to the distressed

[6]The firms part of the Espírito Santo Group that drove the collapse of the bank are not part of our estimations since (i) these firms were mostly based abroad and our dataset only captures firms headquartered in Portugal, and (ii) their credit claims were transferred to the "bad bank" and therefore do not appear in the post-shock period even if a firm is based in Portugal – see the detailed list of assets transferred to "bad bank" in subparagraph (a) of Annex 2 in Banco de Portugal (2014a).

[7]The CET1 ratio of the "good bank" immediately after the resolution was 10.3 percent, above the regulatory minimum (Novo Banco, 2014).

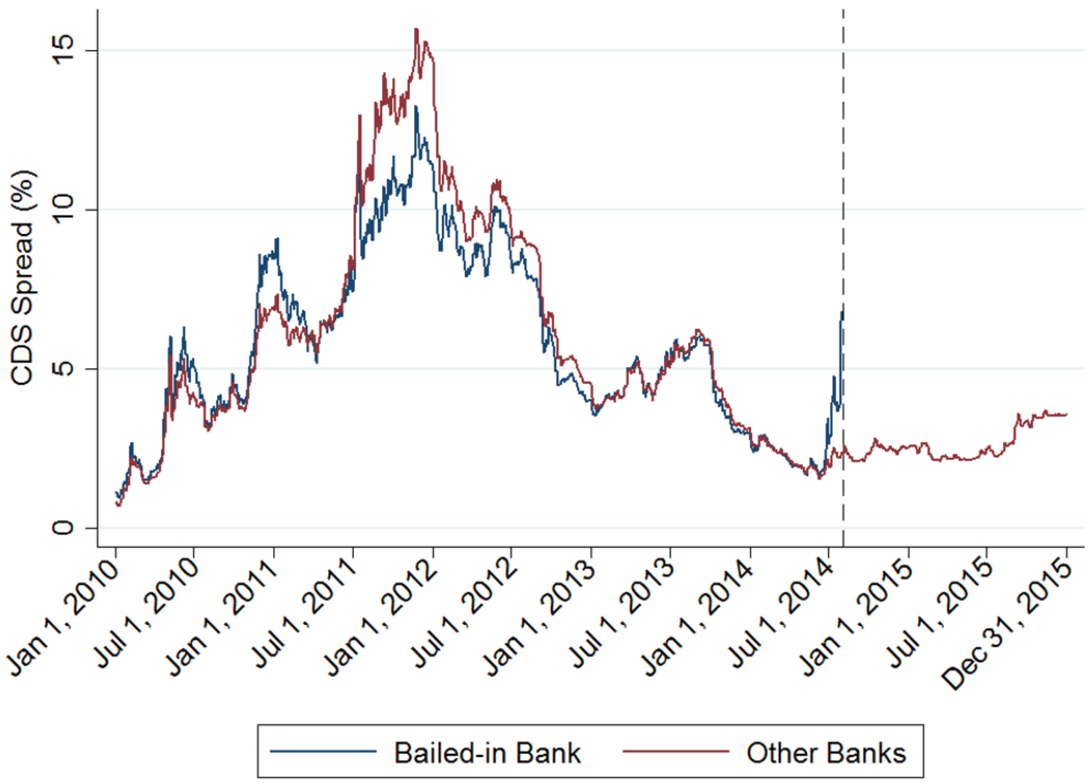
bank), and given that the country lenders responsible for the Fund and who bear the risks will have several years to recoup the shortfall with their ordinary contributions (FT, 2017).[8]

Figure 2.1 shows the unexpected and idiosyncratic nature of the bank failure. CDS spreads of the bailed-in bank moved in line with the rest of the sector until late June 2014 when the degree of exposures to the Group's entities owned by the same family started to be revealed. Within a month, the spreads moved from less than 2 percent to almost 7 percent. The event came after a long period of increasing stability in the banking sector, with CDS spreads for Portuguese banks having declined from its crisis peak of around 16 percent in late 2011.[9] The figure also shows the limited contagion from the bailed-in bank to the remainder of the banking system, with the average CDS spread for all other resident banks considered significant credit institutions by the ECB increasing only slightly in the weeks leading up to the intervention and remaining below 3.5 percent until December 2015. This is consistent with the simulation results of Hüser, Halaj, Kok, Perales, and van der Kraaij (2018) that, using granular data on the securities cross-holdings among the largest euro area banking groups, show that bail-ins lead to limited spillovers due to low levels of securities cross-holdings in the interbank network and no direct contagion to creditor banks. Nevertheless, in our analysis we still take into account the exposure of other banks to the bail-in, even if small, through the institution-specific amount of financing to the ad-hoc loan granted to the Resolution Fund.

[8]The Portuguese central bank decided to move even further towards a bail-in type of intervention with a re-resolution in the last days of 2015 - 16 months after the original intervention. In detail, a limited number of bonds were transferred to the "bad bank", imposing losses on almost €2 billion of senior bondholders (Banco de Portugal, 2015; FT, 2016). In October 2017, Lone Star Funds (a US private-equity fund) acquired 75 percent of the "good bank" in return for a capital injection of €1 billion, with the remaining 25 percent held by the Bank Resolution Fund (Banco de Portugal, 2017). Given that we only have loan and firm-level data available until 2015, our analysis does not consider these two shocks and is instead solely focused on the effects of the original resolution in August 2014.

[9]Following demanding requirements imposed by the European Banking Authority and the Bank of Portugal, the Core Tier 1 ratio in the Portuguese banking sector reached 12.3 percent at the end of 2013 (Banco de Portugal, 2014b). At the country-level, by the end of EC/ECB/IMF Economic Adjustment Program in June 2014, Portugal was growing 0.3 percent faster than the EU, excluding Germany (Reis, 2015).

Figure 2.1: Evolution of bank CDS spreads over time



The figure plots daily 5-year CDS spreads on senior unsecured debt between January 1, 2010 and December 31, 2015. The resolution occurred in August 2014 (dashed vertical line). CDS spreads for the group "Other Banks" are computed as the equal-weighted average across banks headquartered in Portugal with available information (Caixa Geral de Depositos, Banco BPI, Banco Millennium BCP). The banks considered correspond to the four significant institutions (SIs) operating in Portugal as defined by the ECB. Source: Thomson Reuters Datastream.

In short, even when being conservative and considering this resolution a "hybrid of bail-in and bail-out" (Economist, 2014), this intervention differs markedly from the bail-outs of most distressed banks during the financial crisis as all losses were ultimately imposed on shareholders and (junior and later senior) bondholders. This resolution was therefore also distinctly different from the bail-out and recapitalization of several large Portuguese banks in 2012, a difference we also explore in our empirical analysis. Finally, while this resolution occurred before transposition of the EU Bank Recovery and Resolution Directive (BRRD) into national legislation, the Portuguese resolution regime introduced in 2012 and then in force was already, in substance, very similar to the final European directive (World Bank, 2016). Although this new framework hypothetically lets banks fail without resorting to taxpayer funding (Avgouleas and Goodhart, 2015), it also allows for extraordinary public support under specific conditions (Schoenmaker, 2017).[10] As a result, this shock provides a unique laboratory to study the potential effects of future (similar) interventions.

2.3 Identification Strategy

We investigate the credit supply and real effects of a bank bail-in in two steps. First, we assess whether the resolution induced significant changes in the supply of credit to firms that were differently exposed to the bail-in (within-firm analysis). Second, assuming that the tightening of credit did occur, we investigate whether these firms were able to substitute funding from other (less exposed) banks, if they were able to maintain their average interest rates on credit, as well as the consequences of this shock for firm real outcomes such as investment and employment (cross-sectional analysis). The first part of the analysis uses firm-bank matched data to exploit variation within firms that have more than one lending relationship, while the second makes use of variation across firms with different pre-shock exposures to the bail-in.

Within-Firm Analysis. The main challenge of our empirical analysis is to identify the causal impact of bail-ins on loan supply, price conditions and real outcomes. In fact, this shock may be correlated with underlying changes in the overall economic situation that may affect both credit provision and firms' loan demand and risk. To address this identification problem, we exploit an exogenous shock in August 2014 corresponding to an unexpected bank failure and subsequent resolution as discussed above, and use a difference-in-differences approach to compare lending before and after the bank collapse across banks more and less exposed to the resolution.

In detail, following the novel approach of Khwaja and Mian (2008), we exploit our panel of matched bank-firm data and account for unobserved heterogeneity in firms' loan demand,

[10]The EU and the US strengthened their bank resolution regimes and introduced bail-in powers via the Bank Recovery and Resolution Directive (BRRD) and the Dodd-Frank Act, respectively. Despite many similarities between the EU and US resolution schemes, there are still some important differences such the lack of a restructuring option in the US (Philippon and Salord, 2017).

quality and risk by saturating our model with firm fixed effects. As a result, our identification comes entirely from firms that were borrowing from at least two different banks before and after the resolution program. This strategy isolates the causal impact of the bail-in shock on the change in credit supply by comparing the within-firm variation in the change in lending from banks differently exposed by the intervention. The baseline specification is defined as:

$$\Delta log(Credit)_{bi} = \beta(BankExposure_b) + \delta' X_b + \alpha_i + \varepsilon_{bi} \tag{2.1}$$

where the main dependent variable $\Delta log(Credit)_{bi}$ is the log change in granted credit from bank b to firm i from the pre to the post-period.[11] We also consider the change in granted credit credit lines from bank b to firm i from the pre to the post-period as an alternative dependent variable. In this case, our identification comes from the sub-set of firms (35 percent) with credit lines from at least two different banks before and after the resolution program.[12]

As in Khwaja and Mian (2008), the quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. This adjustment has the advantage that our standard errors are robust to auto-correlation (Bertrand, Duflo, and Mullainathan, 2004). The main independent variable, $BankExposure_b$ is the percentage of assets of each bank exposed to the bail-in: the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc €0.7 billion loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. We do not include in this measure the ordinary contributions to the Fund that each bank made in 2013 as these were already priced

[11] Since we want to ensure that changes in credit are not driven by sudden draw-downs of credit lines by certain firms, we consider throughout the paper the total amount of committed credit i.e., the total amount of credit that is available to a borrower, not only the portion that was taken up.

[12] While our identification strategy relies on within-firm variation in credit supply, we test the robustness of our findings by using the sample of all borrowers (i.e., including firms with only one bank relationship) and replacing bank-fixed effects with location-size-sector fixed-effects to partially control for demand side factors (De Jonghe, Dewachter, Mulier, Ongena, and Schepens, 2018).

in before the resolution.[13] α_i are firm fixed effects that capture firm-specific determinants of credit flows and can be interpreted as a measure of credit demand (e.g., Cingano, Manaresi, and Sette, 2016).

X_b refers to a set of bank-level controls measured in the pre-period, including bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). These controls are particularly relevant in our setting since bank-specific exposures to the bail-in are not randomly assigned but a function of bank characteristics (e.g., the contribution to the resolution fund is determined by each bank's amount of liabilities), which may be correlated with changes in their willingness to lend. Finally, since the shock is bank-specific, changes in the credit granted from the same bank may be correlated. As a result, we use robust standard errors clustered at the bank level in all within-firm regressions.

Cross-Sectional Analysis. Although the above specification allows us to examine whether there was indeed a credit contraction and which type of firms were more likely to be affected by the shock, it is not appropriate to assess aggregate effects. This is because the within-firm analysis is not able to capture credit flows from new lending relationships and also ignores all terminated lending relationships between the pre and post-shock period. Given the importance of the extensive margin for credit adjustment, we also estimate the related between-firm (cross-sectional) effect of firm exposure to the shock as:

$$\Delta log(Y)_i = \beta(FirmExposure_i) + \tau' F_i + \delta' \bar{X}_i + \hat{\alpha}_i + \varepsilon_i \tag{2.2}$$

where $\Delta log(Y)_i$ is the log change in total bank credit or in granted credit lines between 2013:Q4 to 2015:Q4 from all banks to firm i. We use the same model to examine the effects on other credit conditions and analyze potential real effects.

[13] These bank-specific figures were manually collected from each of the banks publicly-available 2014 Annual Reports. The percentage of assets that was effectively bailed-in for the resolved bank amounts to 6.8 percent, while the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks ranges from 0.04 to 0.37 percent of assets.

$FirmExposure_i$ is the exposure of each firm to the bail-in computed as the weighted average of *Bank Exposure* across all banks lending to a firm, using as weights the pre-period share of total credit of each bank. F_i are firm characteristics including firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital (equity to total assets) and firm liquidity (current assets to current liabilities) - all measured in 2013:Q4. We also include industry and district fixed effects in the model. Bank controls $\bar{X}_i$ include the same variables as in specification (2.1) but are averaged at the firm-level according to the share of total credit granted to the firm by each bank prior to the shock.

Finally, given that in the between-firm model (2.2) the firm-specific demand shock α_i cannot be absorbed, a OLS estimate of β would be biased if $FirmExposure_i$ is correlated with credit demand (Cingano, Manaresi, and Sette, 2016; Jiménez, Mian, Peydró, and Saurina, 2014a). To control for loan demand when looking at the cross-section of firms, we follow the method developed by Abowd, Kramarz, and Margolis (1999) and recently applied by Bonaccorsi di Patti and Sette (2016) and Cingano, Manaresi, and Sette (2016), and include in (2.2) the vector of firm-level fixed effects $\hat{\alpha}_i$ estimated from the within-firm specification (2.1).[14] Standard errors are clustered at the main bank and industry levels, where the main bank is the institution that a certain firm has the highest percentage of borrowing with before the shock.

2.4 Data and Descriptive Statistics

The dataset we use throughout this study merges four unique databases held and managed by the Bank of Portugal: (i) Central Credit Register (Central de Responsabilidades de Crédito); (ii) Individual Information on Interest Rates (Informação Individual de Taxas de Juro); (iii) Central Balance Sheet Database (Central de Balanços); and (iv) Bank Supervisory Database.

[14] Jiménez, Mian, Peydró, and Saurina (2014a) propose an alternative method to correct for the bias that arises if the firm exposure to the shock is correlated with credit demand in the firm-level regressions. They use a numerical correction exploiting the difference between OLS and FE estimates of β in the Khwaja and Mian (2008) within-firm regression. Cingano, Manaresi, and Sette (2016) show that the approach of Jiménez, Mian, Peydró, and Saurina (2014a) and the one we use in this paper are equivalent.

The Central Credit Register provides confidential information on all credit exposures above 50 euros in Portugal.[15] It covers loans granted to non-financial companies by all banks operating in the country as reporting to the central bank is mandatory. Besides recording the outstanding debt of every firm with each bank at the end of every quarter, each claim also specifies the amount that each borrower owes the bank in the short and long-term, and the amount that is past due. The database also provides information on other loan characteristics e.g., if the loan is an off-balance sheet item such as the undrawn amount of a credit line or credit card.

The database on Individual Information on Interest Rates reports matched firm-bank interest rate information on new loans. While only banks with an annual volume of new corporate loans of more than €50 million were required to report between June 2012 and December 2014, this requirement was extended to all resident banks in January 2015. For consistency, we restrict the analysis to banks that reported interest rate information before and after this reporting change. Besides interest rates, we have loan-level information on the amount, maturity, date of origination, if the loan is collateralized or not, and loan type.

The Central Balance Sheet Database provides detailed financial information with an annual frequency for virtually all Portuguese firms e.g., total assets, year of incorporation, equity, net income, number of employees, total debt, cash holdings. Finally, we also match the above datasets with bank balance-sheet data from the Bank Supervisory Database e.g., bank size, profits, capital, liquidity and non-performing loans. Given the very low threshold to capture credit exposures in the credit register, the zero minimum loan size of the interest rate database and the compulsory reporting of balance sheet information by all firms and banks operating in Portugal, the combined dataset we use is arguably one of the most comprehensive loan-bank-firm matched databases available worldwide.

[15] This threshold alleviates any concerns on unobserved changes in bank credit to SMEs. In addition, it has significant advantages when studying credit supply restrictions of smaller firms when compared to other widely-used datasets e.g., US Survey of Small Business Finances or the LPC Dealscan which have incomplete coverage of entrepreneurial firms.

Table 2.1 presents firm-level descriptive statistics computed using the bank-firm matched sample. Specifically, we present the mean, median and standard deviation of the dependent variables, firm and bank characteristics across the 40,927 firms in our sample that have more than one lending relationship. On average, firms' total credit and granted credit lines increased by 1.1 and 0.3 percent from the pre-shock (2013:Q4-2014:Q2) to the post-shock period (2014:Q3-2015:Q3), respectively. Over the same period, firm investment shrank on average by 2.6 percent, employment increased by 3.2 percent, while cash holdings increased by 10.8 percent. Finally, there was an average decrease in interest rates from the pre- to the post-resolution period of 88 and 69 basis points on total credit and credit lines, respectively, an increase in loan maturity of 1.9 months, and a decrease in the share of collateralized credit of 2.9 percentage points.

Turning to firm characteristics, the average pre-failure firm exposure to the bail-in was 0.008, with a standard deviation of 0.013. Firms in our sample have on average 4 lending relationships and 32 percent started a new lending relationship within a year after the resolution. SMEs constitute 98 percent of all firms.

Before the shock, the average firm had €0.75 million in assets, was operating for 13.6 years, had a capital ratio of 26 percent, suffered losses of 0.6 percent of total assets and had a current ratio of 2.2. Finally, we present bank characteristics, which are averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank. These are also measured in 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans).

2.5 Results

In this section, we first present results examining the effect of the bank failure and subsequent resolution through a bail-in on credit supply and firms' borrowing conditions. We then trace these effects to real sector outcomes and examine the role of firms' internal liquidity position in explaining our findings.

Table 2.1: Summary statistics

	N	Mean	Median	SD
Dependent Variables:				
Δ *log* Total Credit	40,927	0.011	-0.031	0.485
Δ *log* Granted Credit Lines	14,320	0.003	0.008	0.570
Δ *log* Investment	40,927	-0.026	-0.054	0.978
Δ *log* No. Employees	40,927	0.032	0.000	0.433
Δ *log* Cash Holdings	40,927	0.108	0.117	1.526
Δ Interest Rates - Total Credit	31,472	-0.875	-0.848	4.265
Δ Interest Rates - Credit Lines	12,429	-0.691	-0.578	3.321
Δ Maturity	31,472	1.912	0.000	27.35
Δ Collateral	31,472	-0.029	0.000	0.320
Firm Characteristics:				
Firm Exposure	40,927	0.008	0.002	0.013
No. Bank Relationships	40,927	4.106	3.000	2.280
New Lending Relationship	40,927	0.323	0.000	0.467
SME	40,927	0.983	1.000	0.129
Firm Size	40,927	13.53	13.40	1.516
Firm Age	40,927	2.679	2.773	0.752
Firm ROA	40,927	-0.006	0.008	0.143
Firm Capital Ratio	40,927	0.261	0.286	0.424
Firm Current Ratio	40,927	2.191	1.414	3.555
Bank Characteristics:				
Bank Size	40,927	23.90	24.36	1.349
Bank ROA	40,927	-0.010	-0.009	0.008
Bank Capital Ratio	40,927	0.054	0.053	0.021
Bank Liquidity Ratio	40,927	0.012	0.011	0.005
Bank NPLs	40,927	0.064	0.065	0.020

The table presents the relevant firm-level summary statistics computed using the bank-firm matched sample. The firm-specific change in the log level of total (committed) credit and the change in the log level of granted credit lines are constructed by collapsing (time-averaging) the quarterly data for each credit exposure into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. Log change in investment (i.e., tangible assets), no. employees, and cash holdings are the firm-specific changes in the log level of the each variable between 2013:Q4 and 2015:Q4. Change in interest rates on new credit operations and credit lines (in percentage points), maturity (in months) and share of collateralized credit (in percentage points) refer to the firm-level change in the loan-amount-weighted value of the respective variable. Since the interest rate dataset only captures new credit operations (rather than outstanding amounts), we consider all new credit operations for each firm between 2013:M12 and 2014:M7 (pre-period) and 2014:M9 and 2015:M9 (post period). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. New lending relationship is a dummy variable taking the value of 1 if the firm has a new loan after the shock (2014:Q3-2015:Q3) with a bank that it had no loan before, and 0 otherwise. Firm size categories are defined according to the EU Recommendation 2003/361. Firm characteristics include size (log of total assets), age (ln(1+age)), ROA (net income to total assets), capital ratio (equity to total assets) and current ratio (current assets to current liabilities) - all measured as at 2013:Q4. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are also measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans).

2.5.1 Bank Resolution and Credit Supply

Within-Firm Analysis. The results in Table 2.2 show a significant reduction in credit supply, including granted credit lines, from banks more exposed to the bail-in. Columns (1) and (2) present the results without and with firm fixed-effects, while column (3) adds bank-level controls measured at 2013:Q4 – bank size, ROA, capital ratio, liquidity ratio and NPLs. Column (4) differentiates the main effect of interest across SMEs and large firms. The unit of observation is the change in the log level of total committed credit between each of the 116,245 firm-bank pairs, corresponding to 40,927 firms. As in Khwaja and Mian (2008), the quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. *Bank Exposure*, the main explanatory variable, is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan granted to the Bank Resolution Fund for the 8 participating banks (as a percentage of assets), and 0 otherwise. All specifications focus on borrowers with more than one bank relationship. This ensures that any observed changes in lending are due to the bank supply shock which is orthogonal to idiosyncratic firm-level shocks such as changes in credit demand or borrowers' risk profile.

The relative credit contraction from banks more exposed to the shock is both statistically and economically significant. The coefficient of interest in column (3) indicates that a one standard deviation increase in bank exposure to the bail-in (0.020) is associated with a supply-driven decrease in credit for the average firm of 3.0 percent. Importantly, while the effect is significant across different firm size groups, the results in column (4) show that it is more than twice as strong for large firms than small and mid-sized firms – 6.3 vs. 2.9 percent, respectively.

In columns (5) to (6) of Table 2.2 we focus on firms with multiple credit lines simultaneously held at different banks. This corresponds to 14,320 out of 40,927 firms, for a total of 39,573 firm-bank relationships. In line with Ippolito, Peydró, Polo, and Sette (2016) who find that following the 2007 freeze of the European interbank market Italian banks managed liquidity

Table 2.2: Credit supply and firm size – within-firm estimates

	$\Delta logTotalCredit_{bi}$				$\Delta logCreditLines_{bi}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Bank Exposure	-0.989***	-1.143***	-1.520*		-2.723***	
	(0.311)	(0.320)	(0.824)		(0.863)	
Bank Exposure × SMEs				-1.441*		-2.659***
				(0.829)		(0.881)
Bank Exposure × Large Firms				-3.133***		-4.048***
				(0.836)		(0.866)
No. Observations	116,245	116,245	116,245	116,245	39,573	39,573
No. Firms	40,927	40,927	40,927	40,927	14,320	14,320
Adj. R^2	0.001	0.047	0.049	0.050	0.103	0.103
Bank Controls	N	N	Y	Y	Y	Y
Firm FE	N	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y
Credit Lines with ≠ Banks	N	N	N	N	Y	Y

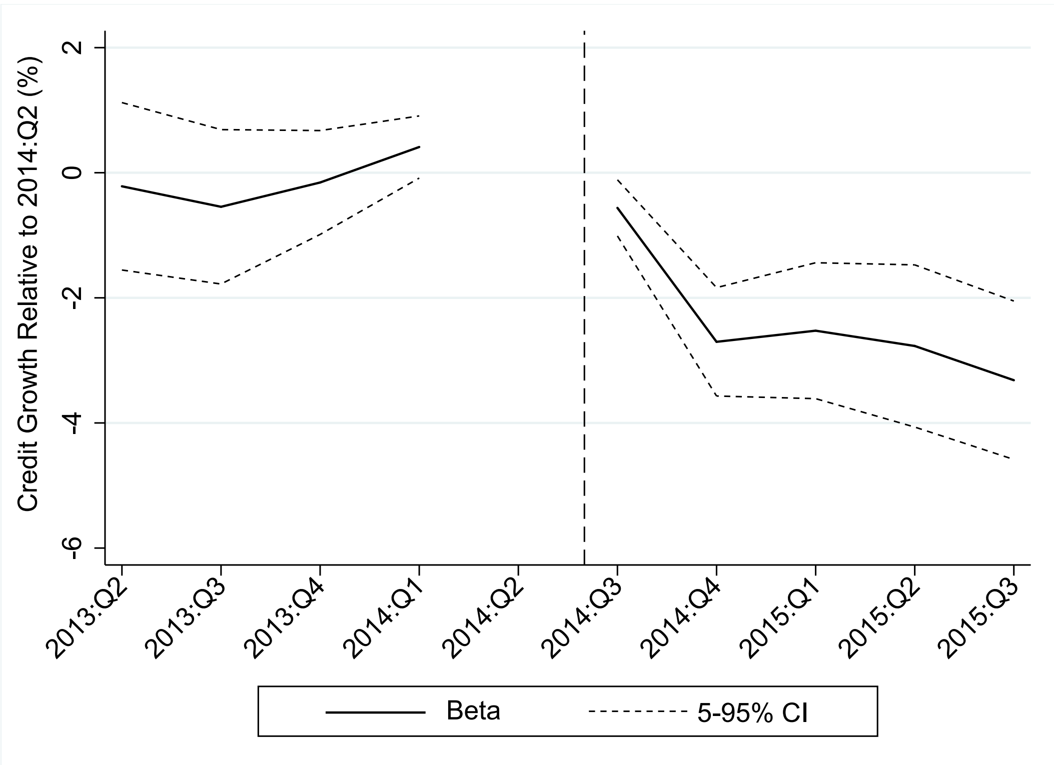
The table presents estimation results of the within-firm specification (2.1) where the dependent variables are the change in the log level of total (committed) credit and the change in granted credit lines between each firm-bank pair. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank Controls are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm size categories are defined according to the EU Recommendation 2003/361. Heteroskedasticity-consistent standard errors clustered at the bank level are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

risks by extending fewer and smaller credit lines, the coefficient estimates show that granted credit lines were the main channel through which banks more exposed to the bail-in reduced credit supply – a decrease of 5.7 percent to the average firm for a one standard deviation increase in bank exposure to the bail-in.

Identifying Assumptions. The validity of our identification strategy relies on two main assumptions. First, our quasi-experimental research design requires that in the absence of treatment (i.e., the bank failure and subsequent resolution), banks more exposed to the shock would have displayed a similar trend in terms of credit supply to that of other less exposed banks. While the parallel trends assumption cannot be tested explicitly due to the absence of a counterfactual, Figure 2.2 shows it is likely to be satisfied in our setting.

In specific, we use a modified version of the within-firm specification (2.1), regressing for each quarter the change in the log level of total committed credit between each firm-bank pair in that quarter relative to 2014:Q2 (the last period before the shock) on $BankExposure_b$ and firm fixed-effects. *Bank Exposure* is here standardized to have mean 0 and standard deviation of 1, and the dashed lines indicate the 5%–95% confidence interval using standard errors clustered at the bank level. Before the shock, there is no significant variation in credit provision across banks more or less exposed to the resolution. Starting from 2014:Q3, however, credit supply from banks more exposed to the bail-in decreased significantly and deteriorated over time.

Figure 2.2: Impact of the bail-in on credit supply at the intensive margin



The figure presents coefficient estimates of a modified version of the within-firm specification (2.1) where the dependent variable ($\Delta log(Credit)_{bi}$) is regressed on $BankExposure_b$ and firm fixed-effects. Each coefficient estimate in each quarter corresponds to a different within-firm regression, where the outcome variable is the change in the log level of total committed credit between each firm-bank pair between the respective quarter and 2014:Q2. $BankExposure_b$ is standardized to have mean 0 and SD of 1. The dashed lines indicate the 5%–95% confidence interval using standard errors clustered at the bank level.

Second, the implicit assumption behind using firm fixed-effects to control for idiosyncratic demand shocks in the Khwaja and Mian (2008) within-firm specification is that firm-specific loan demand changes proportionally across all banks lending to the firm i.e., individual firms take their multiple banks as providers of a perfectly substitutable good. In our setting, this assumption could be violated if firms reduced credit demand from more exposed banks after the shock while increasing it from other (healthier) banks.[16] However, some factors suggest the effects we observe are indeed supply driven and unlikely to be explained by within-firm changes in demand. First, as clearly stated in both its 2014 and 2015 Annual Reports, after the resolution the bailed-in bank "conducted a very strict and selective lending policy, without ceasing to support the small and medium-sized enterprises" (Novo Banco, 2014, p. 100, 115; Novo Banco, 2015, p. 87, 97). The bank further reinforced that the contraction in corporate loans was achieved "mainly through the reduction in large exposures" (Novo Banco, 2015, p. 87) as well as through "the non-renewal of credit lines" (Novo Banco, 2014, p. 71). Given that the bailed-in bank is by far the most exposed bank to the resolution (i.e., it has the highest *Bank Exposure* value), this deleveraging plan following the intervention focused on reducing large exposures and credit lines is entirely consistent with the credit supply contraction at the intensive margin we show in Table 2.2.

Finally, in contrast with a shift in firm demand from the bailed-in bank to other banks explained by reputational damage or even liquidity and solvency concerns, the 13 percent contraction in corporate loans from August 2014 to December 2015 was accompanied by a 7.4 percent increase in customer deposits (Novo Banco, 2015, p. 97). This suggests that despite the challenges brought by the resolution measure, the bank was able to stabilize its funding sources and at least partially recover its customers' confidence.[17]

[16] Although we argue here against this demand explanation, it is important to note that even such borrower behavior would be a direct reaction to a supply-side shock and, therefore, would not constitute a demand-side shift per se. In other words, even if part of a possible credit reduction was driven by customers rather than the bank, we would argue that this is still a supply-side shock as caused by the bank failure rather than by changes in firms' credit demand.

[17] As highlighted by Paravisini, Rappoport, and Schnabl (2017), our identifying assumption may also be violated if more exposed banks were specialized in certain industries or sectors such as export markets. In such segments where some banks may have more expertise than others, credit is no longer a homogeneous good

Robustness Tests. The within-firm results presented above are robust to a number of tests. First, we use an alternative bank exposure measure based on daily 5-year CDS spreads on senior unsecured debt. In this case, we consider the four banks operating in Portugal that are classified as significant institutions by the ECB and for which we have CDS spread data. In detail, we define bank exposure to the shock as the bank-specific increase in CDS spreads from one month before to the day before the resolution. Given that, as we show in Figure 2.1, CDS spreads of the bailed-in bank moved in line with the rest of the banking sector until late June 2014 when the exposures to the Group's entities owned by the family started to be revealed, this measure captures the market's perception of the increase in the default probability for the resolved bank as well as the magnitude of potential spillovers for the three other major Portuguese banks. Consistent with the estimates in the baseline regressions, the results in columns (1) to (3) of Appendix Table 2.1 show that a one standard deviation increase in bank exposure to the shock captured through the reaction of CDS spreads (0.014) leads to an decrease in the supply of credit of 2.93 percent for the average firm – 2.58 for SMEs and 8.60 for large firms.[18]

Second, to ensure that our results are not confined to firms with multiple bank relationships, we use the complete sample of borrowing firms in Portugal (including single-bank-relationship firms) in a model that replaces firm fixed-effects with location-size-sector fixed-effects as in De Jonghe, Dewachter, Mulier, Ongena, and Schepens (2018). In detail, the group contains only the firm itself in case the firm has multiple lending relationships, while firms with

offered across different banks and, as a result, sector-level demand shocks may ultimately lead to firm-bank specific loan demand. Nevertheless, untabulated results (for confidentiality reasons) suggest that firm-bank specific demand due to sector specialization is not a source of great concern in our setting. In fact, the bailed-in bank was active in all the main industries and did not control the majority of the lending activity in any of them. Our results could also be biased if certain banks were targeting their lending to firms in industries experiencing particularly severe (and correlated) demand-side shocks. However, when we compare the relative importance of certain industries for the bailed-in bank vis-à-vis all other banks, we observe no discernible differences across industries between the two groups.

[18]Since there are only 4 banks with available CDS spread data, standard errors clustered at the bank-level would be biased (Bertrand, Duflo, and Mullainathan, 2004; Cameron, Gelbach, and Miller, 2008). Thus, in columns (1) to (3) of Appendix Table 2.1 we use heteroskedasticity-consistent standard errors. The coefficients of interest are still significant at conventional levels when using either the wild cluster bootstrap method of Cameron, Gelbach, and Miller (2008) or clustering standard errors at the firm level.

single bank relationships are grouped based on the district in which they are headquartered, their industry, and deciles of loan size. The results are reported in columns (4) to (6) of Appendix Table 2.1. Despite the considerable increase in the number of firms (from 40,927 to 85,149), the coefficient estimates are remarkably similar to those in the smaller sample restricted to firms with multiple bank relationships, both in terms of magnitude and statistical significance.[19]

Third, we limit our sample to loan operations and thus disregard both used and unused credit lines (Appendix Table 2.2, columns 1-3). Only the coefficient estimate for large firms enters significantly and is smaller in magnitude when compared to estimate for total credit. This confirms that banks more exposed to the shock reduced credit supply primarily by extending fewer and smaller credit lines, particularly for SMEs. Finally, in columns (4) to (6) of Appendix Table 2.2 we follow Iyer, Peydró, Da-Rocha-Lopes, and Schoar (2014) and compare lending immediately before (2014:Q2) and one year after the shock (2015:Q3) instead of time-averaging the quarterly credit exposures into a pre (2013:Q4 - 2014:Q2) and post-resolution (2014:Q3 - 2015:Q3) period. Our results are the same, if not stronger, when compared to our baseline regressions.

Firm Heterogeneity. While we observe a credit supply reduction on average and particularly for larger firms, this contraction might vary across other firm characteristics, e.g., firm age, capital, profitability, liquidity or riskiness. In this respect, the results in Table 2.3 show further variation in the effect of the bank collapse and subsequent resolution across different firms by introducing an interaction term between *Bank Exposure* and a dummy variable splitting firms into two sub-groups: (i) firms with low and high values of a certain pre-shock firm characteristic; (ii) firms with and without NPLs with any bank before the resolution; and (iii) firms whose main lender in the pre-period was or was not the bailed-in bank.

[19]Comparing the results across multiple relationship firms (Table 2.2) and all firms (Appendix Table 2.1), the coefficients estimates are -1.339 vs. 1.520 for the average firm, -1.283 vs. -1.441 for SMEs, and -2.915 vs. -3.133 for large firms.

Table 2.3: Firm heterogeneity in credit supply – within-firm estimates

	$\Delta logTotalCredit_{bi}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bank Exposure	-1.651**	-1.641*	-1.480*	-1.630**	-1.934**	-1.322	-1.842**	-1.605*	-0.439
	(0.827)	(0.834)	(0.811)	(0.800)	(0.817)	(0.808)	(0.837)	(0.821)	(0.830)
Bank Exposure × Firm Assets (D_i=Small)	0.650***								
	(0.213)								
Bank Exposure × Firm No. Employees (D_i=Small)		0.430**							
		(0.180)							
Bank Exposure × Firm Age (D_i=Young)			-0.117						
			(0.224)						
Bank Exposure × Firm Capital Ratio (D_i=Low)				0.230					
				(0.283)					
Bank Exposure × Firm ROA (D_i=Low)					0.817***				
					(0.292)				
Bank Exposure × Firm Current Ratio (D_i=Low)						-0.371			
						(0.278)			
Bank Exposure × Firm Interest Coverage (D_i=Low)							0.447		
							(0.335)		
Bank Exposure × Firm with a NPL in the Pre-Period								1.648**	
								(0.762)	
Bank Exposure × Firm Main Lender = Bailed-in Bank									-3.132***
									(0.399)
No. Observations	116,245	116,246	116,247	116,248	116,249	116,250	116,251	116,252	116,253
No. Firms	40,927	40,927	40,927	40,927	40,927	40,927	40,927	40,927	40,927
Adj. R^2	0.049	0.049	0.049	0.049	0.050	0.049	0.049	0.050	0.051
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y	Y	Y	Y

The table presents estimation results of the within-firm specification (2.1) with Bank Exposure interacted with a dummy variable splitting firms into two sub-groups: (i) banks with low and high values of a certain pre-shock firm characteristic; (ii) banks with and without NPLs with any bank before the resolution; and (iii) banks firms whose main lender in the pre-period was or was not the bailed-in bank. The dependent variable is the change in the log level of total (committed) credit between each firm-bank pair. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank Controls are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets) and bank NPLs (non-performing loans to total gross loans). Firm assets, no. employees, age, capital ratio, ROA, current ratio and interest coverage (gross profit over interest expense on loans) are all measured as at 2013:Q4. Heteroskedasticity-consistent standard errors clustered at the bank level are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

The results reported in columns (1) and (2) of Table 2.3 confirm our earlier findings that the credit reduction by banks more exposed to the bail-in was more pronounced for larger firms, here measured by either total assets or number of employees instead of the definition in the EU Recommendation 2003/361 that includes both a balance-sheet size and a staff headcount requirement. We also find that firms with lower profitability suffered a lower reduction in lending by more affected banks (column 5), while borrowers with a non-performing loan before the shock with any bank in Portugal did not suffer from a reduction in lending by banks more exposed to the bail-in (column 8). We do not find any significant interaction of *Bank Exposure* with borrowers' age, capital or liquidity situation. Riskier borrowers therefore suffered less of a credit supply shock by more exposed banks, which is suggestive of evergreening of bank loans by banks more exposed to the bail-in. This could point to a certain degree of moral hazard that may be explained by the need for the "good bank" to show promising bank's financials to potential buyers.[20]

Finally, the results in column (9) show a significant and negative interaction term of *Bank Exposure* with a dummy variable equal to one if the firm main lender before the shock was the bailed-in bank, and zero otherwise. This suggests that those firms likely to have stronger relationships with the resolved bank suffered relatively more from the failure. While this result contrasts the evidence on the insulating effect of relationship banking on the quantity of credit following negative bank shocks (Beck, Degryse, De Haas, and Van Horen, 2018b; Bolton, Freixas, Gambacorta, and Mistrulli, 2016; Liberti and Sturgess, 2018; Sette and Gobbi, 2015), it highlights the disruptive effect that a bank failure can have on established firm-bank relationships, particularly for bank-dependent borrowers. In fact, consistent with the hypothesis that severely distressed banks may simply not have the resources to sustain such mutually beneficial relationships, Carvalho, Ferreira, and Matos (2015) find that bank

[20] This finding is consistent with Blattner, Farinha, and Rebelo (2018) who show that an unexpected increase in capital requirements for a subset of Portuguese banks in 2011 resulted in an increase in underreporting of loan losses and a reallocation of credit to firms in financial distress with prior underreported losses, with negative repercussions for aggregate productivity.

distress is associated with equity valuation losses and investment cuts to firms with the strongest lending relationships.

Cross-Sectional Analysis. So far we have gauged the effect of bank resolution on the supply of credit to firms borrowing from banks more and less exposed to the bail-in. However, the within-firm estimations ignore credit flows from new lending relationships as well as bank relationships that were terminated from the pre- to the post-bail-in period. Therefore, we now turn to the cross-sectional (between-firm) estimations that allow us to test for aggregate effects. Since we cannot use firm-fixed effects in the regressions analyzing the overall impact of bank shocks on credit supply, we control for omitted firm-level factors such as credit demand with a two-step estimation based on Abowd, Kramarz, and Margolis (1999). Specifically, we include in the estimations the vector of firm-level dummies estimated in column (2) of Table 2.2.[21] We also include industry and district fixed effects as additional controls for unobservable demand and risk-profile differences.

The results in Table 2.4 show there was no decrease in overall credit after the shock for firms more exposed to the bail-in when compared to firms exposed less, including when differentiating between firms of different size. However, we do observe a binding contraction in credit lines for SMEs more exposed to the resolution. In detail, the explanatory variable of interest, *Firm Exposure*, is computed as the weighted average of *Bank Exposure* across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Column (1) reports the results for the average firm, while column (2) differentiates between SMEs and large enterprises. None of the coefficients enters significantly at conventional levels. The results in columns (3) and (4), however, indicate that SMEs more exposed to the shock suffered a considerable decrease in the amount of credit lines available to them. Specifically, for a one standard deviation in firm exposure to the bail-in, SMEs experienced a 2.2 percent

[21] If biases due to endogenous matching between firms and banks were present in our data, we should observe a substantial correlation between exposure and $\hat{\alpha}_i$ (Cingano, Manaresi, and Sette, 2016; Jiménez, Mian, Peydró, and Saurina, 2014a). However, exploiting model (2.1), we find that the estimated vector of firm-level dummies is virtually uncorrelated with our main *Bank Exposure* measure (ρ=0.0014).

binding decrease in granted credit lines. As in columns (5) to (6) of Table 2.2, to ensure the effect is properly identified we restrict the sample to firms with credit lines from at least two different banks.

Robustness Tests. The results above are robust to several tests. First, we consider an alternative firm exposure measure that, as in Appendix Table 2.1, is based on reaction of CDS spreads on senior unsecured debt and considers the four banks operating in Portugal that are classified as significant institutions by the ECB. Specifically, in columns (1) to (4) of Appendix Table 2.3 *Firm Exposure* is computed as the weighted average of *Bank Exposure* across all banks lending to a firm (using as weights the pre-period share of total credit from each bank), but where bank exposure to the shock is defined as the bank-specific increase in CDS spreads from one month before to the day of the resolution. Second, in columns (5) to (8) of Appendix Table 2.3 we follow the same procedure but define *Bank Exposure* as a dummy variable equal to one for the bailed-in bank and 0 otherwise i.e., we implicitly assume that only the bailed-in bank was affected by the resolution and there were no spillover effects to other banks in the system. In either case, our conclusions remain the same. Finally, we also confirm our findings when considering the change in total committed credit excluding credit lines (Appendix Table 2.4, columns 1–3) or when comparing lending immediately before (2014:Q2) and one year after the shock (2015:Q3) instead of time-averaging the quarterly credit exposures into a pre and post-resolution period (Appendix Table 2.4, columns 4–6).

The Role of New and Existing Lending Relationships. The results in columns (1) to (3) of Table 2.5 show that firms more exposed to the bail-in were not more likely to start a new lending relationship over our sample period. This result can be explained by the fact that the average firm already has 4 bank relationships – see 2.1. The set-up of the table is identical to Table 2.4, but the dependent variable is now a dummy that takes value one if a firm takes out a loan from a bank with which it had no lending relationship before the shock, and zero otherwise. The coefficient estimated reported in columns (4) to (6) confirm that lenders other than the bailed-in bank (i.e., those banks that were less exposed to the resolution) were crucial for firms to maintain credit. Specifically, the dependent variable is now the change in the log level of total committed credit to each firm from all banks except the bailed-in bank from the pre (2013:Q4-2014:Q2) to the post-resolution period (2014:Q3-2015:Q3). The results show a significant and positive relationship between *Firm Exposure* and credit growth from banks other than the bailed-in bank. In economic terms, a one standard deviation increase in firm exposure to the bail-in is associated with a 5.9 percent increase in lending from other banks. This effect applies to both SMEs and large enterprises.

Overall, our results show that both SMEs and large firms that were more exposed to the bail-in did not suffer an overall reduction in credit when compared to firms exposed less. In fact, these firms were able to compensate the reduction in credit with lending from other (less exposed) financial institutions they already had a relationship with. However, when isolating credit lines from total committed credit by focusing on firms with multiple credit lines, we show that SMEs more exposed to the resolution were subject to a binding contraction in quantity of funds available through lines of credit, a crucial component for corporate liquidity management (Jiménez, Lopez, and Saurina, 2009; Sufi, 2009) and the dominant source of liquidity for firms around the world (Duchin, Ozbas, and Sensoy, 2010).[22]

2.5.2 Bank Resolution and Credit Conditions

We have mainly focused so far on the consequences of the supply shock for credit quantities. Nevertheless, the resolution may have also impacted the interest rates charged on new loans and credit lines, as well as other credit conditions such as loan maturities or the collateral required.[23] The disruption of established bank-firm relationships can ultimately have negative effects on real activity if borrowers are unable to replace these relationships with other lenders on equal terms (Ashcraft, 2005; Bernanke, 1983).

[22] According to Berger and Udell (1995), a credit line "is an attractive vehicle for studying the bank-borrower relationship because the line of credit itself represents a formalization of this relationship".

[23] Santos (2011), for instance, finds that firms that had relationships with less healthy lenders before the subprime crisis paid relatively higher loan spreads afterwards.

The results in columns (1) to (4) of Table 2.6 show that firms across all size groups that were more exposed to the bail-in saw a moderate increase in their interest rates on credit lines, while only more exposed large firms suffered a moderate increase in interest rates on all new credit operations. However, the economic effect is modest: a one standard deviation increase in firm exposure to the bail-in (0.013) is associated with a 20bp increase in the interest rates on credit lines for the average firm – for comparison, the average interest rate charged on credit lines is 11.05 percentage points. Since the interest rate dataset only captures new operations rather than outstanding amounts, here we consider all new loans and credit lines between a firm and a bank between 2013:M12 and 2014:M7 (pre-period) and 2014:M9 and 2015:M9 (post-period) when computing the loan-amount-weighted measures. Compared to Table 2.4, we now also control for loan characteristics such as the pre-shock, firm-specific, loan-amount-weighted maturity and share of collateralized credit for all new loans and credit lines. These results are consistent with the evidence in Khwaja and Mian (2008) and Cingano, Manaresi, and Sette (2016) who analyze a representative universe of firms in Pakistan and Italy and find that despite affecting the quantity of credit, bank-level shocks have no meaningful effects on the interest rates charged.

In line with a moderate tightening of interest rates, the results in columns (5) and (6) of Table 2.6 show a reduction in loan maturity across all firms, with a one standard deviation in firm exposure resulting in a one month reduction in loan maturity. Finally, we also find a relative increase in the share of collateralized credit after the shock for firms more exposed to the bail-in – a 2.75 percentage point increase for a one standard deviation increase in firm exposure (columns 7 and 8), an effect that holds for both SMEs and large enterprises.

Table 2.4: Credit supply and firm size – cross-sectional estimates

	$\Delta logTotalCredit_i$		$\Delta logCreditLines_i$	
	(1)	(2)	(3)	(4)
Firm Exposure	-0.374		-1.785***	
	(0.352)		(0.485)	
Firm Exposure × SMEs		-0.378		-1.839***
		(0.355)		(0.572)
Firm Exposure × Large Firms		-0.267		-0.526
		(0.607)		(1.135)
Firm Size	-0.008***	-0.008***	-0.006	-0.007
	(0.001)	(0.002)	(0.008)	(0.009)
Firm Age	-0.058***	-0.058***	-0.042***	-0.042***
	(0.004)	(0.004)	(0.012)	(0.011)
Firm ROA	0.228***	0.228***	0.575***	0.575***
	(0.043)	(0.046)	(0.133)	(0.132)
Firm Capital Ratio	0.039***	0.039***	0.024	0.024
	(0.009)	(0.010)	(0.029)	(0.031)
Firm Current Ratio	-0.002**	-0.002**	0.003	0.003
	(0.001)	(0.001)	(0.004)	(0.004)
Credit Demand	0.530***	0.530***	0.510***	0.510***
	(0.013)	(0.018)	(0.020)	(0.017)
No. Observations / Firms	40,927	40,927	14,320	14,320
Adj. R^2	0.378	0.378	0.175	0.175
Bank Controls	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
District FE	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y
Credit Lines with ≠ Banks	N	N	Y	Y

The table presents estimation results of the between-firm specification (2.2) where the dependent variables are the change in the log level of total (committed) credit and the change in granted credit lines for each firm. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm-level controls, defined in Table 1, are also measured in 2013:Q4. Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Table 2.5: New lending relationships and credit supply from less exposed banks

	$NewLending$ $Relationship_i$			$\Delta logCredit_i$ (Except Bailed-in Bank)		
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure	0.535	-0.659		4.020***	4.566***	
	(0.352)	(0.423)		(0.518)	(0.558)	
Firm Exposure × SMEs			-0.674			4.540***
			(0.433)			(0.585)
Firm Exposure × Large Firms			-0.220			5.359***
			(0.809)			(1.042)
No. Observations / Firms	40,927	40,927	40,927	40,927	40,927	40,927
Adj. R^2	0.012	0.058	0.058	0.018	0.342	0.342
Firm Controls	N	Y	Y	N	Y	Y
Bank Controls	N	Y	Y	N	Y	Y
Credit Demand	N	Y	Y	N	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2) where the dependent variables are either a dummy that takes value one if a firm takes out a loan from a bank with which it had no lending relationship before the shock, and zero otherwise, or the the change in the log level of total (committed) credit for each firm between 2013:Q4 and 2015:Q3 excluding the bailed-in bank. Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Table 2.6: Firm exposure to the bail-in and interest rates

	$\Delta InterestRates$ $AllNewCreditOperations_i$		$\Delta InterestRates$ $CreditLinesOnly_i$		$\Delta Maturity_i$		$\Delta Collateral_i$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm Exposure	2.335		17.966***		-53.289***		2.125**	
	(3.027)		(4.306)		(12.003)		(0.793)	
Firm Exposure × SMEs		1.495		17.665***		-51.817***		2.130**
		(3.331)		(2.923)		(4.361)		(0.797)
Firm Exposure × Large Firms		24.470***		25.437**		-91.801**		1.984**
		(6.805)		(11.606)		(42.287)		(0.762)
No. Observations / Firms	31,472	31,472	12,429	12,429	31,472	31,472	31,472	31,472
Adj. R^2	0.097	0.097	0.082	0.082	0.031	0.031	0.076	0.076
Loan Characteristics	Y	Y	Y	Y	Y	Y	Y	Y
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2) where the dependent variable is the firm-specific change in the loan-amount-weighted interest rates on new credit operations, interest rates on credit lines (i.e., automatic renewal of credit), maturity and share of collateralized credit. Since the interest rate dataset only captures new operations (rather than outstanding amounts), we consider all new credit operations between a firm and a bank between 2013:M12 and 2014:M7 (pre-period) and 2014:M9 and 2015:M9 (post-period) when computing these measures - the shock occurred in August 2014. Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Firm size categories are defined according to the EU Recommendation 2003/361. Loan characteristics are the pre-shock, firm-specific, loan-amount-weighted maturity and share of collateralized credit for all new loans or all new credit lines. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capitalization (regulatory capital ratio), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

2.5.3 Bank Resolution and Real Sector Effects

Impact on investment and employment. What was the effect of changes in financing conditions on investment and employment decisions taken by the affected firms? On the one hand, it is not clear that we should find significant real effects given the continued access to the same level of external funding, though at somewhat worse conditions and with smaller granted credit lines. On the other hand, the results point towards higher uncertainty for more exposed firms as they had to compensate the lost funding at the intensive margin with credit from other banks and (re)-negotiate loan terms and conditions. We therefore turn to investment and employment growth as real sector outcome variables, and then focus on the role of firms' internal liquidity in driving the results.

The results in Table 2.7 show a relative reduction in investment for SMEs that were more exposed to the resolution. The dependent variable in columns (1) to (3) is the change in the log level of tangible assets for each firm between 2013:Q4 and 2015:Q4, with column (1) presenting a regression for the 14,320 firms with multiple credit lines at different banks, and columns (2) and (3) focusing on our main sample of 40,927 with more than one bank relationship. As before, all specifications include firm and bank controls, a proxy for credit demand, as well as industry and district fixed effects. In both cases, *Firm Exposure* enters negatively and significantly. This reduction in investment, however, is only significant for SMEs (column 3). We find that a one standard deviation increase in firm exposure to the bail-in is associated with a 2.0 percent relative reduction in investment for SMEs. Our results remain the same when using as dependent variable the change in the log level of fixed assets (columns 4-6), our two alternative firm exposure measures as in Appendix Table 2.3 (Appendix Table 2.5, columns 1-4), or when normalizing the change in tangible assets or fixed assets the firms' pre-period total assets (Appendix Table 2.5, columns 5-8).

In line with the evidence for investment, columns 1 to 3 of Table 2.8 show a significant and negative relationship between firm exposure to the bail-in and the growth of the number of employees at firms. As before, this effect is concentrated in SMEs and is not significant for large enterprises. Controlling for firm and bank characteristics, we find a 1.3 percent relative drop in the number of employees at SMEs for a one standard deviation increase in exposure to the resolution. The economic effect is smaller than for investment, in line with stronger persistence in employment than in investment decisions. Our conclusion is therefore consistent with Chodorow-Reich (2014) and Berton, Mocetti, Presbitero, and Richiardi (2018) that find that smaller firms are particularly vulnerable to the negative impact of a credit crunch on employment. Bottero, Lenzu, and Mezzanotti (2017) also show that while the credit supply contraction in Italy following the European sovereign crisis was similar in magnitude for large and small firms, it led to a reduction in investment and employment only in smaller firms.

To capture different margins of adjustment of employment, we also consider the log change in the total number of hours worked by all firm employees as an alternative outcome variable. The results are reported in columns 4 to 6. As before, the reduction in employment is only present in more exposed SMEs – a 1.5 percent relative decrease for a one standard deviation increase in firm exposure. Our findings also remain the same when considering our two alternative firm exposure variables computed as the weighted average of *Bank Exposure* across all banks lending to a firm (using as weights the pre-period share of total credit from each bank), but where bank exposure to the shock is defined as the bank-specific increase in CDS spreads from one more before to the day of the resolution (Appendix Table 2.6, columns 1 and 2), or as a dummy variable equal to one for the bailed-in bank and 0 otherwise (Appendix Table 2.6, columns 3 and 4).

The role of firms' internal liquidity. The option for firms to access liquidity from credit lines should be more valuable when internal liquidity is scarce (e.g.,)campello2011. Thus, if the dampening effects of the bank resolution on real sector outcomes are indeed driven by a reduction in granted credit lines for SMEs, we should observe heterogeneous effects according to their pre-shock liquidity position i.e., if firms view cash and lines of credit as liquidity substitutes and given the tighter credit line limits, illiquid SMEs might respond to the funding shock by increasing cash holdings while decreasing investment and employment. Berg (2018), for instance, shows in a different setting that while liquid SMEs are able to absorb credit supply shocks by using the existing cash buffers, their illiquid counterparts increase cash holdings when a loan application is rejected, cutting non-cash assets by more than the requested loan amount, and thus investment and employment.

We analyze this channel explicitly by splitting firms according to their ex-ante liquidity position i.e., low liquidity (cash holdings-to-total assets ratio before the shock lower than the median) vs. high liquidity firms (cash holdings-to-total assets ratio before the shock higher than the median). Table 2.9 reports the results, with Panel A focusing on the sub-sample of SMEs and Panel B on large firms. The coefficient estimates in column (1) show a significant increase in cash holdings by low liquidity SMEs more exposed to the bail-in. This effect is not present across large firms. Conversely, in line with a precautionary savings motive where firms hold cash as a buffer to protect themselves against adverse shocks (e.g., Duchin, Ozbas, and Sensoy, 2010), high liquidity firms more exposed to the bail-in decrease cash holdings considerably – a result that, in this case, holds for both SMEs and large firms. In economic terms, a one standard deviation increase in firm exposure to the bail-in leads to an increase in cash holdings for low liquidity SMEs of 17.5 percent, but a decrease for high liquidity SMEs of 17.7 percent. Low liquidity large firms do not change cash holdings as a result of the shock, but high liquidity large firms decrease cash holdings by 15.5 percent for a one standard deviation in firm exposure to the resolution.

Column (2) reports the coefficient estimates for investment, while column (3) focuses on employment. The results show that the negative real effects are concentrated in SMEs with low pre-period levels of internal liquidity, corresponding to those firms that increased cash holdings as a result of the shock. This suggests that more exposed SMEs and large firms with high liquidity before the bail-in were able to use their available internal liquidity buffers to compensate for the binding contraction in granted credit lines and thus maintain employment and investment. Instead, low liquidity SMEs more exposed to the bail-in responded by increasing cash holdings while decreasing investment and employment.

A potential concern regarding these findings is that low levels of firm liquidity prior to the shock might reflect declining demand for investment given that cash holdings are chosen at least partially based on anticipated growth opportunities (e.g., Opler, Pinkowitz, Stulz, and Williamson, 1999). To help ruling out this possibility, in Appendix Table 2.7 we split high and low liquidity SMEs according to the firm-specific pre-shock asset growth before the shock i.e., below and above the median of the overall sample. Our results hold across the two sub-samples. Specifically, SMEs with both lower and higher growth opportunities before the resolution increase cash holdings and decrease investment and employment if they had low levels of internal liquidity, and both decrease cash holdings and maintain employment and investment if they were highly liquid before the shock.[24]

In summary, the results in Tables 2.7, 2.8 and 2.9 show that although there was on average and across the different firm size groups no reduction in aggregate borrowing after the bank resolution, SMEs still decreased investment and employment. This is explained by smaller firms with low levels of internal liquidity before the shock reacting to the tightening of credit line limits by hoarding cash while at the same time cutting back on investment and employment. The negative impact of the bank resolution shock on investment and employment can thus be explained with heightened liquidity risk.

2.5.4 Bail-out vs. Bail-in

Our evidence pointing towards negative real effects after a bank bail-in is particularly relevant given the growing evidence that, even if setting the stage for aggressive risk-taking and future fragility, bank bail-outs can be effective in supporting borrowers and the real economy in the short-term.[25] Giannetti and Simonov (2013), for instance, use loan-level data to explore the real effects of bank bail-outs during the Japanese crisis of the 1990s and find that listed firms had easier access to bank lending experienced positive abnormal returns and were able to invest more when the recapitalizations were large enough. Using a similar methodology,

[24]It is important to note that low prior liquidity may also reflect unobservably lower costs of external finance. If that is the case, this would imply we are actually underestimating the effect since we are treating liquidity differences as random.

[25]The implicit or explicit commitment to bail-out distressed banks not only increases idiosyncratic bank risk-taking (Dam and Koetter, 2012), but also give incentives for individual banks to engage in collective risk-taking strategies (Farhi and Tirole, 2012). The resulting common exposures aimed at exploiting a “too-many-to-fail” guarantee may ultimately increase systemic risk due to the higher correlation of defaults and amplification of the impact of liquidity shocks (Silva, 2017).

Augusto and Félix (2014) show that bank bail-outs in Portugal during the European sovereign debt crisis did not lead to a decrease in credit provision at the intensive margin. Laeven and Valencia (2013) examine financial sector interventions in 50 countries after the 2007-2009 financial crisis and show that these improved the value added growth of financially dependent firms. Berger, Makaew, and Roman (2016b) show that TARP-funded bail-outs in the US resulted in an increase in credit supply for recipient banks' borrowers as well as more favorable loan conditions, while Berger and Roman (2017) find that TARP led to increased job creation and decreased business and personal bankruptcies.[26]

Therefore, a fundamental follow-up question is whether a bank bail-out would generate the same negative effects we show in the paper for a bail-in. While we cannot make this comparison directly due to the lack of a counterfactual (e.g., a bank that was bailed-out during the same period), we shed some light into this issue by exploiting the fact that the bail-in of shareholders and junior bondholders we analyze so far in this paper differed significantly from the approach taken by the Portuguese authorities during earlier bank failures during the crisis. Notably, in June 2012 3 of the largest 5 banks (CGD, BCP and BPI) received government-funded capital injections as well as the smaller Banif in December 2012. The bail-outs allowed banks to comply with stricter minimum capital requirements defined by the European Banking Authority (EBA) in the case of the former, and by the Bank of Portugal in the latter.[27]

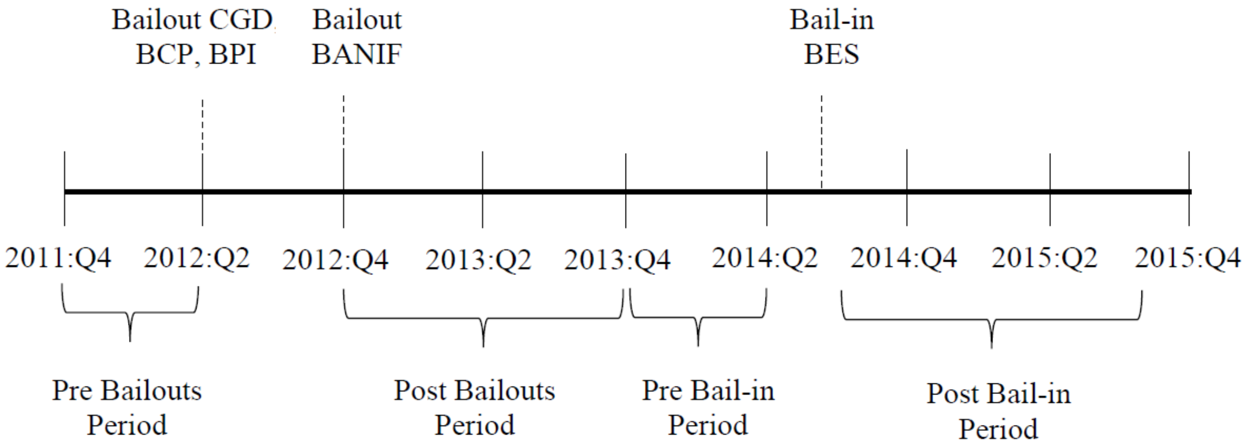
To assess the effects of the bail-outs on credit supply and real outcomes, we use both within- and cross-firm regressions, with data averaged between the fourth quarter of 2011 and the second quarter of 2012 as pre-bail-out period and between the fourth quarter of 2012 and the fourth quarter of 2013 as post-bail-out period – see Figure 2.3. We have data on

[26]By allowing the continuation of healthy lending relationships, either a bail-in or a bail-out should nonetheless affect borrowers less than a closure and liquidation of the bank. In fact, a decisive and effective intervention of either type may be able to reduce negative contagion effects and help off-set any negative credit supply effects by allowing other banks to provide additional credit to affected firms.

[27]The minimum Core Tier 1 ratio increased to 10 per cent in 2012 and banks had to comply until the end of that year. At the same time, banks subject to the stress tests of the EBA were also subject to stricter capital requirements. These additional capital requirements were one of the main factors contributing to the Portuguese bank bail-outs in 2012 (Augusto and Félix, 2014).

45,062 firms who had relationships with at least two banks, including the four bailed-out banks, for a total of 122,749 firm-bank relationships. *Bank Exposure* is a dummy variable that takes on the value one for bailed-out banks and zero otherwise in the baseline case.

Figure 2.3: Timeline of events – bail-outs and bail-in



The figure shows the timeline of the different bank resolutions in Portugal: (i) June 2012 for Caixa Geral de Depositos, Banco BPI and Banco Millennium BCP; (ii) December 2012 for BANIF; and (iii) August 2014 for BES.

The results in column (1) of Table 2.10 show no significant difference in credit growth at the intensive margin between the bailed-out and other banks for the same borrower, suggesting that the recapitalization and bail-out of failing banks during the the crisis had no relative effect on credit supply.[28] The results in columns (2) to (4) present the cross-sectional results where, as before, the outcomes variables of interest are the log change in credit, investment and employment between the pre- and post-bail-out period. The coefficient estimates show no significant variation in firm-level credit supply, investment or employment with exposure to bailed-out banks, suggesting that the bail-outs fulfilled their objectives of protecting borrowers of failing banks.

[28]In robustness tests available in Appendix Table 2.8, we find no significant effect of the bail-out for either large firms or SMEs and show the robustness of our findings to an alternative measure of *Bank Exposure*, defined as a continuous treatment variable equal to the injection amount as a share of assets for each of the bailed-out banks and zero otherwise.

Table 2.7: Firm exposure to the bail-in and investment

	$\Delta logTangibleAssets_i$			$\Delta logFixedAssets_i$		
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure	-1.680***	-1.497***		-1.349***	-1.000**	
	(0.312)	(0.327)		(0.249)	(0.396)	
Firm Exposure × SMEs			-1.531***			-1.018**
			(0.337)			(0.394)
Firm Exposure × Large Firms			-0.489			-0.460
			(1.322)			(1.242)
No. Observations / Firms	14,320	40,927	40,927	14,320	40,927	40,927
Adj. R^2	0.045	0.041	0.041	0.043	0.039	0.039
Firm Controls	Y	Y	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y
Credit Lines with $\neq$ Banks	Y	N	N	Y	N	N

The table presents estimation results of the between-firm specification (2.2) where the dependent variables are the change in the log level of tangible assets and in the log level of fixed assets for each firm between 2013:Q4 and 2015:Q4 (the shock occurred in August 2014). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Table 2.8: Firm exposure to the bail-in and employment

	$\Delta logNo.Employees_i$			$\Delta logNo.WorkedHours_i$		
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure	-1.183**	-0.945***		-1.644***	-1.154***	
	(0.410)	(0.182)		(0.326)	(0.163)	
Firm Exposure × SMEs			-0.971***			-1.182***
			(0.180)			(0.169)
Firm Exposure × Large Firms			-0.190			-0.325
			(0.501)			(0.525)
No. Observations / Firms	14,320	40,927	40,927	14,320	40,927	40,927
Adj. R^2	0.080	0.041	0.041	0.054	0.047	0.047
Firm Controls	Y	Y	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y
Credit Lines with $\neq$ Banks	Y	N	N	Y	N	N

The table presents estimation results of the between-firm specification (2.2) where the dependent variables are the change in the log level of no. employees and in the log level of total no. worked hours for each firm between 2013:Q4 and 2015:Q4 (the shock occurred in August 2014). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Table 2.9: Firm exposure to the bail-in and liquidity

	Δlog $CashHoldings_i$	Δlog $TangibleAssets_i$	Δlog $No.Employees_i$
	(1)	(2)	(3)
Panel A: SMEs			
Firm Exposure × High Liquidity Firms	-13.579***	-0.093	-0.113
	(3.899)	(0.861)	(0.309)
Firm Exposure × Low Liquidity Firms	13.416***	-1.680***	-1.644***
	(4.249)	(0.420)	(0.135)
No. Observations / Firms	40,234	40,234	40,234
Adj. R^2	0.022	0.040	0.067
Panel B: Large Firms.			
Firm Exposure × High Liquidity Firms	-11.885**	-1.485	2.106
	(5.477)	(2.422)	(2.451)
Firm Exposure × Low Liquidity Firms	1.735	-3.870	-0.631
	(2.023)	(2.342)	(1.705)
No. Observations / Firms	689	689	689
Adj. R^2	0.075	0.083	0.094
Firm and Bank Controls	Y	Y	Y
Credit Demand	Y	Y	Y
Industry and District FE	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2) where firms are split according to their ex-ante liquidity position i.e., low liquidity (cash holdings-to-total assets ratio before the shock lower than the median) vs. high liquidity firms (cash holdings-to-total assets ratio before the shock higher than the median). The dependent variables are the change in the log level of cash holdings, investment (tangible assets) and employment (no. employees) for each firm between 2013:Q4 and 2015:Q4 (the shock occurred in August 2014). Panel A focuses on the sub-sample of SMEs and Panel B on large firms. Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Table 2.10: Credit supply and real effects of the 2012 bail-outs

	Within-firm estimates	Cross-sectional estimates		
	$\Delta logCredit_{bi}$	$\Delta logCredit_i$	$\Delta logTangibleAssets_i$	$\Delta logNo.Employees_i$
	(1)	(2)	(3)	(4)
Bank Exposure	0.040			
	(0.054)			
Firm Exposure		-0.027	0.020	-0.009
		(0.023)	(0.025)	(0.014)
No. Observations	122,749	45,062	45,062	45,062
No. Firms	45,062	45,062	45,062	45,062
Adj. R^2	0.066	0.472	0.039	0.076
Bank Controls	Y	Y	Y	Y
Firm FE	Y	N	N	N
Credit Demand	N	Y	Y	Y
Firm Controls	N	Y	Y	Y
Industry FE	N	Y	Y	Y
District FE	N	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y

The table presents estimation results of the within-firm specifications (1) and (2) where Bank Exposure is a dummy variable that takes on the value one for the four Portuguese banks bailed-out in 2012 and zero otherwise. Firm Exposure captures the average exposure of each firm to the bail-outs and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2011:Q4-2012:Q2) and post-shock (2012:Q4-2013:Q4) period. Bank Controls are measured as at 2011:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2011:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression. We use heteroskedasticity-consistent standard errors clustered at the bank level in column (1) and clustered at the main bank and industry levels are in columns (2) to (4). Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

In summary, we find no evidence of a negative impact of the bank bail-out in 2012 on the relative credit supply by bailed-out vs. non-bailed-out banks. Consequently, there was also no relative decline in investment or employment by firms more exposed to the bailed-in banks. Overall, this points to rather sharp differences between bail-out and bail-in of banks, with stronger negative effects of the latter for credit supply and real sector activity. However, we urge caution in interpreting this comparison directly since the macroeconomic situation was considerably different during these two episodes and that the public intervention in 2012 was more systemic in nature. Moreover, previous evidence has shown the detrimental impact on bank risk-taking generated by public guarantees such as bailouts (Dam and Koetter, 2012) or even deposit insurance (Calomiris and Jaremski, 2018).[29] Instead, bank bail-ins should reduce moral-hazard due to creditors' expectation of bearing the losses in case of distress (Schäfer, Schnabel, and Weder, 2016).

2.6 Conclusion

Using loan-level data and exploiting within-firm and between-firm variation in exposure to different banks, including a failed and subsequently resolved bank, we show that banks more exposed to a bail-in significantly reduced credit supply after the shock but that affected firms were able to compensate this overall credit contraction with funding from other banks they already had relationships with. However, SMEs more exposed to the resolution were subject to a binding contraction in quantity of funds available through lines of credit. In addition, we find a moderate relative increase in lending costs for more exposed firms. In spite of the limited effects on credit supply, SMEs reduced both investment and employment, an effect that is concentrated among smaller firms with low

[29] Government interventions also reinforce the negative feedback loop between banks and sovereigns that characterized the euro area crisis (Brunnermeier, Langfield, Pagano, Reis, Van Nieuwerburgh, and Vayanos, 2017). Crosignani, Faria-e Castro, and Fonseca (2018), for instance, show that the ECB's three-year Long-Term Refinancing Operation incentivized Portuguese banks to purchase short-term domestic government bonds that could be pledged to obtain central bank liquidity, thus exacerbating the bank-sovereign negative feedback loop.

pre-shock internal liquidity that increased cash holdings at the expense of investment and employment.

Our findings show that a well-designed bank resolution framework that includes a bail-in of shareholders and bondholders can mitigate the impact of bank failures on credit supply and thus provide supporting evidence for the move from bail-out to bail-ins. However, the negative real effects we find also suggest that such resolution mechanism is not a silver bullet. Instead, only the combination of a robust supervisory and resolution frameworks can ensure a sound banking system and minimize the adverse effects of bank distress on the real economy.

Appendix 2.A. Additional Results

Appendix Table 2.1: Credit supply and firm size – within-firm estimates

	$\Delta logTotalCredit_{bi}$					
	Alternative Bank Exposure Measure (CDS Spread Reaction)			Including Firms With Only One Bank Relationship		
	(1)	(2)	(3)	(4)	(5)	(6)
Bank Exposure	-1.917***	-2.031***		-0.714***	-1.339**	
	(0.297)	(0.345)		(0.261)	(0.649)	
Bank Exposure × SMEs			-1.787***			-1.283*
			(0.350)			(0.652)
Bank Exposure × Large Firms			-5.956***			-2.915***
			(1.703)			(0.667)
No. Observations	40,783	40,783	40,783	160,457	160,457	160,457
No. Firms	17,445	17,445	17,445	85,139	85,139	85,139
Adj. R^2	0.001	0.054	0.054	0.053	0.055	0.055
No. Banks	4	4	4	98	98	98
Bank Controls	N	Y	Y	N	Y	Y
Firm FE	N	Y	Y	N	N	N
Location-Size-Sector FE	N	N	N	N	Y	Y
No. Bank Relationships > 1	Y	Y	Y	N	N	N

The table presents estimation results of the within-firm specification (2.1) where the dependent variables are the change in the log level of total (committed) credit between each firm-bank pair. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. Bank Exposure is defined as the bank-specific increase in CDS spreads from one more before to the day of the resolution in columns (1) to (3) and the percentage of assets of each bank exposed to the bail-in in columns (4) to (6) i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank Controls are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm size categories are defined according to the EU Recommendation 2003/361. In columns (4) to (6) we control for credit demand by replacing the firm fixed-effect in the within-firm regressions by a group (location-sector-size) fixed-effect. The group contains only the firm itself in case the firm has multiple lending relationships, while firms with single bank relationships are grouped based on the district in which they are headquartered, their industry, and deciles of loan size in the credit register. Heteroskedasticity-consistent standard errors clustered at the bank level are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.2: Credit supply and firm size – within-firm estimates

	$\Delta logCredit_{bi}$ (Excluding Credit Lines)			$\Delta logCredit_{bi}$ (2014:Q2-2015:Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
Bank Exposure	-0.963***	-1.108		-1.430***	-2.000**	
	(0.366)	(0.808)		(0.303)	(0.826)	
Bank Exposure × SMEs			-1.063			-1.812**
			(0.806)			(0.832)
Bank Exposure × Large Firms			-1.925*			-5.460***
			(0.986)			(0.927)
No. Observations	96,584	96,584	96,584	97,130	97,130	97,130
No. Firms	35,365	35,365	35,365	34,861	34,861	34,861
Adj. R^2	0.001	0.015	0.015	0.001	0.029	0.030
Bank Controls	N	Y	Y	N	Y	Y
Firm FE	N	Y	Y	N	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y

The table presents estimation results of the within-firm specification (2.1) where the dependent variables are the change in the log level of total credit between each firm-bank pair without considering used and unused credit lines (columns 1-3), the change in the log level of total (committed) credit between each firm-bank pair (columns 4-6). Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank Controls are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm size categories are defined according to the EU Recommendation 2003/361. Heteroskedasticity-consistent standard errors clustered at the bank level are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.3: Credit supply and firm size – cross-sectional estimates

	$\Delta logTotalCredit_i$		$\Delta logCreditLines_i$		$\Delta logTotalCredit_i$		$\Delta logCreditLines_i$	
	Alternative Firm Exposure Measure (Bank Exposure: CDS Spread Reaction)				Alternative Firm Exposure Measure (Bank Exposure: Dummy = 1 for Bailed-in Bank)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm Exposure	-0.520		-2.747		-0.032		-0.114***	
	(0.446)		(1.585)		(0.021)		(0.034)	
Firm Exposure × SMEs		-0.605		-3.051*		-0.032		-0.117***
		(0.454)		(1.522)		(0.022)		(0.034)
Firm Exposure × Large Firms		0.888		3.291		-0.028		-0.033
		(1.103)		(2.275)		(0.039)		(0.116)
No. Observations / Firms	17,444	17,444	5,420	5,420	40,927	40,927	14,320	14,320
Adj. R^2	0.299	0.299	0.162	0.162	0.378	0.378	0.175	0.175
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2) where the dependent variables are the change in the log level of total (committed) credit and the change in granted credit lines for each firm. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2013:Q4-2014:Q2) and post-shock (2014:Q3-2015:Q3) period. Bank Exposure is defined as the bank-specific increase in CDS spreads from one more before to the day of the resolution in columns (1) to (4) and a dummy variable equal to one for the bailed-in bank and 0 otherwise in columns (5) to (8). Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.4: Credit supply and firm size – cross-sectional estimates

	$\Delta logCredit_i$ (Excluding Credit Lines)		$\Delta logCredit_i$ (2014:Q2-2015:Q3)	
	(1)	(2)	(3)	(4)
Firm Exposure	-0.279		-0.478	
	(0.396)		(0.494)	
Firm Exposure × SMEs		-0.294		-0.523
		(0.425)		(0.464)
Firm Exposure × Large Firms		-0.206		0.630
		(0.375)		(1.372)
No. Observations / Firms	35,365	35,365	34,861	34,861
Adj. R^2	0.280	0.279	0.419	0.419
Firm Controls	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
District FE	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.5: Firm exposure to the bail-in and investment

	$\Delta logTangibleAssets_i$				$\Delta FixedAssets$ $/TotalAssets_i$		$\Delta TangibleAssets$ $/TotalAssets_i$	
	Alternative Firm Exposure Measure (Bank Exposure: CDS Spread Reaction)		Alternative Firm Exposure Measure (Bank Exposure: Dummy = 1 for) for Bailed-in Bank)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm Exposure	-4.422***		-0.101***		-0.234***		-0.219***	
	(0.756)		(0.027)		(0.061)		(0.059)	
Firm Exposure × SMEs		-4.518***		-0.104***		-0.240***		-0.226***
		(0.787)		(0.029)		(0.064)		(0.063)
Firm Exposure × Large Firms		-2.841		-0.034		-0.078		-0.023
		(2.428)		(0.093)		(0.154)		(0.161)
No. Observations / Firms	17,445	17,445	40,927	40,927	40,927	40,927	40,927	40,927
Adj. R^2	0.038	0.038	0.041	0.041	0.072	0.072	0.075	0.075
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is defined as the bank-specific increase in CDS spreads from one more before to the day of the resolution in columns (1) and (2), a dummy variable equal to one for the bailed-in bank and 0 otherwise in columns (3) and (4), and the percentage of assets of each bank exposed to the bail-in in columns (5) to (8) i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.6: Firm exposure to the bail-in and employment

	$\Delta logNo.Employees_i$			
	Alternative Firm Exposure Measure (Bank Exposure: CDS Spread Reaction)		Alternative Firm Exposure Measure (Bank Exposure: Dummy = 1 for Bailed-in Bank)	
	(1)	(2)	(3)	(4)
Firm Exposure	-2.346***		-0.061***	
	(0.291)		(0.017)	
Firm Exposure × SMEs		-2.420***		-0.062***
		(0.366)		(0.014)
Firm Exposure × Large Firms		-1.120		-0.002
		(1.059)		(0.050)
No. Observations / Firms	17,445	17,445	40,927	40,927
Adj. R^2	0.065	0.065	0.066	0.066
Firm Controls	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
District FE	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is defined as the bank-specific increase in CDS spreads from one more before to the day of the resolution in columns (1) and (2) and a dummy variable equal to one for the bailed-in bank and 0 otherwise in columns (3) and (4). Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.7: Firm exposure to the bail-in and liquidity

	$\Delta logCashHoldings_i$		$\Delta logTangibleAssets_i$		$\Delta logNo.Employees_i$	
	Low Asset Growth Firms	High Asset Growth Firms	Low Asset Growth Firms	High Asset Growth Firms	Low Asset Growth Firms	High Asset Growth Firms
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Exposure × High Liquidity Firms	-14.732***	-14.302***	0.358	-0.991	-0.155	-0.509
	(3.798)	(4.599)	(0.904)	(0.954)	(0.372)	(0.340)
Firm Exposure × Low Liquidity Firms	8.162***	13.523***	-1.907***	-1.902***	-1.327***	-0.983***
	(2.604)	(4.616)	(0.494)	(0.619)	(0.280)	(0.269)
No. Observations / Firms	19,331	20,030	19,331	20,030	19,331	20,030
Adj. R^2	0.017	0.023	0.031	0.036	0.044	0.058
Firm and Bank Controls	Y	Y	Y	Y	Y	Y
Credit Demand	Y	Y	Y	Y	Y	Y
Industry and District FE	Y	Y	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y

The table presents estimation results of the between-firm specification (2.2) where SMEs are split according to their ex-ante asset growth and liquidity (cash holdings-to-total assets) positions i.e., below and above the median. The dependent variables are the change in the log level of cash holdings, investment (tangible assets) and employment (no. employees) for each firm between 2013:Q4 and 2015:Q4 (the shock occurred in August 2014). Firm Exposure captures the average exposure of each firm to the bail-in and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. Bank Exposure is the percentage of assets of each bank exposed to the bail-in i.e., the percentage of assets that was effectively bailed-in for the resolved bank, the specific contribution to the ad-hoc loan to the Resolution Fund granted as part of the resolution for the 8 participating banks (as a percentage of assets), and 0 otherwise. Bank controls, averaged at the firm-level according to the pre-period share of total credit granted to the firm by each bank, are measured as at 2013:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2013:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression (Column 2 of Table 2). Heteroskedasticity-consistent standard errors clustered at the main bank and industry levels are in parenthesis. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Appendix Table 2.8: Credit supply and real effects of the 2012 bail-outs

	Within-firm estimates				Cross-sectional estimates			
	$\Delta logCredit_{bi}$				$\Delta logCredit_{i}$			
	Dummy Treatment Variable		Continuous Treatment Variable		Dummy Treatment Variable		Continuous Treatment Variable	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bank (col 1-6)/Firm (col 7-12) Exposure	0.040		2.067		-0.027		0.467	
	(0.054)		(1.492)		(0.023)		(0.765)	
Bank (col 1-6)/Firm (col 7-12) Exposure × SMEs		0.042		2.076		-0.027		0.469
		(0.054)		(1.500)		(0.024)		(0.771)
Bank (col 1-6)/Firm (col 7-12) Exposure × Large Firms		-0.022		1.873		-0.033		0.339
		(0.094)		(2.922)		(0.027)		(1.327)
No. Observations	122,749	122,749	122,749	122,749	45,062	45,062	45,062	45,062
No. Firms	45,062	45,062	45,062	45,062	45,062	45,062	45,062	45,062
Adj. R^2	0.066	0.066	0.066	0.066	0.472	0.472	0.472	0.472
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	N	N	N	N
Credit Demand	N	N	N	N	Y	Y	Y	Y
Firm Controls	N	N	N	N	Y	Y	Y	Y
Industry and District FE	N	N	N	N	Y	Y	Y	Y
No. Bank Relationships > 1	Y	Y	Y	Y	Y	Y	Y	Y

The table presents estimation results of the within-firm specifications (1) and (2) where Bank Exposure is a dummy variable that takes on the value one for the four Portuguese banks bailed-out in 2012 and zero otherwise in columns (1)–(2) and (5)–(6), and a continuous variable equal to the injection amount as a share of assets for each of the bailed-out banks, as zero otherwise in columns (3)–(4) and (7)–(8). Firm Exposure captures the average exposure of each firm to the bail-outs and is computed as the weighted average of Bank Exposure across all banks lending to a firm, using as weights the pre-period share of total credit from each bank. The quarterly data for each credit exposure is collapsed (time-averaged) into a single pre (2011:Q4-2012:Q2) and post-shock (2012:Q4-2013:Q4) period. Bank Controls are measured as at 2011:Q4 and include bank size (log of total assets), bank ROA (return-on-assets), bank capital ratio (equity to total assets), bank liquidity ratio (liquid to total assets), and bank NPLs (non-performing loans to total gross loans). Firm controls are also measured before the shock (2011:Q4) and include firm size (log of total assets), firm age (ln(1+age)), firm ROA (net income to total assets), firm capital ratio (equity to total assets) and firm current ratio (current assets to current liabilities). Credit demand is the vector of firm-level dummies estimated in the within-firm regression. We use heteroskedasticity-consistent standard errors clustered at the bank level in columns (1) to (4) and clustered at the main bank and industry levels are in columns (5) to (8). Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

Chapter 3

Financial Access Under the Microscope

3.1 Introduction

The microfinance sector has been a fundamental driver of financial inclusion in developing and emerging countries (Brown, Guin, and Kirschenmann, 2015b; Kaboski and Townsend, 2012; Morduch, 1999). A key but unanswered question is to what extent the expansion of microfinance could translate into easier access to the formal financial system. While individuals without credit history are usually excluded from commercial bank credit, they are often the target of microfinance institutions (MFIs). Thanks to repeated micro-loans and a track record of repayment, these individuals may be able to build a credit history and eventually graduate from MFIs to the formal banking sector. This could, in turn, translate

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into larger and cheaper loans, increasing income levels and promoting entrepreneurship and small business growth.

In this paper, we investigate this question by examining the effects of a large-scale nationwide banking expansion program conducted through MFIs on financial inclusion and considering its spillover effects on commercial banks. We argue that MFIs which target underprivileged populations allow previously unbanked borrowers to signal their creditworthiness and thus reduce information frictions. This under-documented role of MFIs is particularly useful in countries with a credit register and little collateralizable wealth, due to for instance a lack of land registries, illiquid markets for collateral, or weak institutions. The commercial banking sector also benefits from the reduction in information asymmetries as it can expand lending. As a result, microcredit expansion programs could potentially benefit the local economy not only directly by providing financial services to previously unbanked individuals, but also indirectly by facilitating their transition to the formal banking sector.

In detail, we analyze the impact of a large-scale, government-subsidized banking expansion program that created an network of community-focused saving and credit cooperatives (Umurenge SACCOs, henceforth "U-SACCOs", part of the microfinance sector) across the country's 416 municipalities.[1] The program resulted in more than 90% of Rwandans residing within 3 miles of a U-SACCO (AFI, 2014). Despite an official launch in 2009, different U-SACCOs started their lending operations in different months starting in late 2011. Our identification strategy exploits variation from the staggered implementation of the program across municipalities using micro-level and high-frequency data from a comprehensive credit register containing detailed information on the universe of loans extended by all credit institutions—commercial banks, U-SACCOs and other MFIs—to individual borrowers for a total of 9 years around the implementation

[1]Rwanda is representative of other developing countries. In 2015 Rwanda had a credit-to-GDP ratio of 21.3%, which compares to an average of 24% for sub-Saharan African economies and 19% for low-income countries (World Bank World Development Indicators).

of the program. The final cleaned credit register dataset includes more than 4 million observations on bank-borrower loan exposures on a monthly basis for 177,853 unique individual borrowers.

First, we show that the program increased the likelihood of access to credit for the previously unbanked population, particularly in rural areas with ex-ante lower bank presence. This effect is mostly driven by the microfinance institutions that were set up i.e., the U-SACCOs. We also show that borrowers are able to get larger loans and better loan terms as the relationship with the financial institutions mature. However, this effect is weaker for U-SACCOs when compared to commercial banks, suggesting that the former institutions could face operational constraints in expanding their credit supply (Cull, Demirgüç-Kunt, and Morduch, 2014). Consistent with this evidence, we observe that commercial banks start expanding their lending at the extensive margin one year after the introduction of the program, tapping in the pool of first-time borrowers at U-SACCOs. In fact, about 10 percent of first-time borrowers who need a subsequent loan switch to commercial banks.

In the second part of the paper, we zoom in on these previously unbanked individuals who switch from U-SACCOs to commercial banks. Based on the notion that borrowing from MFIs allows previously unbanked individuals to enter the credit register and build a credit history, we track individuals' borrowing activities, distinguishing between those who continue borrowing from U-SACCOs and those who become clients of commercial banks. When they switch from a U-SACCO to a bank, these borrowers benefit from cheaper, larger, and longer-term loans from the bank when compared to similar borrowers at U-SACCOs. However, they also receive smaller loans compared to similar individuals already at banks, but loan size increases over time. Switchers to commercial banks are *ex-post* less risky (e.g., they have a lower default risk) than non-switchers, but they are not riskier than existing commercial bank borrowers.

These results suggest that the program had spillover effects on commercial banks, which "cream-skim" low-risk borrowers from the pool of newly-banked individuals. Our findings also emphasize an important role for the microfinance sector which, coupled with the presence of the credit register, can mitigate information frictions in the credit market and facilitate the transition of individual borrowers from microfinance to the formal banking sector.

Our paper builds on an influential literature showing the positive effects of bank expansion programs on financial inclusion and economic development. Burgess and Pande (2005) and Burgess, Pande, and Wong (2005) document that a large state-led branch expansion program in India significantly reduced rural poverty through increased savings mobilization and credit provision.[2] A recent analysis of the largest financial inclusion program in India (JDY) by Agarwal, Alok, Ghosh, Ghosh, Piskorski, and Seru (2017), which led to 255 million new bank account openings, shows that regions more exposed to the program experienced an increase in lending. Thus, banks catered to the new demand for formal credit by previously unbanked households, which substituted informal lending with less expensive bank credit. In a case study of the branch network expansion by Banco Azteca in Mexico, Bruhn and Love (2014) find that expanded access to finance boosts labor market activity and incomes, particularly among poor individuals and in areas with lower bank presence. Brown, Guin, and Kirschenmann (2015b) show that the expansion of an East European commercial microfinance bank in low-income regions led to an increase in the share of banked households. Focusing on Africa, Allen, Carletti, Cull, Qian, Senbet, and Valenzuela (2014) examine the case of the branch expansion of Equity Bank in Kenya. The bank's expansion into low-income and under-served regions led to an increase in the likelihood of households having bank accounts and obtaining loans.[3]

[2]See also Kochar (2011), and Fulford (2013) for follow-up studies. Young (2017) documents positive impacts on agricultural and manufacturing output of a bank branch expansion program implemented in India in 2005.

[3]The positive effects of increased bank branch density on financial inclusion and economic outcomes are also extensively documented in advanced economies (Brown, Cookson, and Heimer, 2017; Gilje, Loutskina, and Strahan, 2016; Nguyen, 2018). In particular, Celerier and Matray (2018) show that the U.S. interstate bank branching deregulation increased financial inclusion, leading to improved economic conditions for low-income households, through asset accumulation and better financial security.

A common feature of existing studies is that they rely on survey data to measure access, usage of financial services, and economic outcomes. However, surveys may not be representative and suffer from reporting biases, particularly in relation to questions about finance (Greer, Parker, and Souleles, 2006). Furthermore, the data is often aggregated at the district or state level, inviting questions on whether the outcomes are being driven by a particular financial intermediary or its competitors. In other words, this type of analysis cannot establish if the increase in bank accounts, credit and the real effects following a banking expansion program are due to the targeted institutions or other existing banks.[4] Unlike previous studies, this is to our knowledge the first paper that employs extensive microdata from a credit register to study the dynamics of financial inclusion. We use a supervisory data on the lending activities of all microfinance institutions and commercial banks in a country, which allows us to overcome challenges related to aggregation and reporting biases. In addition, the data enables us to gauge not only which banks are driving gains in access to bank credit, but also to track individuals' borrowing activities over time and across lenders, measure the length of their credit history, determine their risk profile based on loan performance, and analyze the terms on which they are able to borrow from, and switch among, different lenders. Finally, the data extends several years into the program, allowing us to examine not only the short-term effect of the program but also its medium-term effects on financial access.

Our paper also relates more broadly to a long-standing literature arguing that banks and financial development are key drivers of economic growth.[5] In this respect, expanding financial inclusion can be seen as a necessary condition for economic development, as better access to credit could sustain entrepreneurship and help consumption smoothing. Access to credit is often provided by microfinance institutions, which extend micro loans

[4] Although banks tend to impose barriers to entry through minimum account balances or large overdraft fees (Barr and Blank, 2008; Ho and Ishii, 2011), the expansion of banks to poorer (rural) areas can have indirect effects on financial inclusion through increased competition with existing microcredit providers and other institutions that are attracted by the profitable opportunities in those areas.

[5] See, e.g., King and Levine (1993), Jayaratne and Strahan (1996), and Beck, Levine, and Loayza (2000).

to previously unbanked individuals. The evidence on the effectiveness of microfinance presents an interesting contrast between studies evaluating single interventions and those based on more aggregate data. On the one hand, a large set of randomized control trials (RCTs) generally reveal "a consistent pattern of modestly positive, but not transformative, effects" of microcredit (Banerjee, Karlan, and Zinman, 2015). On the other hand, as discussed, analyses of household surveys and banking data, which are typically aggregated at the district or state level, show more promising results (Bruhn and Love, 2014; Burgess and Pande, 2005). Our analysis helps to bridge this gap showing how the expansion of U-SACCOs could have positive spillovers on local economies, allowing previously unbanked individuals to transition from microfinance to commercial banking.

Finally, our switching analysis is also closely related to an influential paper by Ioannidou and Ongena (2010), who use data from the Bolivia credit register to document that firms who switch across banks get lower loan rates that subsequently increase, consistent with the presence of adverse selection that leads to a hold-up problem (Sharpe, 1990; von Thadden, 2004). We extend that line of research documenting, for the first time, the switching behavior of individual borrowers and focusing on the transition from microfinance to commercial banking. Moreover, other than looking at loan terms—size, interest rate, and maturity—we can compare the ex-post performance of switching and non-switching loans to assess whether the transition from microfinance to the formal banking sector could affect the riskiness of financial institutions.

The remainder of the paper is organized as follows. In Section 3.2 we describe the financial sector in Rwanda and the banking expansion program. Section 3.3 describes our data sources and Section 3.4 reports our baseline results on the impact of the banking expansion program on financial access. In Section 3.5 we analyze the transition of borrowers to the formal banking sector. Section 3.6 concludes.

3.2 Institutional Background

Rwandan Economy and Financial Sector. Rwanda is a landlocked country in East Africa with a population of 11.5 million. The country has a large rural population and few natural resources. Following a range of business-friendly reforms in the early 2000s, Rwanda experienced gains in competitiveness and strong economic growth. Annual GDP growth averaged 7.8% between 2008 and 2016 and per capita income doubled during the same period (IMF, 2017a). The 2018 World Bank's Doing Business survey ranks Rwanda 2nd in Africa and 41st in the world according to the ease of doing business, while the 2016-2017 World Economic Forum's Global Competitiveness Index ranks it 52nd among 138 countries, outperforming the Sub-Saharan Africa (SSA) average on all dimensions other than market size. The reforms associated with the "Vision 2020" economic strategy, which strives to make Rwanda a middle-income country by 2020, have been accompanied by a reduction in poverty and income inequality (IMF, 2017b).[6]

In recent years Rwanda also experienced a rapid growth of its banking sector. Total bank assets grew from 22% to 39% of GDP from 2008 to 2016, while bank credit to the private sector grew at an annual average of 13% in real terms over the same period (IMF, 2017a). Commercial banks represent about two-thirds of total banking sector assets. The banking sector is relatively concentrated, with the 3 largest commercial banks (out of 17) accounting for more than half of total bank assets, loans and deposits.[7] Most banks are foreign-owned, but the majority of bank funding is domestic and comes from local deposits, limiting exposure to external shocks. There are also 523 microfinance institutions (MFIs), including 416 municipal credit cooperatives (U-SACCOs) that were set up as part of the

[6]Gender inequality in Rwanda is the lowest in SSA. The World Economic Forum's 2016 Gender Gap Index ranks Rwanda 1st among low- and-middle-income countries and 5th worldwide in closing the gender gap.

[7]There are 17 banks in total: 11 commercial banks (one of which is pending regulatory approval), 1 development bank, 1 cooperative bank, and 4 micro-finance banks. We observe the lending activities of the 16 active banks in our data. For purposes of this paper, we refer to all these banks as "commercial banks". We include micro-finance banks in this list because, in contrast to micro-finance institutions, they have a similar legal status as commercial banks.

banking expansion program examined in this paper i.e., one U-SACCO in each of the 416 municipalities, with some only providing savings accounts, and others also granting loans. MFIs account for almost 6% of total bank assets.[8]

Over the past decade Rwanda also made notable improvements on financial inclusion. Access to formal financial services increased from 21% to 68% of the adult population between 2008 and 2016, and access to formal credit from 5% to 17% over the same period (FinScope, 2012, 2016). According to statistics across 26 countries where FinScope surveys are conducted to measure financial access and use of financial products, Rwanda is ranked 2nd in terms of the share of adult population with access to formal financial services.[9] These developments are the result of policies and regulations aimed at expanding financial access for the unbanked population. One such policy is the large-scale banking expansion program we analyze in this paper.

Banking Expansion (U-SACCO) Program. This paper examines the effects of the Umurenge SACCO (U-SACCO) program, which set up a "savings and credit cooperative" (SACCO) in each of Rwanda's 416 municipalities.[10] The goal of the program was to provide financial services at low transaction costs, particularly in rural and underserved communities. U-SACCOs were allowed to provide financial services to all individuals, but in practice targeted the unbanked population. The program was launched in March 2009 and initially focused on providing access to savings accounts, with different U-SACCOs only extending their first loans in late 2011. The program significantly improved the

[8] While not captured in our supervisory dataset, the financial sector also includes informal or semi-formal institutions such as village savings and loans associations, as well as mobile money providers that carry out financial transactions for various institutions (MFR, 2015).

[9] Rwanda also fares well compared to its regional peers in terms raising financial inclusion. The share of adult population with access to formal financial services (68% in 2016) places Rwanda above its East African peers such as Kenya (67% in 2013), Tanzania (57% in 2013), Uganda (54% in 2013) and Mozambique (24% in 2014). The Economist Intelligence Unit's Global Microscope, which ranks countries based on policies for financial inclusion, also puts Rwanda in the 8th position among 55 countries in 2016.

[10] Municipalities (translated in Kinyarwanda as "Umurenge") are administrative subdivisions of the 30 counties that make up 5 provinces. In Rwanda there are also 64 non-Umurenge SACCOs that already existed prior to the Umurenge program and where members come from the same profession. Throughout the paper, non-Umurenge SACCOs are part of the "other MFIs" sample.

availability of financial services across the country, with 1.6 million new customers and 91% of Rwandans residing within 3 miles of a U-SACCO branch (AFI, 2014), a larger share than in similar countries such as Kenya (86%), Uganda (77%), and Nigeria (56.5%).[11] Almost half of U-SACCO loans are extended for trade and tourism services and about one-fifth for agricultural activities, including livestock and fishing (MFR, 2015).

Municipality-specific U-SACCOs are financial intermediaries owned by their members. From a legal perspective, they are formed as micro-finance institutions with the main objective of providing credit and savings facilities exclusively to members, financed mainly from their own resources.[12] These credit cooperatives are regulated under both the Finance and Cooperative laws and are supervised by the Rwanda Cooperative Agency and the National Bank of Rwanda. They are located in both rural and urban areas, with the vast majority only having one branch with membership drawn from the local community (Brown, Mackie, and Smith, 2015a). While established as private cooperatives, U-SACCOs received subsidies from the government before reaching the break-even point. By the end of 2013, 85% of U-SACCOs were profitable and stopped receiving subsidies (AFI, 2014).[13]

It has been widely argued that the U-SACCO program substantially increased the share of the population with access to bank accounts, boosting financial inclusion especially in economically underprivileged areas. We document the rise in the share of banked population using data from the 2012 and 2016 FinScope surveys. As shown in Appendix Table 3.7, between 2012 and 2016 the share of individuals with a savings account rose from 31.9% to 36.4%, while the share of individuals who were granted loans doubled from 4.6% in 2012 to 8.1% in 2016. These results suggest that the program coincided with

[11] These figures are retrieved from http://fspmaps.com/, last accessed May 2018.

[12] Both U-SACCOs and other MFIs have the legal status of cooperatives and are microfinance institutions as they pursue social goals and serve underprivileged groups. U-SACCOs differ from other types of SACCOs in the sense that they target borrowers based on their geographical location (the municipality) while other SACCOs target borrowers based on employment type (MFR, 2015).

[13] At set-up, U-SACCOs were required to maintain a liquidity ratio of 80%, which was reduced to 30% after December 2013. The minimum capital requirement is about USD 8,000. U-SACCOs generally hold high levels of capital, in excess of 30% of total assets (MFR, 2015).

gains in financial inclusion and are consistent with government and news reports (e.g., Randall, 2014).

Our analysis takes the next step and examines whether the banking expansion program had deeper effects than simply increasing access to basic financial services. Specifically, we are interested in the program's impact on previously unbanked individuals' ability to take up loans from U-SACCOs (and on which terms), their ability to build a credit history, and eventually to borrow from the formal banking sector, with possible beneficial effects on local economic activity.

3.3 The Credit Register Data

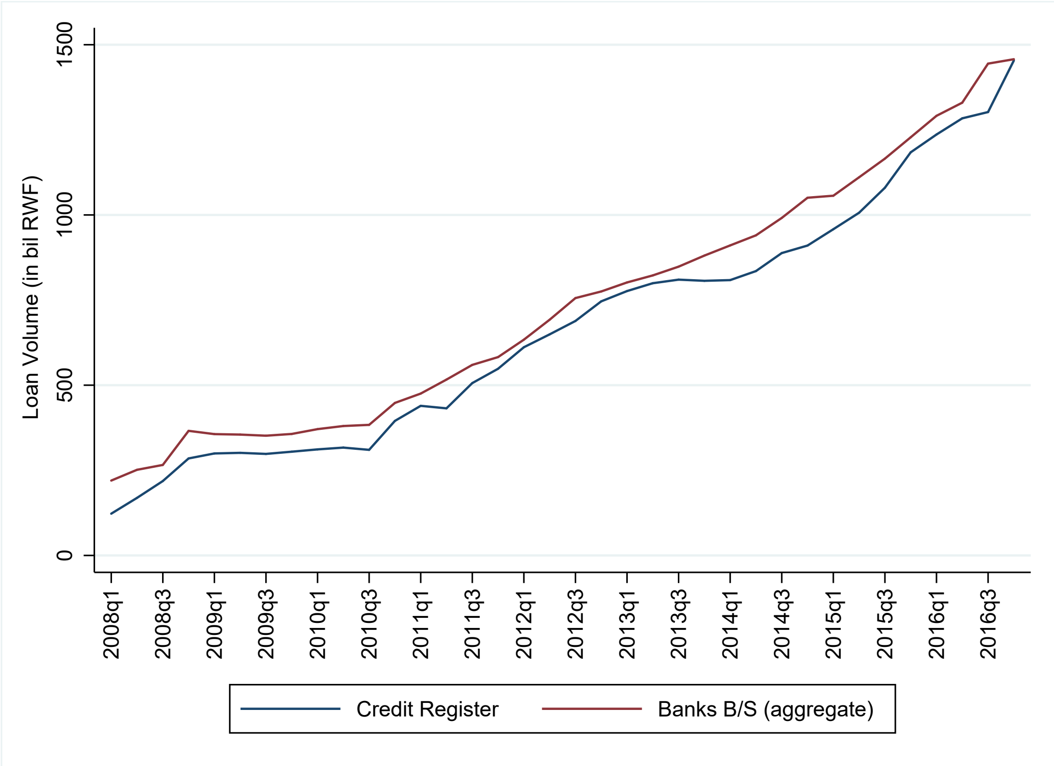
Our study employs detailed loan-level data across all credit institutions operating in Rwanda. The country has a well-functioning and comprehensive credit register that is maintained by the Credit Reference Bureau (CRB), a private credit bureau solutions provider with operations across Africa, under the supervision of the National Bank of Rwanda.[14] The credit register collects data on loans granted by deposit-taking institutions supervised by the central bank, including commercial banks, U-SACCOs, and other MFIs. Reporting institutions provide loan-level information on a monthly basis with no threshold for loan size. Our period of analysis is January 2008 to December 2016. The credit register is highly representative of total banking sector loans, as shown in Figure 3.1.[15]

In our analysis we consider all loans to individuals. We have 4.06 million observations on bank-borrower loan exposures on a monthly basis. For each loan we also know the amount in arrears, the borrower's location (municipality and district) and other

[14] The original provider was a subsidiary of CRB Africa and was taken over in 2015 by TransUnion Africa Holdings, an international credit and information management provider.

[15] The figure compares total bank credit in billions of Rwandan francs (RWF) from the credit register with aggregate statistics from bank balance sheets. The latter aggregate figures (i.e., total credit to both individuals and firms) are only available for the 16 active commercial banks operating in Rwanda and at the quarterly frequency. To ensure comparability, we compute the former also using credit to both individuals and firms in each quarter from the same 16 banks.

Figure 3.1: Credit register representativeness



The figure shows total bank credit in billions of Rwandan francs (RWF) from the credit register as compared to aggregate statistics from bank balance sheets. Data sources: Rwandan Credit Reference Bureau, National Bank of Rwanda.

characteristics such as age, gender, marital status, and sector of employment (government or non-government).[16] After cleaning the data, we have information on the lending activities of banks, U-SACCOs and other MFIs vis-a-vis 177,583 unique individuals residing in 336 municipalities.[17] The borrowers are identified with a unique numerical code which allows us to track their loans over time and across lenders. All loans are extended in local currency.

Summary statistics for the key variables used in the regression analysis are reported in Table 3.1 for the sample of loans with complete information (except interest rates). We show the figures for all financial institutions and separately for U-SACCOs, commercial

[16]The non-government employee category contains all individuals who do not work in the public sector.

[17]This sample covers 11% of total lending to both individuals and firms captured in the credit register and this share is relatively stable over time.

banks, and other MFIs. The average loan balance amounts to 2.8 million Rwandan francs (RFW) (approximately USD 3,250) and the average interest rate on outstanding loans is 18%. U-SACCOs provide smaller, shorter-term, and more expensive loans than other credit institutions. Commercial banks have the highest market share, accounting for 50.1% of all granted loans. U-SACCOs account for 24.7% of loans, and other MFIs for the remaining 25.2%. More than one third of borrowers are female, 23% are younger than 30 years old, and 10% are government employees.

Regarding municipality-specific characteristics, the average share of the working-age individuals (older than 16 years) in a municipality who have an outstanding bank loan before the U-SACCO starts operating in that municipality is 1%. We use this measure to compare the differential impact of the banking expansion program on financial access in regions with varying degrees of ex-ante bank presence. Given that each U-SACCO effectively started its lending activities in a different month, bank presence varies both across municipalities and over time. The median and average share of urban population in a municipality is 0% and 11.8%, respectively, while the average night-time luminosity in the pre-period is 2.6. The latter variable is used to measure economic activity at a national and sub-national level (Henderson, Storeygard, and Weil, 2012; Pinkovskiy and Sala-i Martin, 2016). Yearly data on night-time luminosity comes from satellite images and were obtained from the National Oceanic and Atmospheric Administration (NOAA) of the U.S. Department of Commerce. Finally, when considering the 297 municipalities with at least a U-SACCO, all measures of financial and economic development pre-period are lower than the overall sample averages, confirming that the program targeted rural and less-developed areas.

Table 3.1: Summary statistics

	All financial institutions (n=420)				**U-SACCOs** (n=297)			
	N	Mean	p50	SD	N	Mean	p50	SD
A. Loan Characteristics								
Loan Exposure (RWF mn)	4,060,497	2.839	0.602	17.80	1,001,895	0.574	0.316	1.025
Loan Principal (RWA mn)	4,060,497	4.060	1	23.76	1,001,895	0.854	0.500	1.068
Interest Rate (%)	3,207,401	18.46	17.64	12.69	394,460	24.32	20	21.79
Maturity (months)	4,060,497	28.19	24	25.46	1,001,895	15.86	12	7.134
Relationship Length (months)	4,060,497	17.66	12	17.03	1,001,895	10.82	8	10.23
Loan from Bank	4,060,497	0.501	1	0.500	1,001,895	0	0	0
Loan from other MFI	4,060,497	0.252	0	0.434	1,001,895	0	0	0
Loan from U-SACCO	4,060,497	0.247	0	0.431	1,001,895	1	1	0
B. Borrower characteristics								
Female	177,853	0.377	0	0.485	74,935	0.262	0	0.440
Single	177,853	0.0991	0	0.299	74,935	0.0982	0	0.298
Young	177,853	0.231	0	0.421	74,935	0.209	0	0.407
Government Employee	177,853	0.0985	0	0.298	74,935	0.0671	0	0.250
C. Municipality characteristics								
Bank Presence	336	0.0101	0.00636	0.0127	297	0.00730	0.00563	0.00695
Share of Urban Population	336	0.118	0	0.261	297	0.0948	0	0.234
Nightlights	336	2.644	0	9.754	297	2.270	0	9.277

	Commercial Banks (n=16)				**Other MFIs** (n=107)			
	N	Mean	p50	SD	N	Mean	p50	SD
A. Loan Characteristics								
Loan Exposure (RWF mn)	2,033,512	4.658	0.943	24.90	1,025,090	1.442	0.534	3.235
Loan Principal (RWA mn)	2,033,512	6.602	1.600	33.24	1,025,090	2.149	0.999	3.980
Interest Rate (%)	1,904,814	18.07	18	9.350	908,127	16.74	12.70	12.70
Maturity (months)	2,033,512	36.20	36	29.87	1,025,090	24.35	23.34	21.38
Relationship Length (months)	2,033,512	19.31	15	17.63	1,025,090	21.09	15	19.14
Loan from Bank	2,033,512	1	1	0	1,025,090	0	0	0
Loan from other MFI	2,033,512	0	0	0	1,025,090	1	1	0
Loan from U-SACCO	2,033,512	0	0	0	1,025,090	0	0	0
B. Borrower characteristics								
Female	87,021	0.452	0	0.498	43,693	0.391	0	0.488
Single	87,021	0.108	0	0.310	43,693	0.110	0	0.312
Young	87,021	0.248	0	0.432	43,693	0.226	0	0.418
Government Employee	87,021	0.0759	0	0.265	43,693	0.310	0	0.462
C. Municipality characteristics								
Bank Presence	336	0.0101	0.00636	0.0127	336	0.0101	0.00636	0.0127
Share of Urban Population	336	0.118	0	0.261	336	0.118	0	0.261
Nightlights	336	2.644	0	9.754	336	2.644	0	9.754

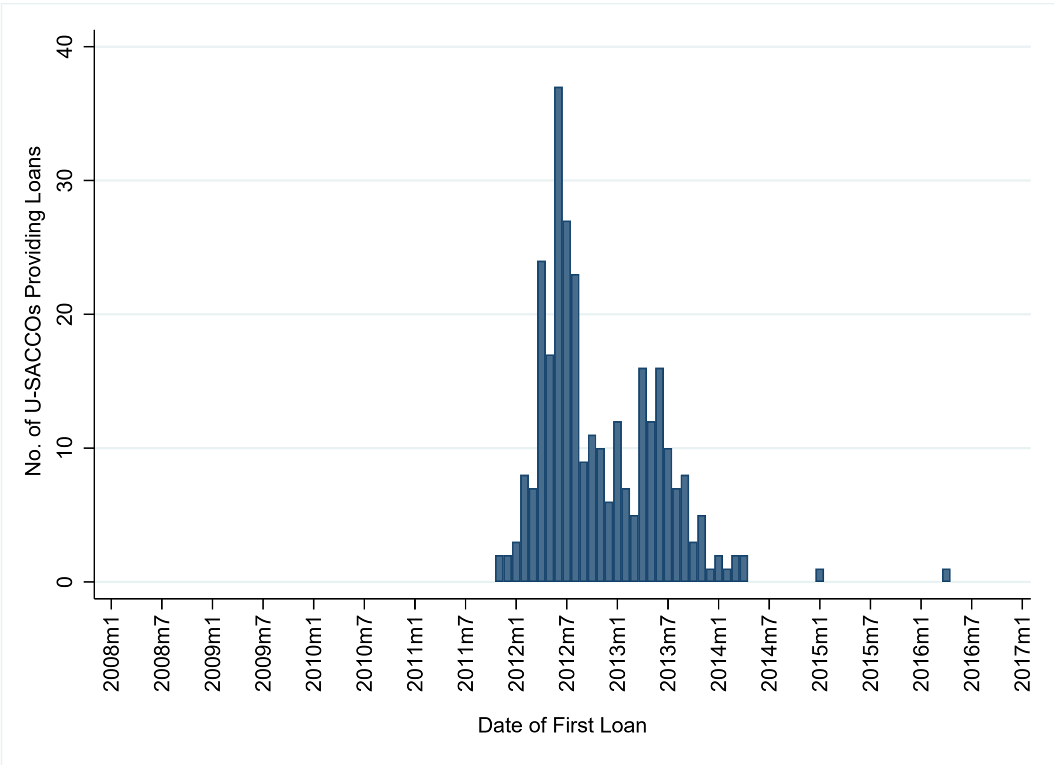
The table presents summary statistics for the main variables in our sample for which all information is available (except interest rates). The sample period is 2008:M1 to 2016:M12 and includes 177,853 unique individuals in 336 municipalities who borrow from commercial banks, U-SACCOs, and other MFIs. Loan exposure and principal amounts are expressed in million of Rwandan franc (RWF). The dummy variable Female is equal to 1 for female borrowers and 0 for male borrowers. The dummy variable Young takes value 1 for individuals below 30 years of age, and 0 otherwise. The Single dummy is equal to 1 for single individuals and 0 for any other marital status. The Government Employee is a dummy equal to 1 for government employees and 0 for any other occupation as well as for those unemployed. Bank Presence is the share of the working-age population (aged 16 and above) with a loan before the program The municipality-specific share of urban population and nightlights are also calculated before the program. Data sources: Rwandan Credit Reference Bureau and National Oceanic and Atmospheric Administration (NOAA).

3.4 The Banking Expansion Program and Access to Credit

3.4.1 Empirical Strategy

We identify the effect of the banking expansion program on access to credit by exploiting its staggered implementation, with different U-SACCOs extending their first loans in different months starting in late 2011. The time-series variation in exposure to the program is illustrated in Figure 3.2, which shows the number of institutions that granted their first loan in a given municipality in each month. U-SACCO started granting loans in 297 out of 336 municipalities during our sample period (i.e., January 2008–December 2016), with the first two extending credit as early as November 2011 and the last one in April 2016.

Figure 3.2: Staggered implementation of the U-SACCO Program



The figure depicts the number of U-SACCOs that granted their first loan during the banking expansion program. Data sources: Rwandan Credit Reference Bureau, National Bank of Rwanda.

As discussed before, data from the FinScope surveys offer suggestive evidence that the banking expansion program coincided with an increase in financial inclusion for the overall population (Appendix Table 3.7). Here we ask if the program had deeper effects on financial access by raising the probability of loan granting for previously unbanked individuals. Using a (balanced) panel dataset at the borrower-municipality-month level, we estimate the following specification:

$$P(Loan_{imt}) = \upsilon + \beta Post\ U\text{-}SACCO_{mt} + \delta' X_i + \alpha_m + \phi_t + \varepsilon_{imt} \tag{3.1}$$

where i denotes the individual, m the municipality and t the year-month.[18] $Loan_{imt}$ is equal to 1 if individual i in municipality m has an outstanding loan with any financial institution at time t, and 0 otherwise. $\mathbf{X}_i$ is a set of time-invariant individual characteristics, including gender, marital status, age, and sector of occupation. Our main variable of interest is the dummy $Post\ U\text{-}SACCO_{mt}$, which is equal to 1 after a U-SACCO starts its lending activities in a given municipality m at time t, and 0 otherwise. Municipality fixed effects α_m control for unobserved spatial unobserved factors—such as credit demand, the degree of urbanization, or economic development—that could be correlated with the timing of U-SACCO openings and with financial access. Time (year:month) fixed effects ϕ_t absorb any common time-varying shock e.g., domestic economic conditions. The identification of β therefore derives from comparing the probability of an individual having an outstanding loan in a municipality before and after the local U-SACCO grants its first loan relative to a control group of individuals living in municipalities where a U-SACCO did not yet start granting loans. In our most demanding specification, we also include municipality-specific time trends to control for the possibility that our results are driven by differences in linear time trends across municipalities. We estimate equation 3.1 as a linear probability model and standard errors are clustered at the municipality level.

[18] We also examine the robustness of our results when structuring our balanced panel with a different time dimension, collapsing our data at a yearly or quarterly frequency.

3.4.2 Baseline Results

The baseline results reported in Table 3.2 show a large positive impact of the U-SACCO program on the probability that an individual obtains a loan. The first three columns refer to loans granted by all institutions (U-SACCOs, commercial banks, and other MFIs) and report results adding sequentially municipality and time (year:month) fixed effects (column 1), borrower controls including gender, marital status, age and employment status (column 2), and municipality-specific time trends (column 3). The coefficient β is precisely estimated across specifications and the point estimate becomes larger when including municipality-specific time trends where the effect is identified by a deviation from a trend that differs for each municipality. Specifically, the results indicate that the U-SACCO program significantly increased the probability of an individual having an outstanding loan by 3.7 percentage points. This effect is economically significant, given that the average share of individuals with an outstanding loan in the pre-program period is 9.6 percent. The set of control variables also indicates that male, single and older individuals as well as government employees are more likely to have access to credit.

To rule out the possibility of potential anticipation effects which could undermine our identification strategy, we explore the dynamic effects of the U-SACCO program during the sample period. In detail, we split the β coefficient by time elapsed before and after the implementation of the program, considering intervals of one, two, and more than two years before and after program implementation. The estimated coefficients and associated confidence intervals are plotted in Figure 3.3. The estimates show that the likelihood of having a loan is higher after the program and rises over time. The increasing magnitude of the effect over time suggests that the program had sustained effects on financial access as opposed to a one-off (transitory) effect. The chart also confirms that the parallel trends assumption is likely to be satisfied in our setting, since the point estimates before the program are close to zero and statistically not significant, although precisely estimated.[19]

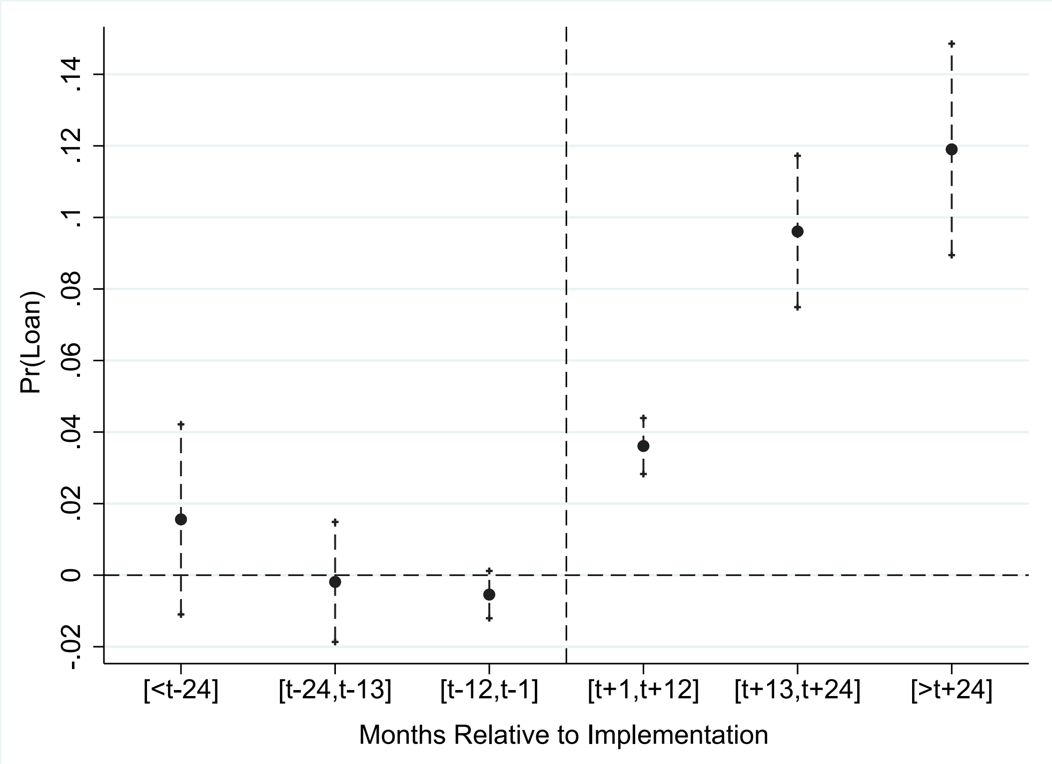
[19] Column 1 of Appendix Table 3.1 shows the estimated coefficients illustrated in Figure 3.3.

Table 3.2: Impact of the U-SACCO program on access to credit – baseline results

	Dummy =1 if individual has a loan with:					
	Any Institution			U-SACCO	Bank	Other MFI
	(1)	(2)	(3)	(4)	(5)	(6)
Post U-SACCO	0.0283***	0.0283***	0.0374***	0.0370***	0.00479	0.00102
	(0.00547)	(0.00547)	(0.00630)	(0.00457)	(0.00349)	(0.00188)
Female		-0.0345***	-0.0345***	-0.0214***	-0.0179***	0.00149
		(0.00159)	(0.00159)	(0.00151)	(0.00250)	(0.000962)
Single		0.0220***	0.0220***	0.00495***	0.0223***	0.000342
		(0.00255)	(0.00255)	(0.00114)	(0.00249)	(0.00117)
Young		-0.0365***	-0.0365***	-0.00447***	-0.0183***	-0.0166***
		(0.00299)	(0.00299)	(0.000663)	(0.00312)	(0.000753)
Government Employee		0.221***	0.221***	-0.0176***	0.0275***	0.244***
		(0.00468)	(0.00468)	(0.00157)	(0.00360)	(0.00692)
Municipality FE	Y	Y	Y	Y	Y	Y
Time (Year:month) FE	Y	Y	Y	Y	Y	Y
Borrower Controls	N	Y	Y	Y	Y	Y
Municipality Time Trends	N	N	Y	Y	Y	Y
No. Observations	19,208,124	19,208,124	19,208,124	19,208,124	19,208,124	19,208,124
No. Municipalities	336	336	336	336	336	336
No. Individuals	177,853	177,853	177,853	177,853	177,853	177,853
Adjusted R^2	0.169	0.201	0.206	0.143	0.112	0.155

The table presents OLS estimates of model 3.1. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with: any institutions (columns 1-3) or specifically in U-SACCOs (column 4), commercial banks (column 5) or other MFIs (column 6). *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month and 0 otherwise. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). As indicated in the bottom rows, different specifications include a different set of municipality and time fixed effects, and municipality-specific time trends. The data are at the borrower-municipality-month level. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Figure 3.3: Probability of obtaining a loan, before and after the program



The figure shows the effect of the Umerenge SACCO program on the probability of individual having a loan in any institution (U-SACCO, other MFI or commercial bank) before and after the U-SACCO becomes operative in that municipality by extending its first loan. The chart plots the estimated coefficients and the associated 90 percent confident intervals of the interaction terms between the U-SACCO variable and a set of time dummies, as reported in Appendix Table 3.1, column 1. The vertical line corresponds to the month in which each U-SACCO granted the first loan in the municipality. Data source: Rwandan Credit Reference Bureau.

Given that U-SACCOs were likely competing for clients with existing banks and MFIs, a natural question that emerges from this baseline result is whether the overall effect of the program is driven by U-SACCOs *per se* or by other financial intermediaries due to increased competition in the local financial sector. To investigate this issue, we use our preferred specification with municipality-specific time trends as in column 3 of Table 3.2, but consider loans from U-SACCOs, commercial banks, and other MFIs separately i.e., the dependent variable is a dummy equal to one for individuals that in a given month have a loan with each specific financial institution. The results clearly indicate that the improvement in the availability of credit is driven by U-SACCOs. As in our main specification, we examine the dynamics of the average effect in Figure 3.4.[20] In addition to the absence of any differential trends in the pre-program period, even when looking at the three types of institutions separately, two other important results emerge. First, the main effect is indeed driven by the U-SACCOs, with the likelihood of having an outstanding loan rising in the first two years of the program and then stabilizing at about 10 percentage points higher than in the pre-program period. Second, there are "spillover" effects of the program to commercial banks, which catch up with a lag. In fact, starting in the second year into the program, the probability of obtaining a loan in banks starts increasing up to 3.5 percentage points more than in the pre-program period. In contrast, there is no such effect for other MFIs.

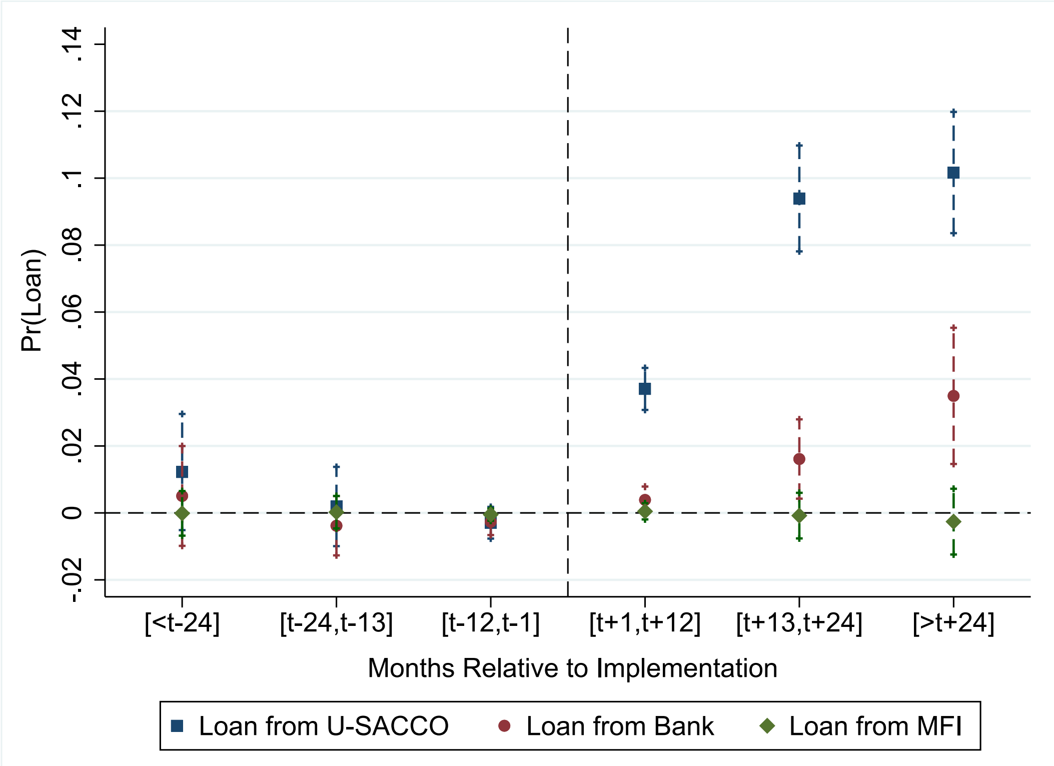
3.4.3 Spatial and Borrower Heterogeneity

Our baseline results point to a significant positive average effect of the banking expansion program on access to credit. However, such effect is likely to differ across municipalities with different levels of financial inclusion, urbanization and economic development prior to the introduction of U-SACCOs. Bruhn and Love (2014), for instance, show that the positive impact of Banco Azteca's opening on employment and income is concentrated in Mexican municipalities that were relatively underserved by the formal banking sector, as measured by bank branch penetration. Agarwal, Alok, Ghosh, Ghosh, Piskorski, and Seru (2017) also show that the JDY financial inclusion program in India increased account openings and bank lending relatively more in regions with lower bank branch presence and a higher share of unbanked households. In the same vein, we expect the increase in access to credit to be larger in areas where financial access was relatively low prior to the Umerenge SACCO program.

To test this hypothesis, we first exploit variation across municipalities according to the (ex-ante) share of individuals with an outstanding bank loan relative to the

[20]Column 2 to 4 of Appendix Table 3.1 shows the estimated coefficients illustrated in Figure 3.4.

Figure 3.4: Probability of obtaining a loan, by institution type, before and after the program



The figure shows the effect of the Umerenge SACCO program on the probability of individual having a loan, separately, in U-SACCO, other MFI and commercial bank, before and after the U-SACCO becomes operative in that municipality by extending its first loan. The chart plots the estimated coefficients and the associated 90 percent confident intervals of the interaction terms between the U-SACCO variable and a set of time dummies, as reported in Appendix Table 3.1, columns 2-4. The vertical line corresponds to the month in which each U-SACCO granted the first loan in the municipality. Data source: Rwandan Credit Reference Bureau.

municipality-specific working-age population. In particular, we follow Bruhn and Love (2014) and split the continuous variable around the 75^{th} percentile of its distribution to create a low (below the 75^{th} percentile) and high (above the 75^{th} percentile) bank presence dummy. We also separate rural from urban municipalities, with the former defined as municipalities where the entire population reside in rural areas in the pre-period, and the latter as municipalities where at least a fraction of the population reside in urban areas. Finally, we identify high- and low-income municipalities based on the night-time luminosity in 2011 (i.e., before the shock), again splitting the continuous variable around the 75^{th} percentile of its distribution in our sample.

The results are reported in Table 3.3. In all cases, we observe that the effects are concentrated in municipalities with lower levels of financial and economic development.

This provides further support to the fact that the impact of the banking expansion program was mostly due to increased access to credit of previously underserved individuals, in line with the mission of U-SACCOs. The coefficient on the interaction between the *Post U-SACCO* dummy and the low bank presence indicator is positive and significant in the overall sample and when considering separately U-SACCOs, but insignificant when examining loans from MFIs or commercial banks. The point estimate is larger that that on the stand-alone *Post U-SACCO* dummy (compare Table 3.3, columns 1-2, with Table 3.2, columns 3-4). Specifically, the probability of having a loan increased by 4.3 percentage points in low bank presence municipalities after the program, a considerable increase given that the average share of individuals with a loan in the pre-program period in low bank presence municipalities was 4.6 percent. The same result holds when isolating rural from urban municipalities, and low-income from high-income municipalities.

We also exploit the richness of our micro-data to analyze heterogeneity of the effect of the program based on borrower characteristics. While the credit register does not collect information on borrower (household) income, consumption, or assets, it has information on the individuals' age, gender, marital status, and sector of employment. We use these dimensions of borrower heterogeneity to further analyze the program impact using a number of dummy variables.

As shown in column (1) of Table 3.4, our results suggest that the program expanded the provision of credit through U-SACCOs mainly to non-government employees. Assuming government employees are relatively more creditworthy borrowers borrowers due to the stability of their labor contracts, this result suggests the program was able to reach out to riskier borrowers who were otherwise unable to obtain loans. Finally, we also find that the U-SACCOs improved access to credit to both young and old borrowers, single and married individuals, as well as males and single females. Married female borrowers, on the other hand, were not affected by the banking expansion program, potentially because credit is contracted by males on behalf of the household.

Table 3.3: Impact of the U-SACCO program on access to credit – spatial heterogeneity

	Dummy =1 if individual has a Loan in			
	Any Institution	U-SACCO	Bank	Other MFI
	(1)	(2)	(3)	(4)
Panel A: Low vs. High Bank Presence Municipalities				
Post U-SACCO x Low Bank Presence	0.0432***	0.0432***	0.00471	0.00154
	(0.00544)	(0.00367)	(0.00362)	(0.00186)
Post U-SACCO x High Bank Presence	0.0100	0.00762	0.00515	-0.00142
	(0.0142)	(0.00993)	(0.00504)	(0.00468)
Panel B: Rural vs. Urban Municipalities				
Post U-SACCO x Rural	0.0417***	0.0421***	0.00437	0.00125
	(0.00550)	(0.00364)	(0.00365)	(0.00188)
Post U-SACCO x Urban	0.0123	0.00723	0.00723	-0.000300
	(0.0158)	(0.0113)	(0.00508)	(0.00506)
Panel C: Low vs. High Development Municipalities				
Post U-SACCO x Low Development	0.0427***	0.0431***	0.00460	0.00128
	(0.00587)	(0.00396)	(0.00371)	(0.00194)
Post U-SACCO x High Development	0.0193*	0.0160*	0.00542	0.000142
	(0.0113)	(0.00866)	(0.00450)	(0.00359)
Municipality FE	Y	Y	Y	Y
Time (Year:month) FE	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y
Municipality Time Trends	Y	Y	Y	Y
No. Observations	19,208,124	19,208,124	19,208,124	19,208,124
No. Municipalities	336	336	336	336
No. Individuals	177,853	177,853	177,853	177,853

The table presents OLS estimates of model 3.1. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with: any institutions (column 1) or specifically in U-SACCOs (column 4), commercial banks (column 5) or other MFIs (column 6). *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month, and 0 otherwise. The coefficient on the *Post U-SACCO* dummy is split across: i) low versus high bank presence municipalities (defined as the share of individuals with a bank loan before the program, Panel A); ii) rural versus urban municipalities (Panel B); and iii) low versus high development municipalities (defined on the basis on night-time luminosity before the program, Panel C). To define low vs high bank presence and luminosity we split the continuous variables around the 75^{th} percentile of the sample distribution. Each regression includes municipality and time fixed effects, and municipality-specific time trends. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). The data are at the borrower-municipality-month level. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau and National Oceanic and Atmospheric Administration (NOAA).

Table 3.4: Impact of the U-SACCO program on access to credit – borrower heterogeneity

	Dummy =1 if individual has a Loan in a U-SACCO				
	(1)	(2)	(3)	(4)	(5)
Post U-SACCO x Government Employee	0.00102				
	(0.00541)				
Post U-SACCO x Non-Gov. Employee	0.0409***				
	(0.00465)				
Post U-SACCO x Young		0.0332***			
		(0.00468)			
Post U-SACCO x Old		0.0399***			
		(0.00459)			
Post U-SACCO x Single			0.0493***		
			(0.00545)		
Post U-SACCO x Married			0.0356***		
			(0.00457)		
Post U-SACCO x Female				0.00388	
				(0.00475)	
Post U-SACCO x Male				0.0564***	
				(0.00466)	
Post U-SACCO x Single Female					0.0199***
					(0.00526)
Post U-SACCO x Married Female					0.00264
					(0.00480)
Post U-SACCO x Single Male					0.0598***
					(0.00574)
Post U-SACCO x Married Male					0.0559***
					(0.00464)
Municipality FE	Y	Y	Y	Y	Y
Time (Year:month) FE	Y	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y	Y
Municipality Time Trends	Y	Y	Y	Y	Y
No. Observations	19,208,124	19,208,124	19,208,124	19,208,124	19,208,124
No. Municipalities	336	336	336	336	336
No. Individuals	177,853	177,853	177,853	177,853	177,853
Adjusted R^2	0.144	0.143	0.143	0.146	0.146

The table presents OLS estimates of model 3.1. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with a U-SACCOs. *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month, and 0 otherwise. The coefficient on the *Post U-SACCO* dummy is split across: i) sector of occupation (using a dummy equal to one for government employees and zero for any other occupation and unemployed); ii) young versus old individuals (using a dummy equal to one for individuals less than 30-year old); iii) marital status (using a dummy equal to one for single individuals and zero for any other status); and iv) gender. Each regression includes municipality and time fixed effects, and municipality-specific time trends. The data are at the borrower-municipality-month level. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

3.4.4 Robustness and Falsification Tests

We conduct several tests to assess the sensitivity of our findings to different aggregations of the data and analyze the external validity of the results using a survey-based dataset. First, we examine the robustness of our results when collapsing our data at a quarterly and yearly frequency, though still setting up the regressions at the borrower-municipality-time level. The results are shown in Appendix Table 3.2 and indicate that the baseline effect of the program as well as the role of U-SACCOs are robust to these alternative ways of setting up the data.

To account for the possibility that some U-SACCOs may be granting loans but not reporting to the credit register, we also test the validity of our results when dropping the 39 (out of 336) municipalities where we never observe a U-SACCO granting a loan over the sample period. As before, our conclusions remain the same (see Appendix Table 3.3).

In a different exercise, we use the cross-sectional individual-level data from the 2012 and 2016 FinScope surveys. The main differences from the baseline analysis is that (i) we only have two time periods, and (ii) borrower location can only be identified at the district (rather than municipality) level. In addition, the surveys may suffer from limitations related to representativeness and reporting bias. The dependent variable is either a dummy taking the value of 1 for survey respondents with a savings account, or for respondents with a savings account *and* a loan from a U-SACCO or a commercial bank. Given that we cannot exploit the staggered implementation of the program due to the nature of the data, we compare changes in access to savings and credit before and after program implementation in districts with higher ex-ante program exposure relative to districts with lower program exposure, as measured by the district-specific share of working age population with a bank loan in the pre-period—see Annex 3.6 for a detailed description of the survey and research design. Appendix Table 3.8 reports the results. The probability of having a savings account and of being granted a loan (conditional on having an account) increased between 2012 and 2016 relatively more for individuals in districts with higher program exposure. Both results are driven by U-SACCOs. These findings confirm our previous micro-level evidence showing that the banking expansion program increased access to finance.

Finally, to help ruling out the possibility that our results are driven by events other than the implementation of the U-SACCO program, we also conduct a falsification test where we randomly assign the treatment across municipalities and over time. Specifically, for each municipality we randomly assign the program implementation date in the interval 2008:M1–2016:M12 and we repeat this exercise 100 times. Appendix Table 3.4 reports the average coefficients of the simulations corresponding to randomized assignments of the *Post U-SACCO* variable across municipalities. The average estimated coefficient is very close to zero and statistically insignificant, suggesting that our main findings are not driven by a spurious correlation between the roll-out of the program and access to credit.

3.4.5 Lending Capacity of U-SACCOs

So far we have focused on the effects of the banking expansion program on credit provision at the extensive margin i.e., to new, mostly first-time borrowers. In this section, we focus on the intensive margin and examine how access to credit evolves once individuals get their first loan at the U-SACCO. Specifically, we analyze how loan size, interest rates and loan maturity change as a function of the length of the bank-borrower relationship, comparing U-SACCOs with other financial institutions. Given that informational opaqueness is likely to be an issue for the majority of individuals in our sample that have no or very limited credit history, we expect that repeated borrowing from the same lender translates into larger loans and better loan terms (Bharath, Dahiya, Saunders, and Srinivasan, 2011). At the same time, we conjecture that U-SACCOs may be at a disadvantage with commercial banks when facing increasing credit demand for credit. In specific, small size and insufficient funding, together with the presence of borrowing limits, might constrain the ability of U-SACCOs to provide larger loans and better terms as the relationship with a specific borrower matures (Cull, Demirgüç-Kunt, and Morduch, 2014).[21]

We formally test this hypothesis by running a set of linear probability models similar to our baseline specification 3.1 but conditional on individuals having an outstanding loan. The dependent variables identify large loans and those with low interest rates and long maturity. In particular, we define: (i) a dummy equal to 1 if loan size is larger than the 75^{th} percentile of the sample distribution, and 0 otherwise; (ii) a dummy equal to 1 if the interest rate on the loan is lower than the 25^{th} percentile of the sample distribution, and 0 otherwise; and (iii) a dummy equal to 1 if the maturity is larger than the 75^{th} percentile of the sample distribution, and 0 otherwise. The right-hand side variables include the standard set of fixed effects, municipality-specific time trends, and borrower characteristics, but it is augmented with a measure of the length of the relationship, measured as the number of months since the first loan in the same financial institution. This variable is then interacted with the Post-U-SACCO dummy to test whether U-SACCOs are more constrained in terms of lending capacity than other financial institutions. The control group includes either loans from commercial banks and other MFIs or exclusively loans from commercial banks.

Table 3.5 shows the likelihood of obtaining large, cheap and long-term loans increases with the length of the relationship between borrowers and financial intermediaries. This result is in line with a large literature stressing the benefit of relationship lending, especially for informationally opaque borrowers (Bharath, Dahiya, Saunders, and Srinivasan, 2011; Boot and Thakor, 1994). However, regardless of the choice of the control group, we find that U-SACCOSs are less likely to grant large, cheap and long-term loans. In fact, the

[21] All MFIs have to meet specific requirements as set up by the National Bank of Rwanda in the regulation of microfinance activity. In particular, "a microfinance institution, union or federation may not grant guarantees or loans, including overdrafts or credit facilities to the same natural person or legal entity or group for an amount exceeding 5% of its total net worth as established in its most recent financial statements. The ceiling is set at a maximum of 10% for savings and credit cooperatives whose non-performing overdue loans are under 5%. In no case may a single loan exceed 2.5% of the total deposits of the micro finance institution."

negative and significant coefficient on the interaction terms indicates that the beneficial effect of relationship length on loan terms is weakened, if not completely offset, in the case of U-SACCOs. For instance, one additional year of relationship with a commercial bank or other MFIs raises the likelihood of obtaining a loan in the top quartile of the distribution of loan size by 4 percent, but this effect reduces to 2.2 percent for U-SACCOs.[22] Similarly, the effect of one additional year on maturity is equal to 10.9 percent for banks and other MFIs, while it is only 3.3 percent for U-SACCOs. Finally, while the length of the bank-borrower relationship is associated with a higher likelihood to obtain a loan in the bottom quartile of the interest rate distribution, this effect becomes not significantly different from zero in the case of U-SACCOs.

Overall, these findings point towards the presence of constraints in the capacity by U-SACCOs to offer larger and better loan terms to their borrowers when the lending relationship matures and informational asymmetries become less binding. This feature could be particularly constraining for entrepreneurs, who might still need to rely on commercial banks for larger or longer-term loans. In fact, this is what we observe in our data, with 4 percent of borrowers that had their first loan at U-SACCOs switching to commercial banks. This figure is more pronounced when isolating first-time borrowers at U-SACCOs that needed and were granted a subsequent loan, with 10 percent of such individuals switching to commercial banks.[23] Thus, in the next section we zoom in on switchers, comparing loan terms between U-SACCOs and commercial banks when borrowers graduate from the microfinance to the formal banking sector.

[22]Considering the estimates reported in column 1 of Table 3.5, $0.00333 \times 12 = 0.039$, while $(0.00333 - 0.00145) \times 12 = 0.022$.

[23]These figures are comparable with previous studies examining loan conditions when firms switch banks (Bonfim, Nogueira, and Ongena, 2017; Ioannidou and Ongena, 2010).

Table 3.5: Impact of the U-SACCO program on access to credit – loan terms

	Dummy =1 if					
	loan amount $> 75^{th}$ percentile		interest rate $< 25^{th}$ percentile		maturity $> 75^{th}$ percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Post U-SACCO x Relationship length	-0.00145***	-0.00195***	-0.00558***	-0.00456***	-0.00634***	-0.00806***
	(0.000196)	(0.000213)	(0.000302)	(0.000294)	(0.000246)	(0.000307)
Post U-SACCO	-0.144***	-0.125***	-0.101***	-0.112***	-0.233***	-0.334***
	(0.00584)	(0.00696)	(0.00881)	(0.00860)	(0.00669)	(0.00931)
Relationship length	0.00333***	0.00396***	0.00448***	0.00279***	0.00907***	0.0102***
	(0.000117)	(0.000145)	(0.000192)	(0.000178)	(0.000134)	(0.000241)
Control Group	Banks & MFIs	Banks	Banks & MFIs	Banks	Banks & MFIs	Banks
Municipality x Time (Year:month) FE	Y	Y	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y	Y	Y
No. Observations	4,058,854	3,033,257	3,205,693	2,296,987	4,058,854	3,033,257
No. Municipalities	336	336	336	336	336	336
No. Individuals	177,853	152,201	149,919	122411	177,853	152201
Adjusted R-squared	0.154	0.207	0.333	0.167	0.273	0.399

The table presents OLS estimates of model 3.1. The dependent variable is (i) a dummy equal to 1 if loan size is larger than the 75^{th} percentile of the sample distribution, and 0 otherwise (columns 1-2); (ii) a dummy equal to 1 if the interest rate on the loan is lower than the 25^{th} percentile of the sample distribution, and 0 otherwise (columns 3-4); and (iii) a dummy equal to 1 if the maturity is larger than the 75^{th} percentile of the sample distribution, and 0 otherwise (columns 5-6). *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month, and 0 otherwise. Relationship length measured the length of the bank-borrower relationship, in months. Each regression includes municipality and time fixed effects, and municipality-specific time trends. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). The data are at the borrower-municipality-month level, conditional on individuals having an outstanding loan. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

3.5 Transition to the Formal Banking Sector

The first part of our analysis examined the impact of the banking expansion program on financial inclusion. We show that the program increased the probability of borrowers accessing credit, particularly in areas with lower financial and economic development, and directly through the newly set-up U-SACCOs. We also provide evidence that U-SACCOs may face constraints to meet increasing demand and, consistent with this finding, that commercial banks expanded credit after the program with a lag. Together, these results point towards the presence of spillover effects from U-SACCOs to commercial banks. Therefore, in this section we investigate in detail the transition of first-time borrowers (i.e., borrowers than obtained their first loan in a U-SACCO through the program) from the microfinance to the commercial banking sector. Specifically, we examine the characteristics of loans to borrowers who switch from U-SACCOs to commercial banks – loan size, interest rate, and maturity – relative to loans granted to similar borrowers that did not switch and kept borrowing from U-SACCOs, or similar borrowers that were already in the formal banking sector.

3.5.1 Empirical Strategy

Following Ioannidou and Ongena (2010), we define *switching loans* as new loans (i) granted to borrowers who had at least one relationship with a financial institution in the previous 12 months; and (ii) with a financial institution with which the borrower did not have a lending relationship in the previous 12 months. All new loans not satisfying these two conditions are classified as non-switching loans. Using this definition, we identify 2,180 switching loans from first-time borrowers at U-SACCOs to commercial banks, corresponding to 10% of first-time U-SACCO borrowers that were granted more than one loan throughout the sample period.

Ideally, we would like to compare the terms of switching loans (loans to a borrower in a relationship with lender A that switches and takes up a new loan from lender B) with those of loans offered by the previous bank in the same period (lender A). Given that we are unable to observe the loan conditions offered by lender A to such borrowers, we compare switching loans with two alternative groups: (i) new loans granted by U-SACCOs to similar borrowers that do not switch; and (ii) new loans extended by banks to individuals already borrowing from banks.

Loan conditions across switchers (treated) and control borrowers may vary for multiple reasons, including borrower characteristics and economic conditions. To alleviate any concerns that such factors may bias our results, we match loans granted in the same month to borrowers of the same age group, gender, marital and employment status. We also match according to the type of loan i.e., mortgage or another type of loan. Within the set borrowers matched "exactly" on these characteristics, we select the nearest-neighbour of each switching loan based on the loan amount, interest rate and/or maturity, as well as the degree of bank presence prior to the U-SACCO program in the municipality where the borrower resides.[24]

Table 3.6 reports summary statistics for the treatment group and the alternative control groups. Switching loans given by commercial banks to borrowers who switched from U-SACCOs are larger, have lower interest rates and longer maturities compared to new loans given by SACCOs to non-switchers. However, compared to new loans to individuals already at banks, switching loans are on average considerably smaller i.e., RWF 1.678 million (around USD 2,000) vs. RWF 3.324 million considering all new loans from commercial banks, or RWF 6.813 million when taking into account exclusively bank-to-bank switchers.

[24]The results are robust to an alternative matching approach combining exact matching with propensity score matching. In the first step, we "exactly" match loans across treated and control groups granted in the same month. Within this sample of loans, we carry out one-to-one propensity score matching procedure that incorporates the same set of borrower, loan and municipality-level characteristics as in our baseline specification—see Appendix Tables 3.5 and 3.6.

Table 3.6: Analysis of switching borrowers – treatment vs. control groups

	Switching Loans (U-SACCO → Banks) (n=2,154)	New Loans from U-SACCOs to Non-Switchers (n=57,196)	New Loans from Banks to Non-Switchers to (n=155,971)	New Loans from Banks Bank → Bank Switchers (n=8,125)
	(1)	(2)	(3)	(4)
Loan Amount	1.678	0.667***	3.324***	6.813***
Loan Interest Rate	19.63	25.18***	20.41***	19.45
Loan Maturity	23.16	14.11***	23.74	34.76***
Female Borrower	0.264	0.260	0.424***	0.310***
Single Borrower	0.108	0.098	0.112	0.155***
Young Borrower	0.211	0.224	0.234***	0.205
Government Employee	0.095	0.075***	0.101	0.184***
Mortgage	0.033	0.017***	0.057***	0.087***
NPL - within 1 year	0.045	0.049	0.047	0.083***
NPL - within 1 year	0.071	0.060*	0.078	0.147***
NPL - until maturity	0.073	0.064	0.090***	0.166***

The table presents average loan and borrower characteristics for the treatment (column 1) and control groups (columns 2-4) in the analysis of switching loans described in Section 3.5. Column 1 refers to borrowers who switch from a U-SACCO to a commercial bank, column 2 to U-SACCO borrowers who do not switch, column 3 to all commercial bank borrowers, and column 4 to commercial bank borrowers who switch from another commercial bank. The dataset captures new loans. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ for two-sided t-tests of equality of means between the treatment and control group. Data sources: Rwandan Credit Reference Bureau.

3.5.2 Results for Switching Loans

We first examine the loan terms of switching loans (new loans to borrowers who switch from the first-time loan at U-SACCOs to a commercial bank) compared to similar borrowers who did not switch and obtained similar loans from any U-SACCOs in the same month. As shown in columns 1-3 of Table 3.7, switchers obtain larger, cheaper, and longer-term loans relative to non-switchers at U-SACCOs. These effects are economically sizable. The coefficient magnitudes suggest that switching loans are on average larger by RWF 0.396 million, cheaper by 422 basis points, and their maturity is longer by almost 6.6 months. Our conclusions do not change when considering similar loans by similar borrowers granted on the same month by the *same* U-SACCO the switcher switched from—see columns 4-6 of Table 3.7.

Next, we compare switching borrowers from U-SACCOs to commercial banks with new loans granted by the same bank in the same month to borrowers that were already in the formal banking sector. Table 3.8 reports the results. Switching loans have similar interest rates and maturities than those granted to the control group, but considerably smaller principal amounts. Specifically, the coefficient in column 1 suggests that switching loans are on average smaller by RWF 0.470 million.

A potential concern is that the latter result can be driven by differences in relationship length between switchers and individuals already borrowing from the destination bank of the switcher, given that loan conditions tend to improve as the bank-borrower relationship matures. Thus, we also consider a more narrowly specified control group defined as new loans by the same bank in the same month to borrowers who were already in the formal banking system, but that switched from another commercial bank in the same period (i.e., U-SACCO-to-bank b switchers vs. bank j-to-bank b switchers). As shown in columns 4-6 of Table 3.8, our results are qualitatively the same and, if anything, they are stronger.

Table 3.7: Analysis of switching borrowers – comparison with U-SACCOs

	Control Group: New loans by all U-SACCOs to non-switcher borrowers in the same month			Control Group: New loans by inside U-SACCO to non-switcher borrowers in the same month		
	Loan Amount	Interest Rate	Loan Maturity	Loan Amount	Interest Rate	Loan Maturity
	(1)	(2)	(3)	(4)	(5)	(6)
Switching Loan − Other Loans	0.396***	-4.224***	6.596***	0.484***	-3.283**	3.481***
(with matching)	(0.111)	(0.679)	(0.446)	(0.177)	(1.514)	(0.795)
Switching Loan − Other Loans	1.010***	-5.548***	9.052***	1.038***	-5.158***	7.375***
(without matching)	(0.023)	(0.456)	(0.170)	(0.034)	(0.782)	(0.270)
Matching Variables:						
Year:Month of Loan Initiation	Y	Y	Y	Y	Y	Y
U-SACCOs	Y	Y	Y			
Inside U-SACCO				Y	Y	Y
Young Borrower	Y	Y	Y	Y	Y	Y
Female Borrower	Y	Y	Y	Y	Y	Y
Single Borrower	Y	Y	Y	Y	Y	Y
Government Employee	Y	Y	Y	Y	Y	Y
Mortgage	Y	Y	Y	Y	Y	Y
Bank Presence	Y	Y	Y	Y	Y	Y
Loan Amount		Y	Y		Y	Y
Loan Interest Rate	Y		Y	Y		Y
Loan Maturity	Y	Y		Y	Y	
No. Switchers (Treated)	2,154	2,154	2,154	729	729	729
No. Untreated Borrowers	57,196	57,196	57,196	57,196	57,196	57,196

The table presents the OLS estimates of a regression in which the dependent variable is, alternatively, loan size, interest rate, and maturity (as indicated in column headings) and the explanatory variable is an indicator for switching loans. The comparison between switching and other loans is done with "exact matching" on lender, year:month, and borrower and loan characteristics, as listed at the bottom of the table. The dataset captures new loans. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Table 3.8: Analysis of switching borrowers – comparison with commercial banks

	Control Group: New loans by the same commercial bank the U-SACCO borrower switched to			Control Group: New loans to bank-to-bank switchers by the same commercial bank the U-SACCO borrower switched to		
	Loan Amount (1)	Interest Rate (2)	Loan Maturity (3)	Loan Amount (4)	Interest Rate (5)	Loan Maturity (6)
Switching Loan – Other Loans	-0.470***	-0.072	-0.268	-1.505***	0.133	-0.662
(with matching)	(0.152)	(0.250)	(0.387)	(0.362)	(0.407)	(0.808)
Switching Loan – Other Loans	-1.647**	-0.779***	-0.578	-5.268***	0.681***	-13.664***
(without matching)	(0.543)	(0.228)	(0.533)	(0.561)	(0.264)	(0.864)
Matching Variables:						
Year:Month of Loan Initiation	Y	Y	Y	Y	Y	Y
Outside Bank	Y	Y	Y	Y	Y	Y
Switcher				Y	Y	Y
Young Borrower	Y	Y	Y	Y	Y	Y
Female Borrower	Y	Y	Y	Y	Y	Y
Single Borrower	Y	Y	Y	Y	Y	Y
Government Employee	Y	Y	Y	Y	Y	Y
Mortgage	Y	Y	Y	Y	Y	Y
Bank Presence	Y	Y	Y	Y	Y	Y
Loan Amount		Y	Y		Y	Y
Loan Interest Rate	Y		Y	Y		Y
Loan Maturity	Y	Y		Y	Y	
No. Switchers (Treated)	2,154	2,154	2,154	1,751	1,751	1,751
No. Untreated Borrowers	155,971	155,971	155,971	8,125	8,125	8,125

The table presents the OLS estimates of a regression in which the dependent variable is, alternatively, loan size, interest rate, and maturity (as indicated in column headings) and the explanatory variable is an indicator for switching loans. The comparison between switching and other loans is done with "exact matching" on lender, year:month, and borrower and loan characteristics, as listed at the bottom of the table. The dataset captures new loans. Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Data sources: Rwandan Credit Reference Bureau.

A key question when analyzing switching is borrower riskiness.[25] On the one hand, if those who transition from MFIs to commercial banks are on average riskier than the standard borrower, the expansion of access to credit could affect asset quality of commercial banks, possibly threatening financial stability. On the other hand, commercial banks could take advantage of the screening role played by the microfinance sector and be able to select the most creditworthy individuals from the pool of U-SACCOs borrowers. While we do not have *ex-ante* measured of riskiness, we can still test for these alternative hypotheses using *ex-post* loan outcomes. Following the definition applied by the National Bank of Rwanda, which corresponds to the standard definition in the literature, we consider a loan as non-performing if it goes into arrears for more than 90 days. We consider 3 different windows: arrears emerging within one year from loan origination, within two years, or any time until maturity. The comparison of switching and non-switching loans reported in Table 3.9 clearly shows that the switching loans are less likely to become non-performing than similar loans extended by U-SACCOs (columns 1-3). In addition, the switching loans are not riskier than similar loans granted by commercial banks (columns 4-6).

Overall, these results support the hypothesis that borrowers switching from U-SACCOs to commercial banks have a demand for credit that cannot be met by U-SACCOs. When able to switch and get credit from banks, they get larger and longer-term loans than those that they would be able to obtain from a U-SACCO. At the same time, commercial banks seem to engage in "cream-skimming" behavior when they select new clients from the U-SACCO borrower pool, as they lend to low-risk borrowers with better *ex-post* loan performance.

[25] Both academics and policymakers highlight the pervasive issues associated with a rapid expansion of microfinance and excessive credit provision (e.g., Banerjee, 2013; Zinman, 2014). Chen, Rasmussen, and Reille (2010) document that NPLs reached 7% in Bosnia-Herzegovina, 10% in Morocco, 12% in Nicaragua and 13% in Pakistan in 2009. Most prominently, the state of Andhra Pradesh in India saw a major crisis in the MFI sector in 2010 following a rapid expansion of the microcredit industry. The characteristics of the crisis resemble those of a classical credit boom and bust cycle, where the high growth and profitability of Indian MFIs led to excessive borrowing and indebtedness among low-income clients (Beck, 2015).

Table 3.9: Analysis of switching borrowers – non-performing loans

	Control Group: New loans by all U-SACCOs to non-switcher borrowers in the same month			Control Group: New loans by the same commercial bank the U-SACCO borrower switched to		
NPL definition:	within 1 year (1)	within 2 years (2)	until maturity (3)	within 1 year (4)	within 2 years (5)	until maturity (6)
Switching Loan − Other Loans (with matching)	-0.005 (0.009)	-0.029*** (0.011)	-0.036*** (0.011)	-0.005 (0.006)	-0.006 (0.008)	-0.009 (0.008)
Switching Loan − Other Loans (without matching)	-0.004 (0.005)	0.010** (0.005)	0.009* (0.005)	-0.002 (0.005)	-0.007 (0.006)	-0.017*** (0.006)
Matching Variables:						
Year:Month of Loan Initiation	Y	Y	Y	Y	Y	Y
U-SACCOs	Y	Y	Y			
Outside Commercial Bank				Y	Y	Y
Young Borrower	Y	Y	Y	Y	Y	Y
Female Borrower	Y	Y	Y	Y	Y	Y
Single Borrower	Y	Y	Y	Y	Y	Y
Government Employee	Y	Y	Y	Y	Y	Y
Mortgage	Y	Y	Y	Y	Y	Y
Degree of Bank Presence	Y	Y	Y	Y	Y	Y
Loan Amount	Y	Y	Y	Y	Y	Y
Loan Interest Rate	Y	Y	Y	Y	Y	Y
Loan Maturity	Y	Y	Y	Y	Y	Y
No. Switchers (Treated)	2,154	2,154	2,154	2,154	2,154	2,154
No. Untreated Borrowers	57,196	57,196	57,196	155,971	155,971	155,971

The table presents the OLS estimates of a regression in which the dependent variable is the probability that the a loan becomes an NPL (according to three alternative windows as indicated in column headings) and the explanatory variable is an indicator for switching loans. The comparison between switching and other loans is done with "exact matching" on lender, year:month, and borrower and loan characteristics, as listed at the bottom of the table. The dataset captures new loans. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

3.5.3 Results for Post-Switching Loans

To further analyze the transition of SACCO borrowers to the formal banking sector, we also exploit the time dimension of the credit register and analyze *subsequent* loans that switching borrowers obtain from their new commercial bank. Using a similar approach to the previous section, we compare the terms of all subsequent loans granted to a U-SACCO-to-bank switcher with the terms of the first loan granted to the same switcher by the same commercial bank. Formally, we do an exact matching of the loans within the borrower and the bank. Subsequent loans are grouped into buckets depending on the date the loan was granted (less than 6 months, 7 to 12 months, 13 to 24 months, and more than 24 months after the first loan). In this way, we are able to tease out the effect of credit history on subsequent loan conditions.

The results reported in Table 3.10 show that loans gradually become larger as the length of the relationship between the switcher and the bank increases (Panel A). In specific, the coefficient estimates indicate that loans granted more than two years after switching are larger by RWA 0.452 million than the original switching loan. We also find evidence that the maturity of loans improves over time, though this positive effect dissipates, on average, after two years (Panel C). Finally, we observe no relative difference between the interest rate spread (interest rate minus the repo rate) charged on the initial and subsequent loans (Panel B).

3.6 Conclusion

We exploit the staggered implementation of a large scale microfinance expansion program and trace its effects on financial access as well as on the transition of previously unbanked individuals to the formal banking sector. Our data come from a large supervisory dataset comprising the universe of individual loans granted by all financial institutions in Rwanda between 2008 and 2016. The use of loan-level data from a credit register allows us to overcome power issues that are common in randomized evaluations (Banerjee, Karlan, and Zinman, 2015; Ravallion, 2009). In this respect, our approach complements the evidence

Table 3.10: Analysis of switching borrowers – subsequent loans

Time since the switching loan:	1 to 6 months	7 to 12 months	12 to 24 months	24+ months
Panel A: Loan Amount				
New Loan − Original Switching Loan	0.015	0.011	0.098***	0.452***
	(0.016)	(0.028)	(0.034)	(0.133)
No. Switching Loans	2,154	2,154	2,154	2,154
No. Future Loans of Switchers	10,980	7,519	9,843	4,519
Panel B: Interest Rate Spread				
New Loan − Original Switching Loan	-0.155	-0.349	-0.375	-0.616
	(0.160)	(0.260)	(0.356)	(0.654)
No. Switching Loans	2,154	2,154	2,154	2,154
No. Future Loans of Switchers	10,980	7,519	9,843	4,519
Panel C: Loan Maturity				
New Loan − Original Switching Loan	0.060	0.336*	0.891***	0.390
	(0.078)	(0.181)	(0.280)	(0.715)
No. Switching Loans	2,154	2,154	2,154	2,154
No. Future Loans of Switchers	10,980	7,519	9,843	4,519

The table presents the OLS estimates of a regression of loan characteristics—Amount (Panel A); Interest rate (Panel B); and Maturity (Panel C)—on an indicator for on subsequent loans that the borrower switching from a U-SACCO obtain from a commercial bank, grouped in buckets depending on the time elapsed since the first loan, as indicated in column headings. The comparison between additional and the original loans is done for the same lender-borrower pair. The dataset captures new loans. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

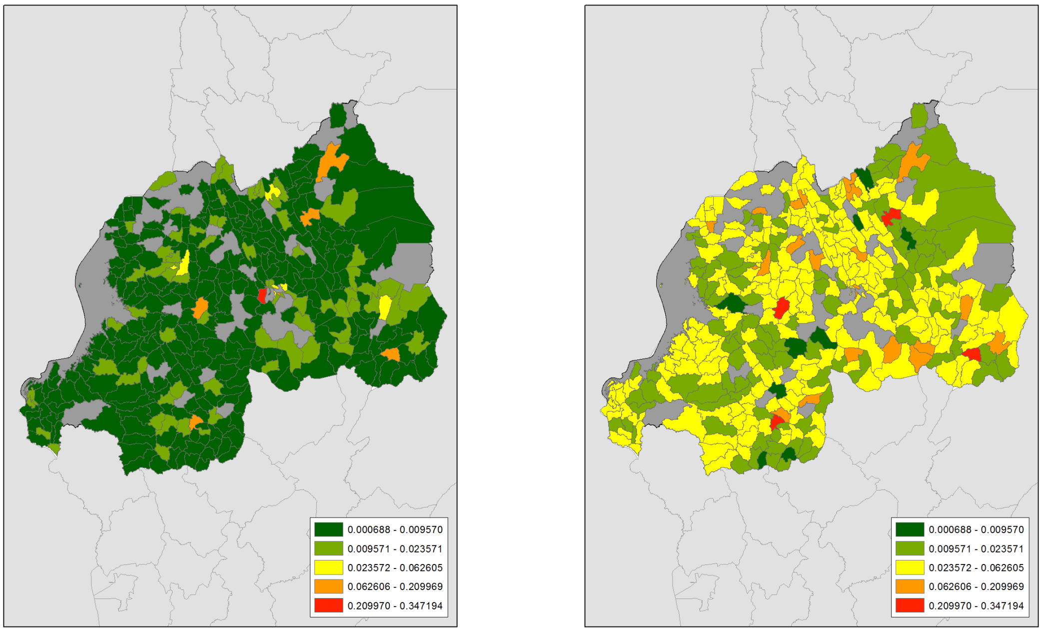
from RCTs, providing evidence of the aggregate effects of the microfinance expansion program, including the spillover effects to the formal banking sector. We show that the program raised the likelihood of access to bank loans for the previously unbanked population, especially in rural and less financially developed municipalities. Thanks to the availability of loan-level data, we can also show that the overall effect is driven by the newly set-up U-SACCOs, although we also observe a catching up of commercial banks about one year after the roll-out of the program.

The latter result is consistent with the presence of a significant share of first-time borrowers who, when in need of a second loan, switch to commercial banks, where they can obtain larger loans and better loan terms than what they were getting from U-SACCOs. Additional evidence suggests that U-SACCOs are not as able as commercial banks to satisfy the borrowing needs of their costumers as the relationship matures, which thus have incentives to switch to formal banking services.

Our analysis supports the notion that microfinance institutions which target low-income individuals have an important screening and signaling role. Commercial banks can expand their lending activity cream-skimming low-risk borrowers from MFIs by offering more attractive loan terms. Our findings suggest that the microfinance sector, coupled with well-functioning credit reference bureaus, can mitigate informational frictions in the credit market and play a crucial role for financial development.

Appendix 3.A. Additional Results

Appendix Figure 3.1: Share of Individuals with a Loan Before and After the Umerenge SACCO Program



The figure depicts the share of individuals with an outstanding loan over total adult population, by municipality, before (left figure) and after (right figure) the banking expansion program. Data sources: Rwandan Credit Reference Bureau, National Bank of Rwanda.

Appendix Table 3.1: Impact of the U-SACCO Program on Access to Credit – Effects over time

	Dummy =1 if individual has a loan in:			
	Any Institution	U-SACCO	Bank	Other MFI
	(1)	(2)	(3)	(4)
Post U-SACCO [< t-24]	0.0156	0.0122	0.00507	-0.000118
	(0.0135)	(0.00882)	(0.00758)	(0.00339)
Post U-SACCO [t-13,t-24]	-0.00189	0.00191	-0.00383	0.000230
	(0.00853)	(0.00602)	(0.00450)	(0.00246)
Post U-SACCO [t-1,t-12]	-0.00542	-0.00292	-0.00265	-0.000490
	(0.00336)	(0.00241)	(0.00201)	(0.000933)
Post U-SACCO [t+1,t+12]	0.0361***	0.0370***	0.00389*	0.000474
	(0.00398)	(0.00319)	(0.00203)	(0.00123)
Post U-SACCO [t+13,t+24]	0.0961***	0.0939***	0.0161***	-0.000806
	(0.0108)	(0.00804)	(0.00602)	(0.00346)
Post U-SACCO [> t+24]	0.119***	0.102***	0.0350***	-0.00260
	(0.0150)	(0.00920)	(0.0103)	(0.00499)
Municipality FE	Y	Y	Y	Y
Time (Year:month) FE	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y
Municipality Time Trends	Y	Y	Y	Y
No. Observations	19,208,124	19,208,124	19,208,124	19,208,124
No. Municipalities	336	336	336	336
No. Individuals	177,853	177,853	177,853	177,853
Adjusted R^2	0.207	0.146	0.112	0.155

The table presents OLS estimates of model 3.1. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with: any institutions (column 1) or specifically in U-SACCOs (column 4), commercial banks (column 5) or other MFIs (column 6). *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month and 0 otherwise. The coefficient on the *Post U-SACCO* dummy is split by time elapsed before and after program implementation, using six dummies equal to 1 for: i) more than 2 years before the program, ii) two years before the program; iii) one year before the program; iv) one year after the program; v) two years after the program, and vi) more than 2 years after the program. Each regression includes municipality and time fixed effects, and municipality-specific time trends. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). The data are at the borrower-municipality-month level. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Appendix Table 3.2: Impact of the U-SACCO Program on Access to Credit – Alternative Data Structure

Data frequency:	**Quarterly**				**Yearly**			
	Dummy =1 if individual has a loan in:							
	Any Inst.	U-SACCO	Bank	Other MFI	Any Inst.	U-SACCO	Bank	Other MFI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post U-SACCO	0.0366***	0.0374***	0.00359	0.000976	0.0337***	0.0414***	-0.000769	-0.000592
	(0.00614)	(0.00463)	(0.00341)	(0.00189)	(0.00754)	(0.00524)	(0.00424)	(0.00215)
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Time (Year:quarter) FE	Y	Y	Y	Y	N	N	N	N
Time (Year) FE	N	N	N	N	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Time Trends	Y	Y	Y	Y	Y	Y	Y	Y
No. Observations	6,402,708	6,402,708	6,402,708	6,402,708	1,600,677	1,600,677	1,600,677	1,600,677
No. Municipalities	336	336	336	336	336	336	336	336
No. Individuals	177,853	177,853	177,853	177,853	177,853	177,853	177,853	177,853
Adjusted R^2	0.204	0.145	0.110	0.153	0.191	0.154	0.100	0.142

The table presents OLS estimates of model 3.1 collapsing the original dataset at the borrower-municipality-month level at a quarterly (columns 1-4) or yearly (columns 5-8) frequency. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with: any institutions (columns 1 and 5) or specifically in U-SACCOs (columns 2 and 6), commercial banks (columns 3 and 7) or other MFIs (columns 4 and 8). *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month and 0 otherwise. Each regression includes municipality and time fixed effects, and municipality-specific time trends. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Data sources: Rwandan Credit Reference Bureau.

Appendix Table 3.3: Impact of the U-SACCO Program on Access to Credit – Excluding municipalities where U-SACCOs never operated

	Dummy =1 if individual has a loan in							
	Any Institution		U-SACCO		Bank		Other MFI	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post U-SACCO	0.0305***		0.0326***		0.00229		0.000285	
	(0.00696)		(0.00508)		(0.00359)		(0.00167)	
Post U-SACCO [< t-24]		-0.00511		0.00130		-0.00810		0.00231
		(0.0186)		(0.0139)		(0.00776)		(0.00344)
Post U-SACCO [t-13,t-24]		-0.00616		-0.00191		-0.00627		0.00182
		(0.0125)		(0.00941)		(0.00547)		(0.00255)
Post U-SACCO [t-1,t-12]		-0.00478		-0.00324		-0.00215		-6.09e-05
		(0.00463)		(0.00352)		(0.00235)		(0.000909)
Post U-SACCO [t+1,t+12]		0.0324***		0.0349***		0.00184		0.000852
		(0.00449)		(0.00365)		(0.00199)		(0.00117)
Post U-SACCO [t+13,t+24]		0.0817***		0.0834***		0.00837*		0.00269
		(0.0107)		(0.00871)		(0.00493)		(0.00305)
Post U-SACCO [> t+24]		0.0867***		0.0790***		0.0164**		0.00551
		(0.0125)		(0.00999)		(0.00719)		(0.00435)
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Time (Year:month) FE	Y	Y	Y	Y	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Time Trends	Y	Y	Y	Y	Y	Y	Y	Y
No. Observations	16,909,020	16,909,020	16,909,020	16,909,020	16,909,020	16,909,020	16,909,020	16,909,020
No. Municipalities	297	297	297	297	297	297	297	297
No. Individuals	156,565	156,565	156,565	156,565	156,565	156,565	156,565	156,565
Adjusted R^2	0.213	0.213	0.139	0.140	0.101	0.101	0.164	0.164

The table presents OLS estimates of model 3.1. The sample exclude the 39 municipalities where no U-SACCO granted any loan during the sample period. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with: any institutions (columns 1-2) or specifically in U-SACCOs (columns 3-4), commercial banks (columns 5-6) or other MFIs (columns 7-8). *Post U-SACCO* is a dummy equal to 1 after a U-SACCO starts its lending activities in a given municipality and month and 0 otherwise. In odds columns, the coefficient on the *Post U-SACCO* dummy is split by time elapsed before and after program implementation, using six dummies equal to 1 for: i) more than 2 years before the program, ii) two years before the program; iii) one year before the program; iv) one year after the program; v) two years after the program, and vi) more than 2 years after the program. Each regression includes municipality and time fixed effects, and municipality-specific time trends. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). The data are at the borrower-municipality-month level. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Appendix Table 3.4: Impact of the U-SACCO Program on Access to Credit – Falsification Tests

	Dummy =1 if individual has a Loan in:			
	Any Institution	U-SACCO	Bank	Other MFI
	(1)	(2)	(3)	(4)
Post U-SACCO	0.0004	-0.0005	-0.00004	0.00034
	(0.00636)	(0.00391)	(0.00367)	(0.00177)
Female	-0.0345***	-0.0214***	-0.0179***	0.00149
	(0.00159)	(0.00151)	(0.00250)	(0.000962)
Single	0.0220***	0.00495***	0.0223***	0.000342
	(0.00255)	(0.00114)	(0.00249)	(0.00117)
Young	-0.0365***	-0.00447***	-0.0183***	-0.0166***
	(0.00299)	(0.000663)	(0.00312)	(0.000753)
Government Employee	0.2214***	-0.0176***	0.0275***	0.244***
	(0.00468)	(0.00157)	(0.00360)	(0.00692)
Municipality FE	Y	Y	Y	Y
Time (Year:month) FE	Y	Y	Y	Y
Borrower Controls	Y	Y	Y	Y
Municipality Time Trends	Y	Y	Y	Y
No. Observations	19,208,124	19,208,124	19,208,124	19,208,124
No. Municipalities	336	336	336	336
No. Individuals	177,853	177,853	177,853	177,853
Adjusted R^2	0.205	0.142	0.112	0.155

The table presents OLS estimates of model 3.1. The dependent variable is a dummy equal to 1 for individuals who, at time t, have an outstanding loan with: any institutions (column 1) or specifically in U-SACCOs (column 4), commercial banks (column 5) or other MFIs (column 6). *Post U-SACCO* is a dummy constructed randomly assign the treatment across municipalities and over time. Specifically, for each municipality we randomly assign the program implementation date in the interval 2008:M1–2016:M12 and we repeat this exercise 100 times. The table reports the average coefficients of the simulation. Each regression includes municipality and time fixed effects, and municipality-specific time trends. Borrower characteristics include a set of dummies for gender (equal to 1 for females and 0 for males), marital status (equal to 1 for single individuals and 0 for any other marital status), young (equal to 1 for individuals less then 30-year old, and 0 otherwise), and sector of occupation (equal to 1 for government employees and 0 for any other occupation as well as for those unemployed). The data are at the borrower-municipality-month level. The sample period is 2008:M1 to 2016:M12. Standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Appendix Table 3.5: Analysis of Switching Borrowers – Comparison with U-SACCOs, Propensity Score Matching

	Control Group: New loans by all U-SACCOs to non-switcher borrowers in the same month			Control Group: New loans by inside U-SACCO to non-switcher borrowers in the same month		
	Loan Amount (1)	Interest Rate (2)	Loan Maturity (3)	Loan Amount (4)	Interest Rate (5)	Loan Maturity (6)
Switching Loan − Other Loans	0.822***	-6.266***	8.643***	0.941***	-9.063***	7.252***
(propensity score matching)	(0.087)	(0.669)	(0.443)	(0.171)	(1.322)	(0.731)
Switching Loan − Other Loans	1.010***	-5.548***	9.052***	1.038***	-5.158***	7.375***
(without matching)	(0.023)	(0.456)	(0.170)	(0.034)	(0.782)	(0.270)
Matching Variables:						
Year:Month of Loan Initiation	Y	Y	Y	Y	Y	Y
U-SACCOs	Y	Y	Y			
Inside U-SACCO				Y	Y	Y
Young Borrower	Y	Y	Y	Y	Y	Y
Female Borrower	Y	Y	Y	Y	Y	Y
Single Borrower	Y	Y	Y	Y	Y	Y
Government Employee	Y	Y	Y	Y	Y	Y
Mortgage	Y	Y	Y	Y	Y	Y
Degree of Bank Presence	Y	Y	Y	Y	Y	Y
Loan Amount		Y	Y		Y	Y
Loan Interest Rate	Y		Y	Y		Y
Loan Maturity	Y	Y		Y	Y	
No. Switchers (Treated)	2,154	2,154	2,154	729	729	729
No. Untreated Borrowers	57,196	57,196	57,196	57,196	57,196	57,196

The table presents the OLS estimates of a regression in which the dependent variable is, alternatively, loan size, interest rate, and maturity (as indicated in column headings) and the explanatory variable is an indicator for switching loans. The comparison between switching and other loans is done with "exact matching" on lender and year:month and propensity score matching on the borrower and loan characteristics listed at the bottom of the table. The dataset captures new loans. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Appendix Table 3.6: Analysis of Switching Borrowers – Comparison with Commercial Banks, Propensity Score Matching

	Control Group: New loans by the same commercial bank the U-SACCO borrower switched to			Control Group: New loans to bank-to-bank switchers by the same commercial bank the U-SACCO borrower switched to		
	Loan Amount	Interest Rate	Loan Maturity	Loan Amount	Interest Rate	Loan Maturity
	(1)	(2)	(3)	(4)	(5)	(6)
Switching Loan − Other Loans	-0.758***	-0.083	-0.266	-1.290***	-0.387	-0.915
(propensity score matching)	(0.250)	(0.345)	(0.629)	(0.454)	(0.562)	(1.080)
Switching Loan − Other Loans	-1.647**	-0.779***	-0.578	-5.268***	0.681***	-13.664***
(without matching)	(0.543)	(0.228)	(0.533)	(0.561)	(0.264)	(0.864)
Matching Variables:						
Year:Month of Loan Initiation	Y	Y	Y	Y	Y	Y
Outside Bank	Y	Y	Y	Y	Y	Y
Switcher				Y	Y	Y
Young Borrower	Y	Y	Y	Y	Y	Y
Female Borrower	Y	Y	Y	Y	Y	Y
Single Borrower	Y	Y	Y	Y	Y	Y
Government Employee	Y	Y	Y	Y	Y	Y
Mortgage	Y	Y	Y	Y	Y	Y
Degree of Bank Presence	Y	Y	Y	Y	Y	Y
Loan Amount		Y	Y		Y	Y
Loan Interest Rate	Y		Y	Y		Y
Loan Maturity	Y	Y		Y	Y	
No. Switchers (Treated)	2,154	2,154	2,154	1,751	1,751	1,751
No. Untreated Borrowers	155,971	155,971	155,971	8,125	8,125	8,125

The table presents the OLS estimates of a regression in which the dependent variable is, alternatively, loan size, interest rate, and maturity (as indicated in column headings) and the explanatory variable is an indicator for switching loans. The comparison between switching and other loans is done with "exact matching" on lender and year:month and propensity score matching on the borrower and loan characteristics listed at the bottom of the table. The dataset captures new loans. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data sources: Rwandan Credit Reference Bureau.

Appendix 3.B. Additional Evidence from Survey Data

We test whether the banking expansion program increased financial access using survey data from the 2012 and the 2016 rounds of the FinScope surveys, run by Access to Finance Rwanda as part of a cross-country project developed by FinMark Trust. The purpose of the FinScope surveys is to describe levels of access to and take-up of financial products and services in the formal and informal financial sector. The microdata for Rwanda are at the district level. Summary statistics on financial inclusion are shown in Appendix Table 3.7.

We employ a different identification strategy than in the baseline analysis given that we have only two cross sections and borrower location is available at the district rather than municipality level. Since we cannot exploit the staggered roll-out of the program across municipalities, we take the 2012 survey data as the pre-program period and the 2016 survey data as the post-program outcome, and compare changes in access to finance before and after the program across districts with a different *ex-ante* exposure to the program.[26] Similar to our main analysis looking at spatial heterogeneity, the variable that captures exposure to the program is *Low Bank Presence* and is defined as the share of working-age individuals without an outstanding bank loan before the program (in the month before each SACCO started its lending operations) and is constructed from the credit register. In all specifications we control for borrower characteristics. We estimate the following specification:

$$Pr(Access)_{idt} = \beta(Low\ Bank\ Presence_d \times Post_t) + \delta' X_i + \alpha_d + \phi_t + \varepsilon_{idt} \tag{3.2}$$

where the dependent variable is alternately the probability that individual i in a district d has a savings account or a bank loan (conditional on a savings account) in year t (where $t = 2012$ or $t = 2016$); α_d are district fixed effects; and ϕ_t are survey fixed effects.

[26] Ideally, we would have used the 2008 survey as baseline, but the microdata is not available. It is important to note, however, that using 2012 as the benchmark will likely underestimate the effects of the program given that its implementation started in 2011.

The results, shown in Appendix Table 3.8, show that the likelihood of individuals having savings and loan accounts is relatively higher in districts with pre-program lower bank presence than in other districts, an effect that is driven by U-SACCOs. The point estimates are close to those in our primary analysis.

Appendix Table 3.7 Descriptive Statistics on the U-SACCO Program and Financial Inclusion – Survey Evidence

	Finscope 2012 (n=6,150)		Finscope 2016 (n=12,480)		Finscope 2012 and 2016 (n=18,630)	
	Mean	SD	Mean	SD	Mean	SD
Savings Account in a:						
Bank, SACCO or MFI	0.319	0.466	0.364	0.481	0.344	0.475
Bank or SACCO	0.305	0.460	0.343	0.475	0.326	0.469
Bank	0.153	0.360	0.120	0.325	0.121	0.326
SACCO	0.192	0.394	0.258	0.438	0.239	0.427
MFI	0.032	0.175	0.044	0.204	0.038	0.192
Loan in a:						
Bank, SACCO or MFI	0.046	0.210	0.081	0.273	0.067	0.250
Bank or SACCO	0.040	0.195	0.067	0.249	0.055	0.228
Bank	0.022	0.145	0.025	0.156	0.022	0.146
SACCO	0.019	0.138	0.044	0.205	0.035	0.183
MFI	0.008	0.088	0.018	0.131	0.014	0.116

The table presents descriptive statistics for two key variables on financial inclusion: an indicator variable for individuals with savings accounts and an indicator variable for individuals with savings and loan accounts. The dataset is repeated cross-sections of borrowers in the 2012 and 2016 FinScope surveys. Source: FinScope Surveys, 2012 and 2016 rounds.

Appendix Table 3.8: Impact of U-SACCO Program on Financial Access – Survey Evidence

Dep. Var.:	Dummy =1 if individual has a savings account in:		
	Bank or SACCO	SACCO	Bank
	(1)	(2)	(3)
Low Bank Presence x Post	1.305	0.902**	0.879
	(0.897)	(0.372)	(0.929)
Post	-1.237	-0.816**	-0.893
	(0.885)	(0.359)	(0.921)
Female	-0.113***	-0.0836***	-0.0474***
	(0.00900)	(0.0104)	(0.00580)
Young	-0.0914***	-0.0681***	-0.0538***
	(0.00838)	(0.00873)	(0.00849)
Single	-0.158***	-0.120***	-0.0602***
	(0.0168)	(0.0102)	(0.0136)
No Formal Education	-0.196***	-0.124***	-0.113***
	(0.0111)	(0.00939)	(0.0124)
District FE	Y	Y	Y
Observations	18,630	18,630	18,630
Adjusted R-squared	0.097	0.064	0.116
Mean Dependent Variable	0.326	0.239	0.121

Dep. Var.:	Conditional on having a savings account, dummy =1 if individual has a loan in:		
	Bank or SACCO	SACCO	Bank
Low Bank Presence x Post	1.296***	1.098***	0.475
	(0.420)	(0.362)	(0.358)
Post	-1.191***	-1.008***	-0.450
	(0.397)	(0.347)	(0.346)
Female	-0.00115	0.00132	-0.00270
	(0.0184)	(0.00833)	(0.0162)
Young	-0.0648***	-0.0375***	-0.0271
	(0.0186)	(0.0110)	(0.0189)
Single	-0.0683***	-0.00973	-0.0679***
	(0.0197)	(0.0151)	(0.0173)
No Formal Education	-0.0469	-0.00445	-0.0530**
	(0.0295)	(0.0188)	(0.0257)
District FE	Y	Y	Y
Observations	2,949	2,949	2,949
Adjusted R-squared	0.037	0.047	0.020
Mean Dependent Variable	0.055	0.035	0.022

The table presents coefficient estimates from a regression of an indicator variable for individuals who have savings accounts (top panel) or loan accounts (bottom panel) on an interaction term between Bank Presence and Post dummy (equal to 1 for the 2016 survey), and borrower characteristics. The dataset is repeated cross-sections of borrowers in the 2012 and 2016 FinScope surveys. Standard errors clustered at the district level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Source: FinScope Surveys, 2012 and 2016 rounds.

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