Banks, Firms, and Households: Credit Shock Amplification and Real Effects

Cédric Huylebroek*
KU Leuven & Norges Bank
Job market paper

Jin Cao†
Norges Bank

Abstract

While a large literature has examined how bank credit shocks affect firms or households, it has not accounted for the fact that such shocks may simultaneously impact both. In this paper, we overcome this limitation and disentangle the real impact of a credit market disruption into the effect of firm-side credit shocks, individual-side credit shocks, and their interaction. To this end, we construct a novel dataset linking Norwegian employees to their employers and their respective bank relationships. We show that individuals' labor income and consumption decline by 1–2% when only they or only their employer face a credit shock, compared to the benchmark where neither do. However, when individuals and their employer simultaneously face a credit shock, labor income and consumption decline by nearly 6%, revealing a strong amplification effect. This amplification arises because personal credit constraints hinder individuals' consumption smoothing and job search when confronted with wage cuts or layoffs triggered by their employer's credit constraints. Our findings suggest that this mechanism also shapes the aggregate transmission of credit shocks.

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Keywords: Credit shocks, Shock amplification, Real effects, Firms, Households, Wages, Consumption

*Email: cedric.huylebroek@kuleuven.be.
†Email: jin.cao@norges-bank.no.

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1. Introduction

A central question in economics is how financial shocks propagate from the banking sector to the real economy. A large body of research has studied this question by analyzing how bank credit shocks affect firms or households and the resulting implications for the broader economy (Bernanke 2018). For example, credit shocks to firms have been shown to lead to reductions in investment and employment (Chodorow-Reich 2014; Huber 2018), while credit shocks to households have been shown to lead to declines in consumption (Benmelech, Meisenzahl, and Ramcharan 2017; Jensen and Johannesen 2017; Mian, Rao, and Sufi 2013).

While prior research has advanced our understanding of how credit shocks affect *either* firms *or* households, it has not accounted for the possibility that such shocks may simultaneously affect *both*, even though the two are connected through employment relationships. This limitation is crucial for at least two reasons. First, in many countries, both firms and individuals are reliant on bank credit, such that both may face credit constraints in the event of a credit market disruption.[1,2] Second, existing research has shown that credit shocks to firms lead to wage cuts and layoffs (Berton et al. 2018; Chodorow-Reich 2014), while individuals' access to credit shapes their ability to smooth income and unemployment shocks (Gross and Souleles 2002; Herkenhoff, Phillips, and Cohen-Cole 2023), implying that there may be important interaction effects between firm-side and individual-side credit shocks. By ignoring this, researchers risk conflating the marginal impact of firm-side and individual-side credit shocks with the impact of their joint occurrence.

In this study, we use unique data to overcome this limitation and disentangle the real impact of a credit market disruption into the effect of firm-side credit shocks, individual-side credit shocks, and their interaction. To do so, we combine the Norwegian corporate credit register and household credit register through the employer-employee register. Linking these three datasets enables us to identify the bank relationships of both firms and their employees. Then, we enrich these data with detailed balance sheet information on banks, firms, and individuals, allowing us to observe a wide range of characteristics, including individuals' bank debt, labor income, wealth, and consumption.

Using this novel dataset, our analysis yields three main insights. First, we show that credit shocks simultaneously affecting firms and individuals have a significantly larger impact than the combined impact of credit shocks affecting only firms or only individuals. Specifically, individuals' labor income and consumption decline by nearly 6% when both they and their employer face a credit shock, compared to 1–2% when only they or only their employer face a credit shock. This result implies that the marginal effects of firm-side and individual-side credit

[1] Firms typically depend on banks for term loans and credit lines to finance labor and investments, while individuals rely on banks for mortgages, home equity lines of credit, consumer loans, and credit cards to meet their housing and consumption needs. Figures from the Global Financial Development Database compiled by the World Bank indicate that, in OECD countries, roughly 50% of firms have outstanding bank credit, and nearly 40% of individuals hold bank loans. These estimates likely understate the role of bank debt, as they exclude those who wish to borrow but are denied credit. Figure A1 in the Appendix plots the net percentage of banks tightening lending standards to firms and households over time, based on data from the U.S. Senior Loan Officer Survey. This figure illustrates that lending standards for both groups move closely together such that both firms and individuals face credit constraints in the event of a credit market disruption, such as during the global financial crisis of 2008.

[2] A credit market disruption refers to a decline in lenders' provision of credit, holding all else equal. In general, such disruptions can be triggered by regulatory changes, increased uncertainty, or behavioral shifts such as heightened risk aversion among lenders.

shocks are limited, while their interaction generates *amplified* real effects. Second, we show that this amplification arises as personal credit constraints hinder individuals' consumption smoothing and job search when confronted with wage cuts or layoffs triggered by their employer's credit constraints. In particular, credit constrained firms typically lay off employees, in which case credit constrained individuals have more difficulties engaging in job search, resulting in larger long-term losses in labor income and consumption than their non-constrained peers. Similarly, credit constrained firms often cut wages, in which case credit constrained individuals have more difficulties smoothing consumption. As expected, these effects are more pronounced for individuals who are more financially vulnerable, such as those with lower liquidity buffers. Lastly, our results suggest that this mechanism shapes the aggregate transmission of credit shocks, as declines in aggregate consumption and economic activity are concentrated where households and their employers are *jointly* credit constrained. Overall, our findings reconcile previously opposing views on the relative importance of firm-side versus household-side credit shocks by showing that *their interaction* is a key mechanism in the transmission of credit shocks.

The laboratory for our study is Norway, where firms and individuals—like in many advanced and developing economies—are highly leveraged and strongly reliant on bank credit. Norwegian firms are mostly small or medium-sized enterprises that rely on bank loans to finance labor and investments, while Norwegian individuals depend on banks for mortgages, credit cards, home-equity lines of credit, and other consumer loans.

To achieve causal identification, we follow prior research (e.g., Iyer et al. 2014; Jensen and Johannesen 2017) and exploit the global liquidity freeze triggered by the failure of Lehman Brothers in 2008 as a quasi-natural experiment. As explained in detail below, the global liquidity freeze exogenously forced a number of Norwegian banks that relied on foreign wholesale funding to cut credit. A key advantage of this setting is that its impact on the Norwegian banking system has distinct features that make it particularly well-suited for our analysis. First, the crisis originated externally, and spread globally through a disruption in short-term funding markets (Brunnermeier 2009). Second, Norwegian firms and individuals are heavily reliant on bank credit, with limited access to alternative financing sources. Moreover, they typically maintain a single bank relationship, limiting their ability to switch lenders in the event of a credit crunch. Third, while the liquidity dry-up forced certain Norwegian banks to cut credit, Norway did not experience a housing market bust, endogenous banking panic, or sovereign debt crisis before or during the period around the global liquidity freeze.

The paper is structured in two parts. In the first part, we show that banks exposed to the global liquidity freeze faced an exogenous funding shock, which led them to cut credit compared to non-exposed banks, consistent with prior studies. Specifically, following the global liquidity freeze, exposed banks' average foreign wholesale funding to total assets declined from about 10% to less than 2%. Using difference-in-differences models and loan-level data from the Norwegian corporate and household credit registers, we find that this led exposed banks to cut credit to firms and individuals by approximately 10% and raise loan rates by around 1 percentage point. These results hold when including firm-year or individual-year fixed effects controlling for credit demand (Khwaja and Mian 2008). Then, we show that affected firms and individuals faced binding credit constraints, as they were unable to offset the credit cut by borrowing from alternative lenders. For firms, corporate credit constraints led to a reduction in employment and investments, in line with Chodorow-Reich (2014) and Iyer et al. (2014). For individuals employed by credit constrained firms, this resulted in increased risk of wage cuts and job loss, leading to a decline in labor income of around 4%, in line with Adamopoulou et al. (2024) and Berton et al. (2018). For individuals facing personal credit constraints—such as reduced access to consumer credit or higher interest rates—consumption expenditures declined by nearly 3% compared to their non-constrained peers, consistent with Jensen and Johannesen (2017), Benmelech, Meisenzahl, and Ramcharan (2017), and Di Maggio et al. (2017).

Importantly, the estimates above are based on empirical specifications used in previous studies, which could not account for the fact that firm-side and individual-side credit shocks may occur simultaneously. As stated earlier, this is an important limitation as it may have led researchers to conflate the *marginal* impact of firm-side and individual-side credit shock with the impact of their *interaction*.[3]

In the second part of the paper, we leverage the richness of our data to overcome this limitation, and estimate a triple difference-in-differences model to compare the economic trajectories of individuals reliant on exposed versus non-exposed banks, employed by firms reliant on exposed versus non-exposed banks, after versus before the global liquidity freeze. Essentially, this enables us to isolate the effect of firm-side credit constraints, individual-side credit constraints, and their interaction in shaping the real impact of a credit market disruption. Put differently, our identification strategy enables us to quantify the economic relevance of the three scenarios illustrated in Figure 1. Based on this, we uncover three new findings.

Our first key finding is that credit shocks simultaneously affecting firms and individuals have a significantly larger impact than the combined impact of credit shocks affecting only firms or only individuals. Specifically, we find that individuals' labor income and consumption decline by around 1–2% when only they or only their employer face a credit shock, compared to the benchmark where neither do. In contrast, individuals whose personal credit constraints coincide with those of their employer experience average declines in labor income and consumption of 6%. The latter is almost twice as large as the sum of the separate effects, highlighting a previously unexplored *amplification effect*. This result holds after controlling for a rich set of individual-specific characteristics and including both individual and firm-residential municipality-year fixed effects, ensuring that we compare individuals with similar characteristics who are ex-ante employed by the same firm and live in the same municipality. Moreover, we do not find pre-trends

[3] As mentioned earlier, in many countries, both firms and individuals depend heavily on bank credit, implying that both are credit constrained in the event of a credit market disruption. Moreover, individuals often bank with the same institution as their employer, creating a direct channel through which firms and individuals can face simultaneous credit constraints, even in the event of idiosyncratic bank shocks. Figure I.A1 in the Internet Appendix shows that roughly one in four individuals in our sample used the same bank as their employer between 2003 and 2020—a pattern that likely extends beyond Norway. First, banks in most countries serve both corporate and retail clients; among the 29 global systemically important banks, 26 engage in both types of lending. Second, bank concentration in many countries has increased over the past two decades (World Bank 2024), and Figure I.A1 indicates that higher concentration is associated with more shared bank relationships. Third, many countries historically developed affiliation-specific financial institutions, such as credit unions, which typically serve geographically proximate individuals and businesses sharing a common bond—e.g., local factory workers, teachers, or residents (Ramcharan, Verani, and Van den Heuvel 2016)—leading employees to rely on the same bank as their employer. Finally, firms in some countries encourage employees to bank with the same institution to streamline payroll processes. While this practice is not common in Norway, it is prevalent in the U.S. and UK, and raises the likelihood that individuals and firms share the same bank.

when estimating dynamic difference-in-differences models suggesting that, absent the credit crunch, the economic trajectories of the individuals would have evolved in parallel.

This finding implies that the marginal effects of firm-side and individual-side credit shocks are limited, whereas their interaction generates not just large but *amplified* real effects. Hence, to fully disentangle the channels through which credit market disruptions affect the economy, research should account for the joint occurrence of firm-side and individual-side credit shocks, particularly in settings where both firms and households rely on bank credit (Beck, Demirguc-Kunt, and Martinez Peria 2007).

Our second key finding is that the aforementioned amplification effect can be attributed to the fact that individuals' personal credit constraints hinder their consumption smoothing and job search, thereby exacerbating the impact of their employer's credit constraints. For example, credit constrained employers typically lay off workers, in which case individuals' personal credit constraints hinder their ability to engage in job search and reduce their long-term income prospects. In a similar vein, when credit constrained employers reduce wages, individuals' limited access to credit impairs their ability to smooth consumption.

To support this interpretation, we first focus on the labor market outcomes of *displaced* workers, as firm-side credit constraints often trigger layoffs (Chodorow-Reich 2014). If personal credit constraints limit individuals' ability to finance job search or sustain consumption during unemployment, financially constrained workers should experience worse post-displacement outcomes (Chetty 2008; Herkenhoff, Phillips, and Cohen-Cole 2023; He and Le Maire 2023).[4] Consistent with this, we find that, among the subsample of displaced workers, these workers take 30 fewer days to find new employment, and conditional on re-employment, earn 9% lower wages. They also seem to reduce the scope of their job search, as we find that they are about 30% less likely to switch occupations following displacement. Together, these findings align with our conjecture that workers' personal credit constraints exacerbate the effects of their employer's credit constraints by reducing their job search effectiveness and lowering their reservation wages, thereby leading to persistently weaker labor market outcomes following displacement.

By contrast, among *non-displaced* workers, wage risk is similar for individuals with exposed and non-exposed banks, suggesting that credit constrained firms do not differentially transmit the shock across these groups. Yet, workers whose personal bank is exposed experience 4% larger declines in consumption, even if they are not displaced, consistent with the notion that personal credit constraints hinder their ability to smooth consumption in response to the wage shocks resulting from their employer's credit constraints (Gross and Souleles 2002; Zeldes 1989).[5]

Next, we explore cross-sectional patterns that support our proposed interpretation. In principle, one would expect the adverse effects to be more pronounced when financial frictions exacerbate individuals' financial vulnerability. Consistent with this, we first show that the

[4] Job search is costly and requires liquidity—for example, to cover living expenses, relocation, or retraining while unemployed. Evidence shows that many job seekers rely on credit to finance these costs. For instance, Herkenhoff, Phillips, and Cohen-Cole (2023) show that roughly one-third of displaced U.S. workers borrow during unemployment, and He and Le Maire (2023) show that Danish homeowners draw on home-equity lines of credit to finance job search (e.g., also see Kabas and Roszbach 2025; Sullivan 2008).

[5] We also find that, within the sample of non-displaced workers, those with personal credit constraints are significantly less likely to voluntarily switch employers. This supports the interpretation that their personal credit constraints not only restrict consumption smoothing but also hinder job mobility, making credit constrained individuals less able to respond to firm-specific wage cuts by seeking better outside options.

effects are concentrated among individuals with lower ex-ante liquidity buffers, for whom credit constraints are particularly binding in smoothing consumption or financing job search. Second, we find that the effects are driven by individuals with a single bank relationship, who face a lower substitutability of credit across lenders. Third, we document that the adverse effects are more pronounced for individuals with higher ex-ante debt-to-income ratios, consistent with the idea that a greater interest burden tightens disposable income and constrains both consumption and labor market choices. We exploit several other dimensions of heterogeneity which support our proposed interpretation, as discussed in detail below.

Our third key result is that the amplification mechanism uncovered above has implications for the aggregate transmission of credit shocks. To explore this, we aggregate our data to the municipality level (as in Giroud and Mueller 2017; Mian, Sufi, and Verner 2020) and examine the importance of firm-side and household-side credit constraints in shaping the economic impact of the credit crunch across municipalities. When we follow prior studies and do not account for interactions between firm-side and household-side credit constraints, we find that both types of constraints appear economically significant in explaining variation in aggregate consumption, firm output, and employment across municipalities following the credit crunch. However, once we account for these interactions, we find that declines in economic activity are concentrated in regions where individuals and their employers are *jointly* exposed to credit shocks. Specifically, aggregate consumption, firm output, and employment fall by 8–10% more in municipalities where all individuals and their employers relied on exposed banks, compared to declines of 0–4% in municipalities where exposure is limited to either individuals or employers. As expected, this interaction effect is more important in municipalities with a higher concentration of non-tradable industries, which are more reliant on local demand (Mian and Sufi 2014; Moretti 2010). We further test this mechanism in a cross-country setting, using historical banking crises and exploiting heterogeneity in firms' and households' reliance on bank debt. The cross-country analysis broadly confirms our main results, strengthening the external validity of our findings.

Overall, our results indicate that *the interaction* between firm-side and household-side credit constraints is a key driver in the transmission of credit shocks. Although our identification strategy is reduced form, the patterns we document align with macroeconomic models in which job losses reduce individuals' labor income and demand, particularly when individuals are also credit constrained, triggering further job losses and amplifying the downturn through a feedback loop (e.g., Kekre 2023; McKay and Reis 2021; Ravn and Sterk 2017).

Our results are robust to a series of additional tests. First, as mentioned earlier, we show that the parallel trends assumption underlying our difference-in-differences models is supported.

Second, one could be concerned about selection effects, where individuals whose personal credit constraints coincide with those of their employer are inherently different (e.g., less financially sophisticated) than their peers. In practice, this does not appear to be a major concern, as these individuals are comparable to their peers across most observable characteristics. In addition, our results hold after including individual fixed effects as well as a series of pre-event individual characteristics interacted with year dummies, including age, gender, education, homeownership, and debt-to-income, among others. Our results also remain unchanged when we employ propensity score matching to construct a matched sample of very similar individuals.

Third, we rule out several potential alternative explanations for our findings. For instance, one could worry that individuals whose personal credit constraints coincide with those of their employer live in localities with weaker labor markets, poorer banking conditions, or more adverse housing price dynamics. We present several analyses to rule out these and many other alternative explanations, supporting the robustness and proposed interpretation of our findings. For example, our results hold when we include firm-occupation-year or firm-residential municipality-year fixed effects. These specifications strengthen our identification strategy by comparing workers from *the same firm* with *the same occupation* or living in *the same municipality*, thereby ruling out the possibility that our findings are driven by differences in occupation or local confounding factors. Further, our results remain robust when excluding exporters and importers (to account for firms' exposure to foreign demand), individuals with stock holdings (to account for individuals' exposure to stock price fluctuations), or single-branch localities. Lastly, we show that changes in hours worked, wage volatility, or spousal income cannot account for our observed effects.

Fourth, as mentioned earlier, a large share of individuals rely on the same bank as their employer, directly exposing them to both firm-side and individual-side credit constraints when their bank cuts credit. This raises the question of whether individuals internalize this risk. To explore this, we analyze whether workers of credit constrained firms who shared the same bank as their employer prior to the credit crunch respond by subsequently avoiding such overlap—potentially reflecting an effort to reduce exposure to joint credit shocks in the future. We do not find evidence for this, consistent with the view that individuals may not intentionally choose to have a relationship with the same or a different bank than their employer.

Overall, our paper offers new evidence on the drivers of credit shock transmission. While prior research has debated the relative importance of firm-side versus household-side credit shocks, our findings reconcile these opposing views by showing that their interaction is central to the transmission of credit shocks. This implies that two credit market disruptions with the same contraction in credit can have very different consequences depending on the overlap between the affected firms and households. This finding, and the mechanisms underlying it, may inform future research and policy design, as discussed in detail below.

Related literature A large strand of research has shown that credit shocks to firms lead to reductions in investment, employment, and wages (Acharya et al. 2018; Amiti and Weinstein 2018; Benmelech, Frydman, and Papanikolaou 2019; Bentolila, Jansen, and Jiménez 2018; Chodorow-Reich 2014; Cingano, Manaresi, and Sette 2016; Iyer et al. 2014). Building on this, recent studies have leveraged employer-employee data to show that firm-specific credit constraints have large, long-lasting effects on individuals' labor income (Adamopoulou et al. 2024; Berton et al. 2018; Jasova et al. 2021; Fonseca and Van Doornik 2022; Moser et al. 2024). Another strand of research has focused on credit shocks to individuals, documenting that such shocks reduce individuals' consumer spending (Benmelech, Meisenzahl, and Ramcharan 2017; Chava et al. 2023; Jensen and Johannesen 2017; Ramcharan, Verani, and Van den Heuvel 2016).

A key limitation of prior research is that it could not account for the fact that credit shocks may simultaneously affect both firms and individuals, as they are connected through employment relationships. As stated earlier, this omission is important because (i) credit shocks often hit both firms and individuals and (ii) credit shocks to firms affect individuals and vice versa. To the best of our knowledge, our paper is the first to overcome this limitation and to demonstrate that accounting for firms' and individuals' joint exposure to credit shocks is key to disentangling the real impact of a credit market disruption. The reason is that individuals' personal credit constraints impair their ability to smooth consumption or finance job search when confronted by wage cuts or job loss as a result of their employer's credit constraints. Consequently, credit shocks that affect both individuals and their employers generate amplified real effects.

More broadly, our study contributes to the literature on the role of firm-side versus household-side credit frictions in shaping the economic impact of credit market disruptions. One strand of research has emphasized firm-side credit frictions, highlighting how bank credit shocks weaken firms' balance sheets and thereby generate economic downturns. For instance, Chodorow-Reich (2014), Giroud and Mueller (2017) and Huber (2018) show that, following the global financial crisis, regions where a larger share of firms faced adverse credit conditions saw the largest declines in economic activity, consistent with the view that tighter credit supply to firms is an important channel for the transmission of credit shocks to the broader economy.[6] More recently, another strand has focused on household-side credit frictions. Mian and Sufi (2010, 2014) and Mian, Rao, and Sufi (2013) for example show that regions that experienced the largest contractions in household debt during the global financial crisis also saw the steepest declines in consumption and employment.[7]

While these studies have significantly advanced our understanding of the channels through which financial shocks propagate to the real economy, the evidence remains inconclusive regarding whether firm-side or household-side credit frictions play the most important role. In this context, Gertler and Gilchrist (2018) emphasize that "a complete description of the Great Recession must take account of the financial distress facing both households and banks and, as the crisis unfolded, nonfinancial firms as well." We view our study as a meaningful step toward addressing this call. By combining rich micro-data on banks, firms, and individuals, we reconcile prior research by showing that *the interaction* between firm-side and household-side credit constraints is a key mechanism in the transmission of credit shocks—a mechanism that may have been relevant for the recession following the 2008 financial crisis.[8,9]

Consequently, while the contribution of our paper is primarily empirical, it also has implications for macroeconomic models that link financial shocks to business cycles. Kehoe, Midrigan, and Pastorino (2018) and Heathcote, Storesletten, and Violante (2009) provide an

[6]See also Ashcraft (2005), Bai, Carvalho, and Phillips (2018), Bernanke (1983), Calomiris and Mason (2003), Driscoll (2004), Greenstone, Mas, and Nguyen (2020), Jermann and Quadrini (2012), Kroszner, Laeven, and Klingebiel (2007), Peek and Rosengren (2000), and Reinhart and Rogoff (2009).

[7]See also Eggertsson and Krugman (2012), Guerrieri and Lorenzoni (2017), Hall (2011), Jones, Midrigan, and Philippon (2022), Justiniano, Primiceri, and Tambalotti (2019), Kehoe, Midrigan, and Pastorino (2019), and Mian, Sufi, and Verner (2020).

[8]In line with this argument, Becard and Gauthier (2022) estimate a macroeconomic model showing that simultaneous credit contractions for firms and households replicates the joint dynamics of *all* key aggregate economic variables during the U.S. Great Recession—i.e., output, consumption, investment, and employment. However, their framework does not account for interaction effects and does not fully pin down the underlying mechanisms. We address this gap by showing that when credit constrained firms reduce wages and employment, credit constrained individuals are less able to smooth consumption or finance job search, so that *interactions* between firm-side and household-side credit constraints amplify the downturn.

[9]In this sense, our study also relates to a growing literature studying the role of corporate versus household debt in credit and business cycles (e.g., Ivashina et al. 2024; Jordà, Schularick, and Taylor 2016; Mian, Sufi, and Verner 2017; Müller and Verner 2024).

overview of the literature on real business cycle models and quantitative heterogeneous agent models, respectively. These frameworks typically examine how financial shocks to either firms or households affect macroeconomic dynamics. Our findings underscore the importance of constructing rich models that combine financial frictions on both the firm and household side with rigidities in both goods and labor markets.[10] In addition, our findings offer an estimated amplification elasticity of roughly 2 for joint firm-side and household-side credit constraints, which may be useful for calibrating future models.

Lastly, our study offers several policy implications. For one, our findings may inform the design of stress testing frameworks. These frameworks currently do not account for the interconnections between firms and households (ECB 2024), which, as we document, can amplify the impact of credit shocks. To strengthen financial stability assessments, regulatory authorities could expand data collection to capture these linkages and integrate them into stress testing models. Further, our findings add nuance to recent studies showing that credit policies targeting credit constrained individuals can yield substantial welfare gains during recessions (e.g., Agarwal et al. 2023; Di Maggio et al. 2017; Eberly and Krishnamurthy 2014; Ganong and Noel 2020). Specifically, our results suggest that such policies should particularly target credit constrained individuals—or regions with a high concentration of credit constrained individuals—who face elevated income or job loss risk.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. In Section 3, we discuss the data sources and summary statistics. Sections 4 and 5 present our identification strategy and main results, followed by a series of additional analyses and robustness checks in Section 6. Finally, Section 7 concludes.

2. Institutional background

In our analysis, we use the global liquidity freeze triggered by the failure of Lehman Brothers in September 2008 as a laboratory to disentangle the real effects of a credit market disruption. Below, we first describe the dynamics of the Norwegian economy and banking sector around this period. After that, we provide institutional details on Norway's banking sector, labor market, and individual balance sheets that might be informative for the external validity of our findings.

2.1. The Norwegian banking sector and the impact of the global liquidity freeze

In the years preceding the global financial crisis, Norway experienced robust economic growth.[11] As bank lending grew faster than deposit inflows, some Norwegian banks turned to the inter-

[10] To the best of our knowledge, only Boar et al. (2025) and Kehoe et al. (2020) have made a significant step in developing models that allow to assess the relative importance of firm-side and household-side credit frictions. However, both studies differ from ours in important ways. Boar et al. (2025) do not consider interactions between these two types of frictions, which we show to be crucial for the transmission of credit shocks. In the theoretical model of Kehoe et al. (2020), credit shocks can affect both firms and households. However, the authors do not find that credit shocks simultaneously affecting both generate an amplification effect. A possible explanation is that the model does not consider how firm-side credit constraints increase individuals' income and employment risk, which, in turn, is exacerbated when individual-side credit constraints limit consumption smoothing and job search, as our evidence suggests.

[11] Figure I.A2a–I.A2f in the Internet Appendix illustrate the evolution of key macroeconomic variables in Norway.

national wholesale funding market to sustain their credit expansion.[12] Although Norwegian banks had minimal exposure to U.S. mortgage-backed securities, those reliant on wholesale funding were vulnerable to the global liquidity freeze following the failure of Lehman Brothers in September 2008 (IMF 2015; Rangvid 2020). To stabilize funding conditions, the Norwegian government introduced a swap arrangement from November 2008 to December 2009, but despite these efforts the global liquidity freeze disrupted the ability of banks exposed to the foreign wholesale market to finance their operations, forcing them to cut credit. In Norway, the impact of the global liquidity freeze became clear in 2009, as aggregate credit supply began to contract. By the end of 2011, the ratio of credit to GDP was almost 10% lower compared to 2009.[13]

2.2. External validity

Banking sector Norway is a bank-based economy. As in most OECD countries, Norwegian firms and individuals are very reliant on bank credit. Most Norwegian firms are small or medium-sized enterprises, which generally do not have access to alternative financing sources. Norwegian individuals rely on banks for various types of credit, including mortgages, home-equity lines of credit (HELOCs), and credit cards. Moreover, both firms and individuals typically maintain an exclusive relationship with a single bank, making switching difficult.

The structure of the Norwegian banking sector resembles that of most OECD economies in being relatively concentrated. While Norway counts more than 100 commercial banks, the share of assets held by the top five banks was nearly 70% in 2023; most of the remaining banks are small, local banks. This is similar to the U.S. and euro area, where the top five banks accounted for, respectively, 50% and 70% of total assets in 2023. Most Norwegian banks provide both corporate credit to firms and retail credit to individuals—a structure that is common among banks globally (e.g., 26 of the 29 G-SIBs offer both corporate and retail banking services).

Labor market The structure of the Norwegian labor market is similar to that of many OECD countries, though it is relatively more rigid. For instance, unemployment insurance is generous—with benefits covering roughly 60% of lost wages—though it does not fully insure against unemployment risk (see Fagereng, Guiso, and Pistaferri 2018).[14] Firstly, unemployment benefits are time limited, replace a fraction of lost wages, and remaining unemployed is economically costly due to scarring effects (Huckfeldt 2022). Secondly, there is little government protection against the risk of wage fluctuations conditional on employment, meaning that wage cuts due to firm-specific shocks can severely affect individuals' disposable income (e.g., see

[12]Note that access to foreign wholesale funding, particularly U.S. money markets, is restricted to large, globally oriented banks for two main reasons: (i) money market funds require a rating from at least one major credit agency, and (ii) the high fixed costs of issuing commercial paper are only viable for large-scale issuances. Only large Norwegian banks satisfied these conditions, while small banks did not. Importantly, however, small Norwegian banks are very comparable to large banks along several dimensions, as discussed in more detail below (also see Ippolito et al. 2024).

[13]The global financial crisis impacted the Norwegian banking sector and real economy, but it did not manifest itself as a solvency crisis. Norwegian banks were relatively well capitalized and, unlike in many other countries, there were no bank failures. Norwegian banks also remained largely unaffected by the Icelandic banking crisis during this period, owing to the limited presence of Icelandic banks in Norway's financial sector (Norges Bank 2008).

[14]Unemployment benefit coverage in Norway is close to the OECD average of 60%. For permanent layoffs, unemployment insurance lasts for 52–104 weeks. For temporary layoffs, unemployment insurance is limited to 26 weeks within a 1.5–year period since layoff. Further, individuals may also have access to disability insurance, sickness and maternity benefits, and labor market programs to enhance their skills in case of displacement. Despite the institutional differences, average unemployment duration in Norway is, on average, only 15% longer than in the U.S. (Fagereng, Guiso, and Pistaferri 2018).

Fagereng, Guiso, and Pistaferri 2017; Juelsrud and Wold 2024). This is crucial as labor income is the most important source of income in Norway, as in most countries.

Individual balance sheets The composition of Norwegian individuals' balance sheets is relatively comparable to that of individuals in other countries. For example, housing wealth accounts for around 65% of gross household wealth and safe assets for 48% of total assets, which is similar to the U.S. (Badarinza, Campbell, and Ramadorai 2016). Homeownership rates in Norway have been stable at around 80% over the past two decades, which is slightly higher than in the U.S. but close to the OECD median. Consumption patterns in Norway are also comparable to those in most advanced economies. Norwegian households spend approximately 12% of total expenditures on food and 35% on housing, water, electricity, and gas, compared to 13% and 33%, respectively, for U.S. households.[15] One notable difference is that Norwegian individuals are, on average, wealthier. For instance, in 2023, 40% of U.S. adults reported they would need to liquidate assets or borrow to finance an unanticipated expense of USD 400. By comparison, around 20% of Norwegian adults reported that they could not cover an unexpected expense of NOK 20,000 (USD 1,800) without selling assets or borrowing money.[16]

Like in the U.S., an important feature of the Norwegian loan market is the widespread availability of flexible credit lines backed by home equity.[17] Many Norwegian individuals use home equity withdrawals for consumption smoothing and financing purchases such as cars, home improvements, or large unexpected expenses, similar to home equity-based borrowing patterns observed in the U.S. and some European countries (Amromin and McGranahan 2015; Calza, Monacelli, and Stracca 2013; Mian and Sufi 2011). Credit cards represent another important form of credit, accounting for approximately 15% of personal consumption expenditures in Norway in 2021 (Norges Bank 2021), compared to about 25% in the U.S. (Chava et al. 2023).[18]

In sum, Norway is characterized by a bank-based economy, a rigid labor market with partial insurance, and household financial patterns akin to those in other advanced economies. If anything, the estimated economic impact of a credit shock in our setting is most likely a lower bound relative to less wealthy countries with less extensive social insurance systems.

3. Data and measurement

Our analysis combines several administrative datasets from Norway. Most of these datasets are collected and maintained by government agencies as a basis for taxation and, hence, free of measurement error. Below, we explain each dataset in detail, how the different datasets are merged, and the filters applied to construct our final data sample.

[15] See https://www.ssb.no/en/inntekt-og-forbruk/forbruk/statistikk/forbruksundersokelsen for Norway and https://www.bls.gov/opub/ for the U.S.

[16] See https://data.ssb.no/api/v0/en/table/12123/ for Norway and https://www.federalreserve.gov/publications/files/2017-report-economic-well-being-us-households-201805.pdf for the U.S.

[17] Note that nearly all mortgages in Norway are adjustable-rate mortgages, mitigating potential concerns that an individual's choice of mortgage rate structure is not random at the time of home purchase or subsequent refinancing (Di Maggio et al. 2017).

[18] Despite the high levels of household leverage, credit default rates have remained low in Norway, averaging around 1% over the past two decades (Lindquist, Solheim, and Vatne 2017). A key reason for this is Norway's full-recourse loan policy which, like several U.S. states, holds borrowers personally liable for mortgage debt even after foreclosure, thereby discouraging strategic defaults.

3.1. Data sources

We combine the Norwegian employer-employee register with corporate and household credit register data, enabling us to observe the bank relationships of both firms and their workers. The employer-employee data are administered by the Norwegian Labor and Welfare Administration and record information on all employment relationships, including which firm an individual works for, the occupation held, the job start and termination dates, as well as the location of the workplace. The corporate and household credit registers are obtained from the Norwegian Tax Administration and contain information on the amount borrowed (deposited) by a firm or individual at a given bank as well as the interest paid (received) over the year.

We complement these data with detailed information on bank, firm, and individual balance sheets. The bank balance sheet data are maintained by Norges Bank, and contain detailed data on banks' asset and liability structure as well as income statements for all banks active in Norway (including subsidiaries and branches of foreign-owned banks). We obtain firm data from the Norwegian Firm Registry ("*Brønnøysundregistrene*"), which contains information on firms' balance sheets as well as general firm characteristics, such as industry and location. This dataset also contains a firm headquarters identifier, which we use to aggregate all firm data to the headquarters level. The individual-level data are collected from the Norwegian population and tax registers. The former include background information such as gender, age, marital status, residential municipality, and education. The latter include detailed balance sheet information on labor income, capital gains, government transfers, debt, and total wealth, among others.

3.2. Data sample construction

To construct our data sample, we first link the different datasets using (anonymized) person, firm, and bank identification numbers that are consistent across all datasets. This results in a bank-firm-individual-bank (bank-employer-employee-bank) level dataset enabling us to observe the bank relationships of both employees and their employers, as well as a range of bank-, firm-, and individual-specific characteristics on an annual basis.

We apply the following filters. First, we exclude firms from the financial and insurance sector, the public administration sector, the education sector, and activities of extra-territorial entities. Second, we restrict our sample to limited liability companies, which account for approximately 90% of private sector employment in Norway. Third, we restrict our sample to firms with at least one employee (e.g., to omit companies set up for tax purposes). Fourth, we exclude individuals younger than 25 years (who may still be in college) or older than 60 years (who may have intermittent participation and access to early retirement). We also remove self-employed individuals since it is generally not possible to separate borrowing for business and private purposes on the balance sheet of those individuals. Finally, only individuals who are employed and who can be matched to an occupation in the pre-event period (i.e., from 2005 to 2008) are included. After these exclusions, our main data sample contains nearly 300,000 individuals linked to approximately 40,000 firms and 143 banks, over the period 2005–2011. The sample period ends in 2011 to avoid confounding effects from subsequent regulatory reforms that could have influenced bank lending.[19]

3.3. Variables

Once the dataset is constructed, we define a unique relationship bank for each firm and individual in 2007 following the procedure of Jensen and Johannesen (2017). As explained later, this facilitates the interpretation of the results presented in our main analysis. For firms and individuals who only had one bank relationship in 2007, this is their relationship bank. For those who had multiple bank relationships, but only had a loan in one of those banks, this is their relationship bank. For those who had loans in multiple banks, the bank in which the loan balance was largest is their relationship bank. For those who had no loans, but had deposits in multiple banks, the bank in which the deposit balance was largest is their relationship bank. We thus assume that loans provide a stronger bank relationship than deposits, and that bank relationships are stronger the larger the account balance.[20] In practice, this classification affects only a small subset of firms and individuals, as most maintain a single bank relationship anyway. Nevertheless, as we show in robustness tests below, our results are unaffected by alternative classification schemes.

Our three individual-level outcomes of interest are total debt, labor earnings, and consumption expenditures, which we define as follows. Total debt is measured as the end-of-year total outstanding debt. In general, individual debt mainly consists of mortgage debt, bank credit card debt, and HELOCs, but the data do not allow us to distinguish between these different types of debt. Total labor earnings consist of the sum of all labor earnings obtained by an individual from all his or her employers (and equal zero if an individual is unemployed). Consumption is measured using the accounting identity that total consumption expenditure equals the sum of total income and capital gains minus the change in wealth (see Browning and Leth-Petersen 2003; Koijen, Van Nieuwerburgh, and Vestman 2014). As this is a common approach used in the literature, we leave details on the measurement for the Internet Appendix. One point worth highlighting is that we apply a set of filters to exclude observations that the literature has identified as potentially problematic for imputing consumption, resulting in a smaller number of observations for consumption relative to the other variables. All other key variables are defined in Table A1 in the Appendix.

3.4. Summary statistics

Table 1 presents the summary statistics for the key bank-, firm-, and individual-level variables used in our analysis. Firms have an average debt-to-assets ratio of 0.85, a median workforce size of six employees, and most firms maintain a single lending relationship. The average individual is 43 years old, approximately 50% are married, 30% are female, and 27% hold a college degree or higher. The average debt-to-income ratio is 1.81, with over half of individuals maintaining only one lending relationship.

[19] For instance, in 2012 the Norwegian government introduced a loan-to-value restriction (Kabas and Roszbach 2025), and in 2013 it implemented risk-weighted bank capital requirements (Juelsrud and Wold 2020).

[20] Figures I.A3a–I.A3d in the Internet Appendix plot the loan and deposit shares of firms and individuals at their relationship bank, confirming that both firms and individuals hold the vast majority of their loans and deposits with their relationship bank.

Among the 143 banks in our sample, 27 relied on foreign wholesale funding in the period preceding the global liquidity freeze. Panel A of Table 2 presents a balance test comparing exposed and non-exposed banks. For each group, we present the mean and standard deviation of key balance sheet variables, as well as the normalized difference test. The latter is a scale-and-sample-size-free estimator proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) proposed a heuristic threshold of 0.25 in absolute value for significant differences. Overall, exposed banks are relatively comparable to non-exposed banks. In particular, we find no significant differences in loans-to-assets ratio, profitability, or loan losses relative to assets. The only notable difference between exposed and non-exposed banks is their size and capitalization, consistent with the idea that smaller (often better-capitalized) banks are less likely to rely on wholesale funding sources.

Panels B and C of Table 2 present balance tests comparing affected and non-affected firms and individuals, respectively. Affected firms and individuals are defined as those whose relationship bank was exposed to the global liquidity freeze (see Section 5.1). Panel B shows that affected and non-affected firms are highly comparable in terms of leverage, profitability, and the ratio of fixed assets to total assets. The main differences are that affected firms tend to be larger and maintain more lending relationships than non-affected firms. Panel C indicates that affected and non-affected individuals are similarly balanced across most characteristics, including age, educational attainment, total income, and consumption. The most notable differences are that affected individuals are, on average, slightly more leveraged and maintain more lending relationships. Table I.A1 in the Internet Appendix provides a further breakdown of affected and non-affected individuals employed by affected and non-affected firms.

Importantly, for our identification to hold, there can be differences between affected and non-affected firms and individuals, as long as trends in the outcomes of the groups would have been parallel absent the shock. In our analysis, we always check that trends are parallel before the shock. In addition, as explained in detail below, our results hold when controlling for a large set of pre-event firm and individual covariates interacted with year dummies.

4. Credit market disruption

The key objective of our study is to disentangle the real impact of a credit market disruption into the effect of individual-side credit constraints, firm-side credit constraints, and their interaction. To do so, the *ideal* experiment would involve a random shock to a bank which lends to a randomly assigned set of firms and a randomly assigned subset of workers of those firms. Following prior research (e.g., Iyer et al. 2014; Jensen and Johannesen 2017), we get as close as possible to this ideal experiment by exploiting the global liquidity freeze triggered by the failure of Lehman Brothers which exogenously forced banks reliant on foreign wholesale funding to cut both corporate and retail credit.

As mentioned earlier, Norway has a number of key features that offer an excellent setting for this. First, the credit shock arising from the global liquidity freeze was exogenous, originating outside the Norwegian economy. Second, unlike other countries such as the U.S. or Spain, Norway did not experience a housing market bust, endogenous banking panic, or sovereign debt crisis before or during the period around the global liquidity freeze. Third, Norwegian firms and individuals are highly reliant on bank credit and often maintain a strong bank relationship with a single bank. Lastly, a more general advantage of using the global liquidity freeze as our setting is that it makes our results more easily comparable to those from previous papers.[21]

The foundation of using the global liquidity freeze as a quasi-natural experiment relies on the premise that banks reliant on foreign wholesale funding tightened their credit supply relative to other banks following the liquidity dry-up. Figure 2 plots the evolution of the natural logarithm of total loans outstanding by exposed banks (in red, right axis) and non-exposed banks (in blue, left axis) in the years before and after the global liquidity freeze, providing suggestive evidence in support of this premise. In the next subsection, we use loan-level data to provide more formal evidence consistent with a differential credit supply response.[22] Our main analysis, presented in Section 5 below, uses this exogenous variation in bank credit supply to disentangle the real impact of a credit market disruption.

4.1. Loan-level evidence of the credit market disruption

We use loan-level data from the Norwegian corporate and household credit registers to examine whether banks reliant on foreign wholesale funding reduced credit supply following the global liquidity freeze, relative to non-exposed banks. A key advantage of these data is that they allow us to address concerns about differential credit demand: in principle, borrowers' demand for credit could be correlated with their bank's exposure to foreign wholesale funding. To rule out this possibility, we exploit the fact that firms and individuals with loan accounts in multiple banks create within-firm and within-individual variation in loan outcomes. If such borrowers experience relatively larger credit reductions from exposed banks after the freeze, this reflects a differential change in credit supply rather than shifts in credit demand. Formally, we estimate the following difference-in-differences model at the bank-firm-level:

$$y_{b,f,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_{f,t} + \epsilon_{b,f,t} \tag{1}$$

where $y_{b,f,t}$ is credit growth between bank b and firm f or the loan rate charged by bank b to firm f in year t.[23] $Post_t$ is a binary variable equal to one after 2008, and zero otherwise.

[21] The global liquidity freeze offers an excellent empirical setting, but in principle the amplification effect we document applies to any credit market disruption that causes banks to cut both corporate and retail lending, as is often the case in the event of bank failures or banking crises (as documented in Internet Appendix E). In contrast, the amplification effect would not necessarily emerge when banks cut credit to only firms or only households, e.g. in the event of loan portfolio rebalancing driven by regulatory reforms (e.g., Juelsrud and Wold 2020). As discussed in detail below, this distinction implies that not all credit market disruptions are equal, yielding important insights for future research and policymakers.

[22] Table I.C1 in the Internet Appendix also presents evidence based on bank-level data, showing that exposed banks reduced both corporate and retail lending. However, since the bank-level regressions do not allow us to control for firms' and individuals' credit demand, we place greater emphasis on the loan-level results discussed in the main body of the paper, which allow to isolate credit supply effects. In addition, there are also several Norwegian news outlets that reported a reduction in Norwegian banks' credit supply as a result of the global liquidity freeze, see for instance https://www.dn.no/strammer-inn-pa-utlanet/1-1-1306725 or https://e24.no/boers-og-finans/i/Jo7B56/norges-banks-utlaansundersoekelse-det-skal-bli-enda-verre-aa-faa-laan.

[23] Credit growth is defined as $\frac{Credit_{f,b,t} - Credit_{f,b,t-1}}{0.5 \times (Credit_{f,b,t} + Credit_{f,b,t-1})}$. This transformation is widely used to measure credit supply (e.g., Chodorow-Reich 2014). The measure captures extensive margin adjustment by taking on a value of 2 when a lender enters a new lending relationship with a borrower and a value of -2 when a lender ends a lending relationship. In between, the measure approximates log first differences in credit. Loan rates are computed as $\frac{Interest\ paid_{f,b,t}}{0.5 \cdot (Credit_{f,b,t} + Credit_{f,b,t-1})}$ following the approach of Iyer et al. (2019). Essentially, as we do not observe loan rates in the micro-data, we impute them based on the interest paid and account balance observed in year t and $t-1$. Summary statistics are reported in Internet Appendix Table I.A2.

$Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding in 2007, and zero otherwise.[24] As mentioned earlier, Panel A of Table 2 shows that exposed and non-exposed banks are similar across a wide range of bank characteristics. Nevertheless, we include a vector of pre-treatment bank controls ($X_{b,pre}$) interacted with year dummies to mitigate concerns about omitted variable bias. The bank controls include banks' size, capital ratio, return on assets, and loan loss provisions to total assets. γ_b and $\gamma_{f,t}$ represent bank and firm-year fixed effects, respectively, and $\epsilon_{b,f,t}$ is the error term which is clustered at the bank-level. We employ a similar regression model at the bank-individual-level where we replace the firm-year fixed effects by individual-year fixed effects. The inclusion of these fixed effects allows us to control for potential demand effects (Khwaja and Mian 2008).

The results are reported in Table 3. In this table, the outcome variable is credit growth in Panel A and loan rates in Panel B. In each panel, the first two columns report the results for banks' lending to firms, and the last two columns for banks' lending to individuals. Across the different columns of Panel A, we find a significantly negative interaction term, which implies a decrease in corporate and retail credit by exposed banks of around 10% compared to non-exposed banks after the liquidity dry-up. The inclusion of firm-year and individual-year fixed effects in the even-numbered columns supports the interpretation that the observed effects are driven by a reduction in credit supply. Moreover, the estimates in Panel B show that exposed banks also increased loan rates charged to firms and individuals by around one percentage point compared to non-exposed banks. In general, the decline in loan amounts and increase in interest rates is consistent with a reduction in credit *supply*. Figures 3a and 3d confirm that exposed and non-exposed banks did not have significantly different lending patterns *before* the global liquidity freeze, while exposed banks cut credit and raised interest rates significantly more *after* the global liquidity freeze, supporting the parallel trends assumption underlying our difference-in-differences model.

Note that the loan-level data do not allow us to distinguish between loan types. However, as discussed in Internet Appendix C, the bank balance sheet data enable us to differentiate between retail loans backed by residential property (primarily mortgages and HELOCs) and other retail loans (such as credit cards, car loans, leasing, and other consumer credit). The results reported in Internet Appendix Table I.C2 indicate that exposed banks cut both mortgage and other retail credit by approximately 15%, suggesting that the contraction in credit supply to individuals documented in Table 3 reflects a decline in both mortgage and other retail loans.

Overall, our results are comparable to those from prior studies on corporate and retail bank lending after the global liquidity freeze. For firm lending, Iyer et al. (2014) for instance document that Portuguese banks exposed to the freeze reduced credit by 10% on average, and Cingano, Manaresi, and Sette (2016) report similar findings for Italian banks. In the context of consumer lending, Chava et al. (2023) show that U.S. banks more reliant on short-term wholesale funding cut individuals' credit card limits by approximately $1,500. Similarly, Jensen and Johannesen (2017) find that Danish individuals whose relationship bank was exposed to the global liquidity freeze were 15% less likely to obtain a new loan, which aligns with the drop in retail credit

[24] Internet Appendix Table I.A3 shows that the results are similar when we use a treatment intensity measure based on banks' ratio of foreign wholesale funding to total assets instead of a treatment dummy.

supply documented by Puri, Rocholl, and Steffen (2011) for Germany.

In a frictionless credit market, a reduction in lending by one bank could, in principle, be fully offset by new credit from other banks. In practice, however, frictions such as switching costs and informational asymmetries make bank relationships sticky and limit the substitutability of credit across lenders (Klemperer 1995; Jaffee and Russell 1976; Sharpe 1990; Stiglitz and Weiss 1981). This has been well documented by several empirical studies, both for corporate and retail credit markets (e.g., Amiti and Weinstein 2018; Cao et al. 2025; Chodorow-Reich 2014; Jensen and Johannesen 2017; Petersen and Rajan 1994). Consistent with the importance of financial frictions, Tables I.A4 and I.A5 in the Internet Appendix show that the reduction in credit supply is concentrated among firms and individuals that are more informationally opaque, have limited collateral, and are located in regions with few alternative lenders. In line with the idea that borrowers could not simply substitute credit across banks, we find that fewer than 10% of affected firms and individuals in our sample established a new bank relationship in the post-event period, underscoring the difficulties of switching lenders. Moreover, as shown in the Internet Appendix, firms and individuals reliant on exposed banks experience a decline in total debt relative to their peers, confirming that they were unable to offset the contraction in credit from exposed banks by borrowing from unexposed banks.

5. Disentangling the real impact of a credit crunch

Many studies have examined the real economic consequences of disruptions in credit supply. Below, we discuss the three main empirical specifications used in previous studies and their main limitation, after which we present our empirical framework designed to address this shortcoming.

A first strand of research has focused on the impact of *firm-level* credit shocks on *firms*. Typically, the empirical specification used in these studies is of the following form:

$$y_{f,t} = \beta \cdot (Post_t \times Treated_f) + \gamma_f + \gamma_t + \epsilon_{f,t}. \tag{2}$$

where $y_{f,t}$ corresponds to a set of firm-level outcomes such as investment or employment, and $Treated_f$ equals one if a firm's lender contracts credit. γ_f and γ_t are firm and year fixed effects, respectively.

A second strand extends the previous framework by using employer–employee data to study how *firm-level* credit shocks impact *individuals* employed by treated firms. The typical regression model is:

$$y_{i(f),t} = \beta \cdot (Post_t \times Treated_f) + \gamma_i + \gamma_t + \epsilon_{i(f),t}. \tag{3}$$

where treatment is still defined at the firm-level, but the outcomes are individual-level variables, such as wages or the probability of being laid off. Here, γ_i and γ_t denote individual and year fixed effects, respectively.

A third, more recent line of research examines the implications of *individual-level* credit shocks for *individuals*, with a particular focus on consumption responses. These studies typically employ specifications of the form:

$$y_{i,t} = \beta \cdot (Post_t \times Treated_i) + \gamma_i + \gamma_t + \epsilon_{i,t}. \tag{4}$$

where treatment is now defined based on individuals' personal exposure to the credit shock. As before, γ_i and γ_t are individual and year fixed effects, respectively.

The findings from these empirical approaches have substantially advanced our understanding of how credit shocks affect the real economy. For instance, at the firm level, credit shocks have been shown to reduce investment and employment (Chodorow-Reich 2014; Iyer et al. 2014; Huber 2018). These firm-level constraints, in turn, negatively affect employees through higher layoff risk, lower wages, and reduced consumption (Adamopoulou et al. 2024; Berton et al. 2018). At the individual level, higher interest rates or reductions in the supply of credit cards and HELOCs have been shown to reduce consumer spending (Benmelech, Meisenzahl, and Ramcharan 2017; Di Maggio et al. 2017; Jensen and Johannesen 2017; Mian, Rao, and Sufi 2013). Tables I.D1, I.D2, and I.D3 in the Internet Appendix show that we can replicate these established findings when estimated using the corresponding models, supporting the validity of our empirical setup.

However, a limitation of each of the three empirical specifications presented above is that they rely on the assumption that credit shocks affect *either* firms *or* individuals. This assumption is problematic if credit market disruptions simultaneously affect both, which, as discussed earlier, is often the case. Consequently, these empirical models cannot isolate the effect of firm-side credit shocks, household-side credit shocks, and their interaction. Our identification strategy, presented below, is designed to tackle this concern.

5.1. Identification strategy

The richness of our data enables us to overcome the limitations that have constrained prior work and disentangle the impact of a credit market disruption into the effect of individual-side credit shocks, firm-side credit shocks, and their interaction. To this end, we estimate the following triple difference-in-differences model:

$$\begin{aligned} y_{i(f),t} = & \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \\ & \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t} \end{aligned} \tag{5}$$

where i, f, and t refer to individual, firm, and year, respectively. The main outcome variables are individuals' total debt, labor earnings, or consumption expenditures (as defined in Section 3.3). $Post_t$ is a binary variable equal to one after 2008, and $Treated_f$ and $Treated_{i(f)}$ are binary variables equal to one if the relationship bank of firm f or individual i, respectively, relied on foreign wholesale funding before the onset of the global liquidity freeze.[25] The key intuition behind the identification strategy is to leverage the quasi-random exposure of firms and individuals to the global liquidity freeze through pre-determined variation in bank relationships. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively. The error term, $\epsilon_{i(f),t}$, is clustered at the individual-level.

The coefficients of interest are β_1, β_2 and β_3. β_1 captures the effect of firm-side credit constraints on individuals who do not face individual-side credit constraints; β_2 captures the

[25] In principle, we can also build a shift-share instrument, in which the shift component is a binary variable equal to one if a bank was exposed to the foreign wholesale funding market and the share components are the shares of a borrower's credit with each bank in 2007, but ultimately both approaches yield very similar results as most firms and individuals in our sample borrow from a single bank. To facilitate the interpretation of the estimates, we therefore use a treatment dummy in our main results and provide results based on treatment intensity in Table I.A18 in the Internet Appendix.

effect of individual-side credit constraints on individuals employed by firms that do not face firm-side credit constraints; β_3 captures the potential *amplification effect* of being jointly exposed to firm-side and individual-side credit constraints. Put differently, a significant β_3 would indicate that the joint effect exceeds the sum of the two separate effects.

In our most saturated regressions, we include both individual and firm-year fixed effects. The former control for unobserved, time-invariant individual heterogeneity, ensuring that we exploit *within-individual dynamics*. The latter control for unobserved, time-varying firm heterogeneity, ensuring that we exploit *within-firm-year dynamics* by comparing the economic trajectories of individuals employed by *the same firm* prior to the global liquidity freeze. In regressions that include firm-year fixed effects, β_1 is absorbed.[26]

Before turning to the results, it is important to emphasize that our identification strategy does not depend on the dynamics of the global liquidity freeze, other than through differences in individuals' exposure to bank credit shocks. Our identification simply relies on the assumption that in the absence of the credit market disruption, individuals employed by the same firm—differing only in whether or not they relied on an exposed bank—would have had similar economic trajectories. Thus, any divergence in economic trajectories between these groups can be attributed to the differential transmission of the credit supply shock, rather than to other unobserved confounding factors. We provide several pieces of evidence in favor of the robustness of our design, as discussed in detail below. First, we provide evidence supporting the parallel trends assumption underlying the difference-in-differences framework. Second, we show that our results also hold when controlling for a large set of ex-ante individual characteristics interacted with year dummies. Third, we rule out that our results are driven by confounding channels, such as potential differences in individuals' exposure to local economic shocks, housing prices, or stock market conditions.

5.2. Main results

Our main results are reported in Table 4. Across the different columns, the outcome variables are the natural logarithm of individuals' total debt, labor earnings, and consumption expenditures. For each outcome, we present results with two different sets of fixed effects: one with individual and year fixed effects, and one with individual and firm-year fixed effects. The latter approach is more restrictive as it implies that we compare the economic trajectories of credit constrained versus non-constrained individuals employed by the same firm.

We first focus on the $Post \times Treated_f$ and $Post \times Treated_{i(f)}$ interactions, which capture the isolated effects of firm-side and individual-side credit shocks, respectively. Both coefficients are significantly negative in most specifications. The $Post \times Treated_f$ estimates imply that a firm-side credit shock reduces individuals' labor income and consumption by about 2% and 1%, respectively, while the $Post \times Treated_{i(f)}$ estimates indicate that an individual-side credit shock is associated with a 1% decline in consumption. These effects are *qualitatively* consistent with prior studies, but *quantitatively* smaller, reflecting the fact that earlier work could not disentangle the marginal impact of firm-side and individual-side credit shocks from the impact of

[26] Figure 1 provides a conceptual illustration of the identification strategy underlying Equation (5).

their interaction. For example, when we replicate the approach used in earlier studies, Internet Appendix Table I.D2 shows that firm-side credit shocks reduce labor income and consumption by 4% and 2%, respectively, while Internet Appendix Table I.D3 shows that individual-side credit shocks are associated with a 3% decline in consumption. The larger magnitudes can be attributed to the fact that these estimates combine marginal effects with interaction effects.

The importance of this argument is further underscored when looking at the $Post \times Treated_f \times Treated_{i(f)}$ interaction, which captures the *amplification effect* of being exposed to both individual-side and firm-side credit constraints. Columns (1)–(2) indicate that credit constrained individuals employed by credit constrained firms suffer a small additional decline in debt of around 1% compared to their co-workers. This suggests a total decline in debt of nearly 10% for these individuals. More importantly, columns (3)–(4) indicate that, following the credit crunch, these workers also suffer significantly lower labor earnings, while columns (5)–(6) document a corresponding decline in consumption. These effects are economically meaningful. For instance, individuals whose personal credit constraints coincide with those of their employer experience a total decline in labor earnings and consumption of 5–6% in the aftermath of the credit crunch, compared to a decline of only 1–2% for individuals who either face personal credit constraints or whose employer faces credit constraints.[27]

Together, these findings offer novel evidence that the marginal impact of firm-side and individual-side credit shocks is limited, while their interaction generates amplified real effects. Before we explore the mechanisms behind this result, we examine the main identification assumption.

The main identification assumption underlying our analysis is that, in the absence of the shock, the economic outcomes of the individuals would have evolved along parallel trajectories. To assess the validity of this assumption, we estimate dynamic difference-in-differences models, with results presented in Figures 4a–4c. These figures show no significant pre-treatment differences in debt, labor earnings, or consumption expenditures across the different groups of individuals. However, following the shock, individuals whose personal credit constraints coincide with those of their employer exhibit significantly lower levels of debt, labor earnings, and consumption than their peers. Moreover, the figures reveal that the negative effects of the credit market disruption are substantially more *persistent* for these individuals. For example, while individuals whose employer experiences a credit shock see a decline in labor earnings and consumption—e.g., due to wage reductions or job loss—their outcomes largely return to pre-shock levels within the subsequent three years. In contrast, individuals who simultaneously face individual-side and firm-side credit constraints experience a much more persistent impact, with labor earnings and consumption remaining substantially below pre-shock levels even three years after the shock.

To further address potential concerns about omitted variable bias, we re-estimate our regression model, including a rich set of pre-shock covariates interacted with year dummies.

[27]It is important to emphasize that the coefficient estimates from Equation (5) capture a partial equilibrium effect, as any general equilibrium dynamics are absorbed by the year (or firm-year) fixed effects. For instance, if the credit supply shock caused individuals employed by credit constrained firms to reduce spending—thereby amplifying the economic downturn and adversely affecting households more broadly—such indirect effects would be absorbed by the year (or firm-year) fixed effects. β_1, β_2, and β_3 from Equation (5) are identified by comparing outcomes between affected versus non-affected individuals, employed by affected versus non-affected firms, after versus before the credit market disruption. We return to this point in Section 5.5, where we attempt to assess if micro-level mechanisms we document have implications for the aggregate transmission of credit shocks.

Specifically, we control for ex-ante age, gender, marital status, education, household size, homeownership, and debt-to-income. The results, presented in Table A2 in the Appendix, are similar to our baseline results, alleviating concerns that our findings are driven by observable differences across the different types of individuals.[28]

Overall, our results suggest that the effects of isolated firm-side and individual-side credit shocks on individuals' labor income and consumption are modest. However, the interaction of the two generates amplified real effects. Specifically, individuals whose personal credit constraints coincide with those of their employer experience income and consumption losses that are nearly twice as large as what would be expected from simply adding the effects of firm-side and individual-side credit shocks. As we show in the following sections, this amplification effect can be attributed to the fact that individuals' personal credit constraints hinder consumption smoothing and job search, thereby amplifying the adverse effects of their employer's financial distress. For instance, credit constrained employers typically lay off workers, in which case personal credit constraints impede those workers from engaging in effective job search, thereby reducing their long-term labor income. Similarly, credit constrained employers often reduce wages, in which case personal credit constraints hinder workers from smoothing consumption

Our findings also inform how to interpret earlier studies. Several studies have shown that firm-side and individual-side credit shocks have real effects. However, most studies estimate average effects, without separating (i) firm-only shocks, (ii) individual-only shocks, and (iii) joint shocks. For example, Adamopoulou et al. (2024) find that individuals employed by credit constrained firms in Italy experienced labor income losses of 3–4% following the global liquidity freeze. Jensen and Johannesen (2017) show that individuals in Denmark whose relationship bank was exposed to the interbank market freeze saw a roughly 4% drop in consumer spending. Similarly, several U.S. studies document that individuals' inability to borrow during the global financial crisis led to large declines in consumption (e.g., Benmelech, Meisenzahl, and Ramcharan 2017; Mian and Sufi 2010). These settings are comparable to Norway in that firms and households are highly bank-dependent, and many of these papers leverage quasi-experimental variation in credit supply resulting from the global liquidity freeze. Our results suggest that such designs likely capture a mix of firm-side, individual-side, and joint shocks. Future research should distinguish between these three components to better understand the mechanisms through which financial disruptions affect the real economy.

5.3. Displaced and non-displaced workers

Having established that individuals' personal credit constraints amplify the impact of their employer's credit constraints, we explore the mechanisms behind this result. To do so, we start by investigating the job search behavior of *displaced* workers, as firm-side credit constraints

[28]Two additional robustness checks are worth mentioning. First, to further mitigate concerns that pre-existing differences across different types of individuals might influence our findings, we apply a propensity score matching (PSM) approach. We match individuals based on their ex-ante wealth, debt-to-income ratio, age, and location, retaining the three closest matches, and then re-estimate our baseline model. The results based on this matched sample are reported in Table I.A8 in the Internet Appendix, confirming that our main findings hold. Second, we perform a falsification test by constructing a placebo dataset in which individuals are randomly linked to firms and their respective banks. Table I.A9 in the Internet Appendix shows that re-estimating our baseline model using this dataset yields no significant results, alleviating concerns that our findings are spurious. Section 6 discusses additional robustness checks that further support the validity of our results.

typically lead to layoffs (Berton et al. 2018; Chodorow-Reich 2014) and individual-side credit constraints play a critical role in individuals' ability to engage in job search (Chetty 2008; Herkenhoff, Phillips, and Cohen-Cole 2023; He and Le Maire 2023; Rendon 2006).

Our data do not contain a direct measure of firing, but we can approximate it with reasonable precision following established approaches from prior research (Caggese, Cuñat, and Metzger 2019). Specifically, we define displaced workers as those who receive non-zero weeks of unemployment benefits in either the year of separation or the following year. We then restrict our sample to individuals who become displaced after the onset of the global liquidity freeze and assess whether the subsequent reallocation of these individuals into new firms varies depending on whether they faced a personal credit shock. If credit constraints impair individuals' job search, we would expect worse post-displacement outcomes for displaced individuals facing personal credit constraints.

We examine three key labor market outcomes, namely the duration of the job search, the wage earned at the new employer, and the occupations into which workers transition. The results are reported in Table 5. Note that, by including firm-year fixed effects, we compare the labor market outcomes of credit constrained versus non-constrained workers who are laid off by *the same employer* (which is crucial considering that labor market outcomes after job switching have been shown to depend on the previous employer, see Bonhomme, Lamadon, and Manresa 2019).[29] For brevity, we only present estimates from regression with individual and firm-year fixed effects.[30] First, column (1) shows that credit constrained workers reduce their job search duration by nearly one month, corresponding to a 20% reduction relative to the average job search duration. Second, column (2) indicates that, conditional on re-employment, these workers accept positions with 10% lower wages. Third, column (3) shows that credit constrained workers are about 4 percentage points less likely to switch occupations, corresponding to a 30% decrease relative to the average probability of occupational switching, suggesting they may face greater difficulties in investing in re-skilling or professional development necessary to change jobs. Finally, in addition to the labor market outcomes of displaced workers, we examine the impact on their spending behavior. Column (4) shows that the effects documented above translate into a 16% decline in consumption. Section 6.1 provides additional analyses showing that these results are not driven by potential changes in labor supply.

Overall, our findings are consistent with the job search models of Chetty and Szeidl (2007) and Shimer and Werning (2007), which suggest that credit constraints lower job seekers' reservation wages and job search scope, making them more likely to accept lower-paying job offers because they cannot afford to wait for better opportunities (see also Basten, Fagereng, and Telle 2014; Gruber 1997). More broadly, the results highlight the importance of credit frictions in shaping the labor earnings consequences of individuals' job loss (Lachowska, Mas, and Woodbury 2020; Schmieder, Von Wachter, and Heining 2023).

[29] Overall, approximately 5% of workers in our sample are displaced following the liquidity freeze. The estimation sample in Table 5 is reduced due to the inclusion of firm-year fixed effects; nonetheless, the resulting sample remains larger than that employed in most studies on mass layoffs.

[30] Note that, we estimate the coefficient on the interaction term of $Post \times Treated_{i(f)}$ in this analysis as we do not distinguish between displaced workers from affected versus non-affected firms. Taking as given that credit constrained firms are more likely to lay off workers (see, e.g., Internet Appendix Table I.D2 and Chodorow-Reich 2014), personal credit constraints following displacement may help explain the amplification effect in our baseline results.

Table 6 focuses on the subsample of *non-displaced* workers. Columns (1) and (2) indicate that the probability of being laid off or the wage reduction experienced by constrained and non-constrained workers is similar, suggesting that credit constrained firms do not offer differential job or wage insurance to employees who face a personal credit shock (Guiso, Pistaferri, and Schivardi 2005, 2013). Further, columns (3) and (4) show that constrained workers are significantly less likely to voluntarily switch employers and experience substantially larger declines in consumption relative to their co-workers. These patterns are consistent with the notion that personal credit constraints hinder both job mobility and consumption smoothing in response to wage cuts or employment uncertainty (Gross and Souleles 2002; Zeldes 1989).

5.4. Heterogeneity

To substantiate the interpretation that individuals' personal credit constraints amplify the impact of their employer's financial distress by impeding consumption smoothing and job search, we leverage the richness of our data to explore heterogeneity across individuals. Intuitively, we expect the effects to be more pronounced among individuals who are more vulnerable to negative income or credit shocks. To test this, we re-estimate our baseline regressions across three distinct subsamples.

First, we split the sample based on individuals with low and high liquidity buffers. Liquid assets can help individuals insulate their consumption from the effects of a negative income shock, such as job loss or wage cuts. Following prior studies (Aydin 2022; Kaplan, Violante, and Weidner 2014), liquid assets include checking and savings accounts, and holdings of stocks, bonds, and funds. It is worth noting that 25% of Norwegian individuals in our sample lack liquid assets equivalent to one month of income.[31] We hypothesize that individuals with lower liquidity buffers would be more adversely affected, as they have fewer financial resources to absorb the shock. Consistent with this conjecture, Panel A of Table 7 confirms that the effect is significantly larger for individuals with ex-ante low liquidity. A potential concern is that by splitting the sample based on individual-level liquidity, we may overlook the role of intra-household risk sharing. To address this, Panel A of Internet Appendix Table I.A6 replicates the analysis using household-level liquidity measures, showing that the results remain similar.

Second, we split the sample based on the number of bank relationships maintained by individuals, as those who have more than one bank relationship may be better able to obtain credit from another bank (Detragiache, Garella, and Guiso 2000). Panel B of Table 7 supports this intuition, illustrating that the drop in labor earnings and consumption is more pronounced for individuals with a single bank relationship. Given that individuals may access credit through the lender of their spouse, Panel B in Internet Appendix Table I.A6 shows that our results are similar when we measure the number of bank relationships at the household-level.

Third, we compare individuals with low versus high ex-ante debt-to-income. In principle, a high debt-to-income ratio might signal excessive leverage, which could make individuals more financially vulnerable when facing their employers' or their personal credit constraints (Gross

[31] This broadly aligns with survey evidence showing that 20% of Norwegian adults would be unable to cover an unexpected expense of NOK 20,000 (USD 1,800) without selling assets or borrowing money (Statistics Norway 2024).

and Souleles 2002; Mian, Rao, and Sufi 2013). Panel C of Table 7 shows that the drop in labor earnings and consumption is larger for individuals with a higher debt-to-income ratio, consistent with our expectation. As before, this result holds when we measure debt-to-income at the household-level, as reported in Panel C of Internet Appendix Table I.A6.

In Table I.A7 of the Internet Appendix, we further show that the effects are stronger for blue-collar workers, and slightly more pronounced for those in tight labor markets and those employed by small firms.[32]

Overall, the cross-sectional results are consistent with the idea that individuals' personal credit constraints amplify the effects of their employers' financial distress, leading to a larger and more persistent decline in labor earnings and consumption.[33] In additional analyses discussed in Section 6.1, we show that the results cannot be attributed to alternative explanations.

5.5. Aggregate implications

Our baseline results capture the differential impact of the credit market disruption on affected versus non-affected individuals employed by affected versus non-affected firms. While these individual-level regressions enable strong empirical identification and allow us to uncover the micro-level mechanisms underlying our results, they do not speak to the aggregate consequences of the mechanisms we document. General equilibrium effects may arise, for example, through aggregate demand or local labor market spillovers (Giroud and Mueller 2017; Mian, Rao, and Sufi 2013; Mian and Sufi 2014), which may indirectly impact other firms and individuals that were not directly exposed to the credit crunch. In theory, the direction of the spillovers is ambiguous. On the one hand, declines in local demand and agglomeration forces may negatively impact non-affected firms. On the other hand, lower local wages may benefit non-affected firms by reducing labor costs.[34]

To assess the relevance of such spillovers, we build on the approach of Mian and Sufi (2014) and Giroud and Mueller (2017), among others, and examine how the credit market disruption affects local economic activity, depending on the extent to which the firms or individuals or both in a given locality were exposed to the credit crunch. The key idea underlying this approach is to treat municipalities as isolated economic units. Although this is a strong assumption, one could argue that this is reasonable for many Norwegian municipalities, as Norway's geographic features imply that many municipalities are relatively isolated and function as distinct economic units. We therefore aggregate the data to the municipality-level and run the following regression:

$$\begin{aligned} y_{m,t} =& \delta_1 \cdot (Post_t \times Firm\ Exposure_m) + \delta_2 \cdot (Post_t \times Individual\ Exposure_m) + \\ & \delta_3 \cdot (Post_t \times Joint\ Exposure_m) + \lambda \cdot C_{m,pre} + \gamma_m + \gamma_t + \epsilon_{m,t} \end{aligned} \tag{6}$$

where $y_{m,t}$ corresponds to total consumption, firm output, or employment in municipality m

[32]We classify workers as white-collar if their occupation falls within managerial, professional, or administrative categories (ISCO groups 1–4), and as blue-collar if they belong to craft, machine operation, or elementary occupations (ISCO groups 5–9). We define tight labor markets as those with below-average unemployment prior to the global liquidity freeze, and we define small firms as those with below-average total assets.

[33]Since the effects are stronger in situations where theory would predict that they would matter more, the findings also suggest that the results are not driven by mechanical effects due to measurement error (see Huber 2023).

[34]For a more extensive discussion on the potential direction and mechanisms of the spillovers, see Eggertsson and Krugman (2012), Guerrieri and Lorenzoni (2017), Ellison, Glaeser, and Kerr (2010), Giroud and Mueller (2017), and Mian and Sufi (2014).

in year t. The exposure measures are defined in detail below. In essence, $Firm\ Exposure_m$ and $Individual\ Exposure_m$ capture the share of individuals in municipality m exposed to the credit market disruption through firm-side and individual-side credit shocks, respectively. $Joint\ Exposure_m$ measures the share of individuals in municipality m who were simultaneously exposed to both personal credit shocks and those experienced by their employer. $C_{m,pre}$ is a vector of pre-event controls interacted with year dummies (including the natural logarithm of the local population, local average income-per-capita, local unemployment, and local bank concentration), and γ_m and γ_t are municipality and year fixed effects, respectively. Standard errors are clustered at the municipality-level.[35]

The exposure measures are defined as follows:

$$Firm\ Exposure_m = \frac{1}{I_m} \sum_{i \in I_m} (\mathbf{1}\{Treated_i = 0\}) \wedge (\mathbf{1}\{Treated_{f(i)} = 1\}) \tag{7}$$

$$Individual\ Exposure_m = \frac{1}{I_m} \sum_{i \in I_m} (\mathbf{1}\{Treated_i = 1\}) \wedge (\mathbf{1}\{Treated_{f(i)} = 0\}) \tag{8}$$

$$Joint\ Exposure_m = \frac{1}{I_m} \sum_{i \in I_m} (\mathbf{1}\{Treated_i = 1\}) \wedge (\mathbf{1}\{Treated_{f(i)} = 1\}) \tag{9}$$

where I_m denotes the sets of individuals in municipality m, and $Treated_i$ and $Treated_f(i)$ are indicators for whether the relationship bank of individual i or her employer f, respectively, was exposed to the global liquidity freeze. The distribution of the three exposure measures is presented in Figures A2a–A2c in the Appendix, illustrating that there is substantial variation. The correlation between any of the three exposure measures does not exceed 0.25.

The specification outlined in Equation (6) allows us to separately identify aggregate effects due to firm-side credit constraints, individual-side credit constraints, and their joint occurrence (similar to the logic of our individual-level identification strategy outlined in Equation (5)). Specifically, δ_1 captures how economic outcomes in municipality m evolve after the shock in proportion to the share of individuals employed by affected firms (but who do not face personal credit constraints). This term indirectly also accounts for the fact that credit constrained firms may reduce output or employment, which could affect other firms in the municipality through input-output linkages or local demand spillovers.[36] δ_2 captures the effect of the shock on local economic outcomes in proportion to the share of individuals who are directly affected by the credit market disruption, reflecting a potential decline in household consumption due to tighter personal credit constraints which may in turn decrease local demand for goods and services. Finally, δ_3 captures the effect of credit shocks on local economic outcomes in proportion to the share of individuals whose personal credit constraints coincide with their employer's credit constraints. Essentially, the intuition is that such interactions may generate feedback loops, whereby constrained employers reduce employment or wages, which in turn leads to further consumption declines among already credit constrained individuals, thereby amplifying the local economic downturn (e.g., Kekre 2023; McKay and Reis 2021; Ravn and Sterk 2017).

[35] To minimize measurement error, we exclude municipalities with fewer than 500 individuals or fewer than 50 firms.

[36] Since large firms tend to generate larger spillovers, a benefit of measuring firm exposure based on the number of individuals employed by affected firms is that large firms are given greater weight by construction (as they tend to employ more individuals).

The results of estimating Equation (6) are presented in Table 8. The key outcome variables are municipality-level consumption, firm output, and employment. For ease of interpretation, all exposure measures are standardized to have a mean of zero and a standard deviation of one. First, we find a significantly negative coefficient for the interaction between the post-treatment indicator and the firm exposure measures across all columns. This implies that credit shocks have adverse real effects operating through corporate credit channels, consistent with prior work. For instance, higher firm exposure may result in layoffs or disruptions in local input–output linkages, thereby dampening local demand and economic activity. By contrast, we do not find statistically significant effects for the interaction with the individual exposure measure, suggesting that individuals' exposure to the credit shock alone does not directly translate into aggregate economic consequences.

Interestingly, the interaction between the post-treatment indicator and the joint exposure measure is also significantly negative—and notably larger in absolute magnitude than the corporate exposure interaction. Specifically, a one standard deviation increase in the joint exposure measure reduces total consumption, output, and employment by approximately 4–5%, compared to a 2% reduction associated with a one standard deviation increase in corporate exposure. This result implies that accounting for the interaction between firm-side and household-side credit constraints is crucial, as it generates amplified real effects.

Failing to account for this interaction may lead researchers to conclude that any firm-side or household-side credit shock generates large economic effects. To illustrate this point, Table A3 in the Appendix reports results obtained under the assumption that credit shocks affect either firms or individuals—without accounting for the fact that credit shocks may simultaneously affect both—as prior research has typically done.[37] Based on this approach, we find that both corporate and individual credit exposures are economically important in explaining variation in economic activity across municipalities following the credit crunch. However, by ignoring that credit shocks may affect (i) only firms, (ii) only households, or (iii) both, this approach obscures the fact that the largest economic effects are driven by credit shocks that jointly constrain individuals and their employers. Thus, while prior research has offered opposing views on the relative importance of firm-side versus household-side credit constraints in shaping the economic impact of credit market disruptions,[38] our results reconcile these perspectives by showing that it is the *interaction* between the two that plays a crucial role.

To further analyze the spillovers and underlying mechanisms, we perform two additional tests. First, we re-estimate the results while excluding directly affected individuals and firms from the calculation of the outcome variables. For example, we calculate a municipality's total consumption based solely on the consumption expenditures of individuals who are not directly affected by the credit crunch. As shown in Table A4 in the Appendix, our results remain robust,

[37]By assuming that credit shocks affect either firms or only individuals, the definitions of $Firm\ Exposure_m$ and $Individual\ Exposure_m$ differ slightly from those described earlier. Specifically, $Firm\ Exposure_m$ corresponds to the share of individuals employed by affected firms, regardless of whether these individuals themselves face personal credit shocks. Conversely, $Individual\ Exposure_m$ corresponds to the share of individuals experiencing personal credit shocks, regardless of whether their employer is affected by credit shocks.

[38]One strand of research has emphasized firm-side frictions, arguing that tighter credit to firms leads to deep and persistent declines in economic activity by weakening firm balance sheets and reducing employment (e.g., Giroud and Mueller 2017; Huber 2018). Another strand has focused on household-side frictions, arguing that contractions in household credit reduce consumption which lowers aggregate demand and thereby depresses overall economic activity (e.g., Mian and Sufi 2010, 2014).

indicating that affected firms and individuals generate spillover effects on non-affected firms and individuals (Huber 2023).[39]

Second, we re-estimate the result separately for municipalities with a high versus low share of non-tradable industries. Theoretically, we would expect the spillovers arising from the interaction between individual-side and firm-side credit shocks to be more important for non-tradable industries which heavily rely on local demand (Giroud and Mueller 2017; Mian and Sufi 2014; Moretti 2010). We follow the classification procedure of tradable and non-tradable industries proposed by Mian and Sufi (2014), and re-estimate Equation (6) separately for municipalities with an above- or below-average share of firms operating in tradable versus non-tradable industries.[40] The results are reported in Table A5 in the Appendix. Consistent with our hypothesis, the interaction between firm-side and individual-side exposure is substantially stronger in municipalities with a high share of firms operating in non-tradable industries, which are more dependent on local demand.

Finally, we assess the external validity of our results in a cross-country setting, exploiting heterogeneity in firms' and households' reliance on bank debt and the occurrence of banking crises. While country-level data do not allow us to precisely measure the exposure of individual workers or firms to credit shocks, they enable us to test a clear aggregate implication of our mechanism: assuming that banking crises are episodes of broad contractions in credit supply, their real effects should be especially severe in economies where both firms and households enter the crisis with high dependence on bank debt. We test this prediction in Internet Appendix E using data from the Global Macro Database and the Global Credit Project (Müller and Verner 2024; Müller et al. 2025). Table I.E3 provides evidence consistent with our conjecture, showing that banking crises are associated with the largest declines in economic activity in countries where both firms and households are ex-ante more reliant on debt.

Taken together, our findings provide novel evidence that joint firm-side and household-side credit constraints amplify the economic impact triggered by credit market disruptions. This amplification mechanism may be particularly important in episodes in which banks sharply cut lending to both firms and households, such as during the global financial crisis (Bernanke 2018; Gertler and Gilchrist 2018).

6. Extensions

6.1. Alternative channels

A potential concern regarding the robustness and proposed interpretation of our main results is the existence of a confounding factor that might explain why individuals whose personal credit

[39] Our baseline difference-in-differences specification relies on the stable unit treatment value assumption (SUTVA), which requires that individuals in the control group are unaffected by the credit market disruption. As we find that control individuals are indirectly affected through local demand spillovers from treated individuals, this assumption may be violated and the estimates from Equation (5) may be biased. To address this concern, we follow Berg, Reisinger, and Streitz (2021), who propose a framework that explicitly accounts for spillover effects by controlling for interactions between treated and untreated individuals as a function of treatment intensity within a group. This analysis, reported in Internet Appendix F, shows that spillovers from treated to control individuals are present, but that accounting for these spillovers leaves our main conclusions largely unchanged.

[40] Specifically, using firm-level import-export data from the Norwegian tax authorities, we classify an industry as tradable if the sum of its exports is at least USD 10,000 per worker (NOK 59,000 at the average 2008 exchange rate) or USD 0.5 billion in total (NOK 2.9 billion at the average 2008 exchange rate). The retail and restaurant industry are classified as non-tradable.

constraints coincide with those of their employer experience a substantially larger decline in consumption and labor earnings following the credit crunch. While our fixed effects structure and the absence of pre-trends mitigate this concern to a large extent, one might still worry that these individuals were disproportionately exposed to other factors. For instance, perhaps they lived in localities with weaker labor markets or more adverse housing price dynamics. Alternatively, perhaps they made poorer stock market investments prior to the global liquidity freeze, leaving them more exposed to the stock market decline in the post-event period. Below, we present several analyses to rule out these and many other alternative explanations, supporting the robustness and proposed interpretation of our findings.

First, a potential alternative explanation for our findings involves housing wealth shocks. That is, if some individuals experience larger declines in housing prices relative to their peers, the resulting loss in housing wealth may reduce their demand for goods and services (Mian and Sufi 2014; Mian, Rao, and Sufi 2013) or lead to adjustments in their labor supply (Bernstein 2021; Donaldson, Piacentino, and Thakor 2019). Relatedly, some individuals might be more likely to face a "housing lock" effect, whereby declining housing prices could reduce individuals' ability to fund a down payment for a new home and thereby constrain their geographical mobility in searching for new employment opportunities (Brown and Matsa 2020; Fonseca and Liu 2024; Gopalan et al. 2021).

Although Norway did not experience a housing crisis like the U.S. or Spain, three additional analyses mitigate potential concerns that our results are driven by changes in housing asset valuations. First, as mentioned earlier, Appendix Table A2 shows that our results are robust to controlling for individuals' pre-treatment debt-to-income ratio and homeownership status, among others, interacted with year fixed effects (He and Le Maire 2023). Second, as shown in Internet Appendix Table I.A10, our main results remain robust when we strengthen the identification strategy by replacing firm-year fixed effects with firm-residential municipality-year fixed effects. This specification ensures that we compare workers from the same firm who live in the same municipality and, hence, are subject to the same local housing price trends. More broadly, this approach mitigates concerns that our results could be driven by differences in, e.g., local government support, business uncertainty, credit market characteristics, or labor market conditions. Third, Appendix Table I.A11 demonstrates that our baseline results remain robust when we restrict our sample to renters, a subgroup of individuals for whom the housing lock effect is irrelevant. These additional analyses imply that it is unlikely that our results can be attributed to housing lock effects, household debt overhang, or other housing asset valuation channels.

Next, we examine whether differences in geographic mobility, hours worked, wage volatility, or spousal income can account for the observed effects. We first analyze whether affected individuals from affected firms are less likely to relocate. We conduct this analysis separately for non-displaced and displaced workers, as reported in Panels A and B of Internet Appendix Table I.A12. Column (1) shows that displaced individuals facing personal credit constraints exhibit lower geographic mobility, consistent with the idea that credit constraints limit the ability to search for and purchase housing—and to relocate to jobs—in more distant locations. Among non-displaced individuals, there is no significant difference in individuals' geographic mobility. Second, we test if affected individuals from affected firms reduce their working hours, potentially explaining their decline in labor earnings. Column (2) shows no significant change in hours worked, neither for displaced nor for non-displaced individuals. Third, we explore wage volatility. As documented in Section I.A12, credit constrained workers are less likely to switch occupations. While this appears to be associated with lower labor earnings, it may also contribute to greater job stability (Busch 2020). To assess this possibility, we compute the two-year rolling standard deviation of annual wages. Column (3) of Internet Appendix Table I.A12 show no significant difference in wage volatility, suggesting that earnings losses are not driven by transitions into lower-paying but more stable jobs. Lastly, we test whether spousal labor supply adjusts in response to the shock. For example, if labor earnings increase disproportionately among spouses of credit constrained individuals employed by credit constrained firms, the latter may respond by reducing their own labor supply. However, column (4) show no evidence of significant changes in spousal income, ruling out this channel.

Another potential alternative explanation for our results is that affected firms might differentially transmit the credit shock to workers reliant on exposed versus non-exposed banks. For example, if individuals reliant on exposed banks are more likely to experience wage cuts or layoffs after their employer faces a credit shock, this could account for the observed declines in labor earnings and consumption among these individuals. Table 6 presented earlier addresses this potential concern. Column (1) in this table shows that individuals reliant on exposed versus non-exposed banks are equally likely to be laid off if their employer faces a credit shock. Furthermore, column (2) of the table shows that, among non-displaced workers, these groups do not experience significantly different wage reductions. Nevertheless, workers whose personal credit constraints coincide with those of their employer exhibit a smaller likelihood of voluntarily switching employers and a larger reduction in consumption relative to their co-workers, as reported in columns (3) and (4) of the table. Since wage reductions are comparable across groups, the consumption gap likely reflects differences in consumption smoothing capability, indicating that personal credit constraints limit these individuals' ability to maintain consumption in the face of wage reductions and volatility imposed by their credit constrained employer.

Extending the previous argument, individuals holding different occupations within the same company may experience varying exposure to the firm's financial conditions. This could raise concerns if individuals whose personal constraints coincide with those of their employer are inherently more likely to occupy roles that are either made redundant during a credit crunch or face greater difficulty in securing alternative employment. To address this, Table I.A13 in the Internet Appendix presents specifications that incorporate firm-occupation-year fixed effects. This approach allows us to compare the economic trajectories of individuals who were ex-ante employed in the *same firm* with the *same occupation* (e.g., two engineers working at the same energy company). As shown in Table I.A13, this leads to a reduction in sample size (due to the exclusion of singleton groups), but our results remain robust.

Given that Norway is a small open economy with a large tradable sector—particularly the oil sector—a potential concern is that our results are confounded by firms' exposure to declining foreign demand or to the fall in oil prices following the 2008 global financial crisis (Amiti and Weinstein 2011). While the fixed effects included in our baseline regressions largely rule out that these factors affect our findings, Tables I.A14 and I.A15 in the Internet Appendix show that our results remain robust when excluding firms engaged in direct export or import activities, as well as when excluding municipalities with high oil-sector intensity.[41]

Another potential confounding factor is the drop in stock prices during our sample period, as individuals adjust consumption in response to stock market returns and losses (Di Maggio, Kermani, and Majlesi 2020). If individuals whose personal bank and their employer's bank were exposed to the global liquidity freeze were also more likely to incur stock market losses, this could partly explain the decline in consumption documented in our main results. Table I.A16 in the Internet Appendix addresses this concern, showing that our findings remain robust when excluding individuals who held stocks at any point during the sample period.

Finally, one may wonder whether our results are driven solely by single-branch localities. In these localities, firms and individuals are more likely to share the same bank, so that individuals are more likely to be exposed to both firm-side and individual-side credit constraints if that bank is forced to cut credit. Moreover, firms and individuals in single-branch localities would have greater difficulty finding alternative lenders in the event of a credit market disruption, which could amplify the adverse effects of the credit crunch. To examine this, Table I.A17 in the Internet Appendix re-estimates our baseline regression excluding single-branch municipalities. The coefficients are slightly smaller—consistent with the notion that a lack of alternative lenders amplifies our baseline effects—but remain statistically and economically significant, confirming that our results are not a single-branch locality phenomenon.

6.2. Measurement choices and empirical specification

In our baseline analysis, we follow the approach of Jensen and Johannesen (2017) by defining a firm's or individual's relationship bank as the institution with which they hold the largest loan or deposit balance. In practice, this classification does not affect many firms or individuals, as most maintain only a single bank relationship.[42] Nonetheless, to address potential concerns that this might influence our results, we re-estimate our baseline results using a shift-share instrument, where the shift component is a binary indicator for exposed banks and the share components reflect the firm's or individual's credit shares across banks. Table I.A18 demonstrates that our results remain robust.

In our baseline analysis, we include individuals who do not actively borrow. For these individuals, we identify their relationship bank as the institution where they hold the largest deposit balance, implicitly assuming that they would turn to this bank for credit if needed and that, if this bank were exposed to the global liquidity freeze, they would face credit constraints.[43]

[41] The two most oil-intensive regions in Norway are Hordaland and Rogaland (Juelsrud and Wold 2024). Rogaland hosts the city of Stavanger, which is often referred to as Norway's oil capital.

[42] Also recall that Panel B of Table 7 presented earlier shows that our results hold (and are more pronounced) for individuals with a single bank relationship, reducing concerns that measurement error in identifying the relationship bank for individuals with multiple bank relationships could bias our findings.

[43] This assumption seems reasonable given that over 70% of first-time borrowers obtain credit from the bank where they previously held their largest deposit balance. Furthermore, our loan-level results indicate that exposed banks reduce credit both at the intensive and extensive margin, implying that, even among individuals with only a deposit relationship, those at exposed banks would be

To address concerns that this might influence our results, Internet Appendix Table I.A19 demonstrates that our results remain robust when we restrict the sample to individuals with outstanding bank credit prior to the global liquidity freeze.

Further, our results are insensitive to alternative regression models or methods of standard error clustering. Internet Appendix Table I.A20 for instance demonstrates that our results remain both quantitatively and qualitatively robust when we estimate the baseline model using Poisson instead of OLS (Chen and Roth 2024), and Internet Appendix Table I.A21 confirms that our results hold if we double cluster standard errors by firms' and individuals' bank rather than clustering standard errors by individual.

6.3. Same bank risk

As discussed earlier, a key channel through which individuals risk simultaneous exposure to individual-side and firm-side credit shocks is by relying on the same bank as their employer. Consequently, one could wonder whether individuals who had the same relationship bank as their employer prior to the credit shock respond by subsequently avoiding such overlap—potentially reflecting an effort to reduce the risk of being exposed to joint credit shocks in the future. To examine this, we estimate the following regression:

$$\begin{aligned} Same\ Bank_{i(f),t} = &\beta_1 \cdot (Post_t \times Treated_f \times Same\ Bank_{i(f)}) + \\ &\beta_2 \cdot (Post_t \times Same\ Bank_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t} \end{aligned} \tag{10}$$

which allows us to test whether affected individuals who previously maintained a relationship with the same bank as their affected employer subsequently seek to avoid this after the credit crunch. $Same\ Bank_{i(f)}$ is a binary variable equal to one if individual i and her original employer f shared the same relationship bank before the onset of the global liquidity freeze. The outcome variable is a time-varying function of this indicator. We restrict our sample to displaced workers, since for those who remained employed it is difficult to disentangle whether a change in bank reflects a deliberate effort to avoid having the same relationship bank as their employer or a response to a personal credit cut. In addition, for those who remained employed, a bank change may arise from a switch initiated by either the worker or the employer, further complicating our analysis. By focusing on displaced workers, we attempt to isolate the sorting margin—namely, whether individuals who shared a bank with their former employer are subsequently more likely to join firms that use a different bank.

The results, reported in Internet Appendix Table I.A22, indicate that individuals who relied on the same bank as their credit constrained employer are not significantly more likely to join firms with a different relationship bank than them post-displacement. This finding is consistent with the view that individuals may not be aware of their employers' bank relationship and, consequently, do not intentionally choose to have a relationship with the same or a different bank than their employer. An important implication that follows from this is that, while firms in some countries encourage employees to bank with the same institution to streamline

less likely to obtain credit compared to those at non-exposed banks. This may affect consumer spending, among others, as even individuals who do not currently use bank credit may adjust their consumption in anticipation of future credit constraints (Deaton 1992; Jappelli and Pistaferri 2010).

payroll processes (World Bank 2016), our results underscore that this practice may inadvertently increase employees' vulnerability to credit shocks.

6.4. Shock transmitters versus absorbers

One might wonder whether banks' engagement in both corporate and retail lending could create incentives to mitigate, rather than amplify, the impact of credit shocks. If banks jointly optimize their corporate and retail loan portfolios, they might for instance consider that cutting credit to a firm could adversely affect its workers.[44] If those workers are also retail customers, the bank may recognize that a firm's credit constraints could raise default risk among employees, as job loss is a key driver of retail loan defaults (Ganong and Noel 2023; Keys 2018). This suggests that banks may protect firms whose employees are retail customers.

To empirically test this, we examine whether banks adjust their lending behavior differentially based on the extent to which a firm's employees are retail customers of the bank. Specifically, we construct a measure that captures the proportion of a firm's employees who are retail customers of the bank, relative to the bank's entire retail loan portfolio:

$$Retail\ Credit_{b,f,t} = \frac{\sum_{i \in I_f} Retail\ Credit_{b,i,t}}{\sum_{i \in I_b} Retail\ Credit_{b,i,t}} \tag{11}$$

where $Retail\ Credit_{b,i,t}$ is the outstanding credit of individual i at bank b, I_f is the set of employees working at firm f, and I_b is the set of all individuals who are retail customers of bank b. We also construct alternative measures using banks' local retail lending in a given municipality as the denominator, to account for the possibility that lending decisions are localized (Degryse and Ongena 2005; Petersen and Rajan 2002).[45] In general, the rationale behind the measure is that if a firm's workforce constitutes a significant share of a bank's retail loan portfolio, the bank may internalize the potential adverse consequences of restricting credit to that firm—namely, an increased risk of loan repayment difficulties among its retail customers. Figures I.A4a–I.A4d in the Internet Appendix present the distribution of the measure, indicating that employees of a single firm can account for a substantial share of a bank's local retail loan portfolio, and in some cases, even a significant share of its total retail loan portfolio. For example, Figure I.A4d shows that the employees of a single firm in the top decile of the distribution can represent more than 20% of a bank's local retail loan portfolio.

Based on this, we test whether banks' retail loan exposure to the employees of a given firm influences the banks' corporate lending decisions using an extension of Equation (1):

$$\begin{aligned} y_{b,f,t} =& \beta_1 \cdot (Post_t \times Treated_b \times Retail\ Credit_{b,f}) + \beta_2 \cdot (Post_t \times Retail\ Credit_{b,f}) + \\ & \gamma_{b,t} + \gamma_{f,t} + \epsilon_{b,f,t} \end{aligned} \tag{12}$$

[44] Although, to the best of our knowledge, there is no existing theoretical or empirical evidence on banks' internalization of cross-portfolio spillovers, a growing body of research documents that banks do internalize spillovers in various other contexts. For example, Giannetti and Saidi (2019) find that banks with high industry market shares increase credit provision to firms affected by industry downturns, as they internalize the negative spillovers from potential fire sales. Favara and Giannetti (2017) and Gupta (2022) document that banks internalize house price declines resulting from their lending or foreclosure decisions in regions where they hold a significant share of outstanding mortgage loans. Degryse, Roukny, and Tielens (2025) show that banks internalize the potential negative spillovers of new technologies on their legacy loan portfolios, which hinders innovative firms' access to bank credit.

[45] For Norway, Herpfer, Mjøs, and Schmidt (2023) provide evidence that bank lending is very localized.

where $Retail\ Credit_{b,f}$ captures a bank's retail exposure to the employees of a given firm using the measures defined above. Note that, in contrast to Equation (1), we can include bank-year fixed effects ($\gamma_{b,t}$) in the current regression model, to absorb any bank-specific time-varying unobserved heterogeneity. As before, we also include firm-year fixed effects ($\gamma_{f,t}$) to control for credit demand (Khwaja and Mian 2008). The standard errors are clustered at the bank-level.

The results are reported in Table I.A23 in the Internet Appendix. The outcome variables in all columns are the credit growth between a bank-firm pair. At the top of the table, we clarify how *Retail Credit* is defined in the corresponding regressions. For ease of comparison across columns, the measures of *Retail Credit* are standardized to have a mean of zero and a standard deviation of one. Our results show that, across the different columns, the estimated triple interaction term is statistically insignificant. This implies that banks' retail loan exposure to a firm's employees does not influence their corporate lending decisions.[46] In other words, banks do not appear to sustain credit to firms whose employees are also their retail clients—which could have mitigated the amplification effect documented in our main results.[47]

7. Conclusion

The 2008 global financial crisis renewed economists' and policymakers' interest in understanding why financial disruptions trigger deep and persistent declines in economic activity (Reinhart and Rogoff 2009; Schularick and Taylor 2012). While the extensive body of research that emerged from this has significantly enhanced our understanding of how credit shocks affect firms or households, it has not been able to account for the fact that credit shocks may impact both simultaneously. We overcome this limitation by constructing a novel dataset that links Norwegian employees to their employers and their respective bank relationships. Using these data, we disentangle the economic impact of individual-side credit shocks, firm-side credit shocks, and their interaction.

Our key finding is that credit shocks simultaneously affecting individuals and their employers have a significantly larger impact than the combined impact of credit shocks affecting only individuals or only employers. This amplification effect arises as personal credit constraints hinder individuals' consumption smoothing and job search, thereby amplifying the adverse effects of their employer's financial constraints. We further show that this mechanism explains regional variation in economic activity across Norway following the credit market disruption. Moreover, cross-country evidence confirms our findings, showing that banking crises lead to larger declines in economic activity in economies where both firms and households are ex-ante more debt dependent. Overall, our study provides novel evidence that *joint* firm-side and household-side credit constraints is key in the transmission of credit shocks to the real economy.

Our paper offers several important implications. While prior research has debated the relative

[46] Unreported regressions confirm that these results hold when we use alternative specifications. Specifically, the results remain unchanged when we use banks' deposit instead of credit exposure to a firm's employees, or when we employ indicator variables capturing whether a bank's retail credit exposure to a firm's employees falls within the top decile (in which case we focus on large employers, by construction).

[47] Alternatively, we can examine whether outcomes differ between individuals who rely on the same versus a different bank than their employer. The results, presented in Internet Appendix Table I.A24, show that the declines in labor income and consumption are not significantly different between individuals sharing the same bank as their employer and those using a different one.

importance of credit shocks to individuals versus credit shocks to firms, our findings show that credit shocks that jointly constrain individuals and their employers generate amplified real effects. This insight is crucial, as not all financial disruptions are equal. For example, two credit market disruptions with the same contraction in credit can have very different consequences depending on the overlap between the affected firms and households. For policymakers, these insights may inform the design of stress testing frameworks and macroprudential policies. For future research, our study highlights the importance of accounting for interactions between firms' and households' credit constraints when examining the economic impact of credit shocks.

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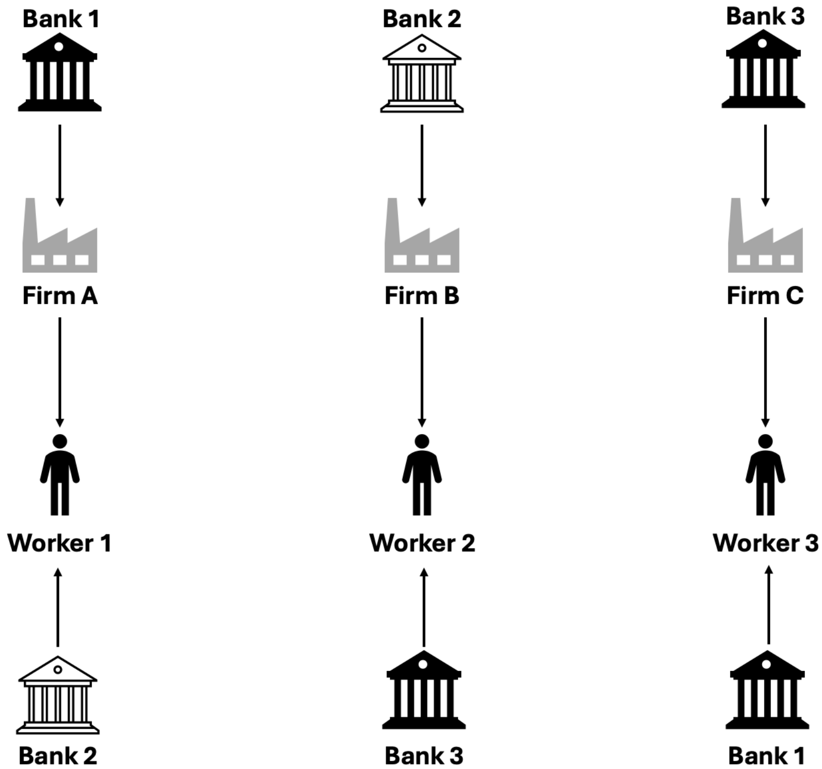
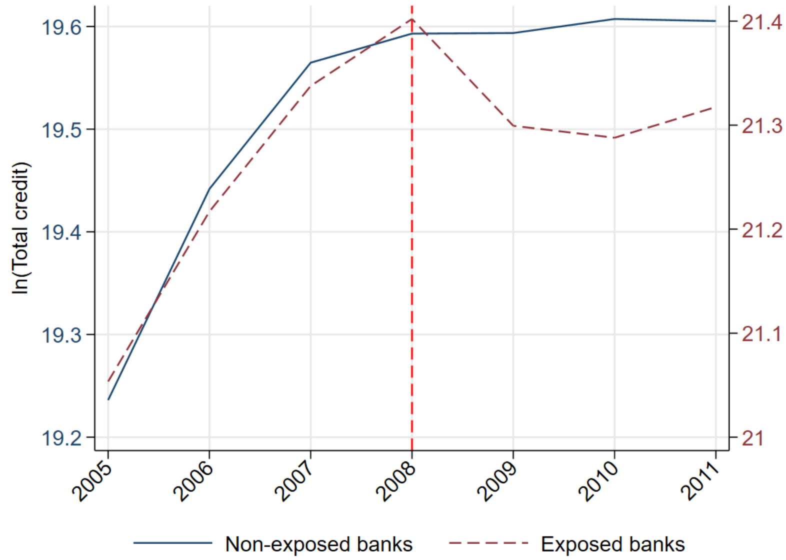
Figures

Figure 1. Conceptual illustration of the empirical setup



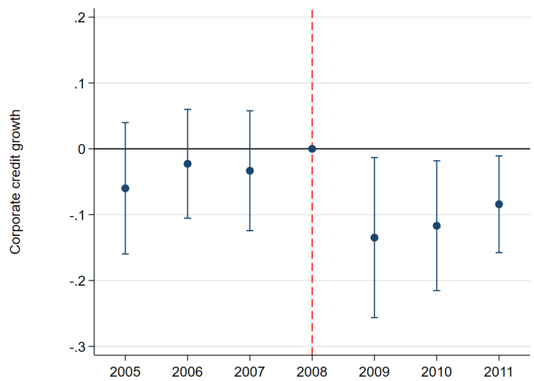
This figure provides a conceptual illustration of our empirical setup. The figure presents three banks, three firms, and three individuals. Bank 1 and Bank 3, shaded in dark, are exposed to the global liquidity freeze, whereas Bank 2, unshaded, is not. Firms A and C maintain a bank relationship with an *exposed* bank, while Firm B has a bank relationship with a *non-exposed* bank. Workers 2 and 3 maintain a bank relationship with an *exposed* bank, while Worker 1 has a bank relationship with a *non-exposed* bank. Our main identification strategy, outlined in Equation (5) in Section 5.1, disentangles the real impact of each of these three channels of credit shock exposure. The coefficient β_1 in Equation (5) captures the effect of firm-side credit constraints, identified by Worker 1, who is affected only through her employer's bank. The coefficient β_2 captures the effect of individual-side credit constraints, identified by Worker 2, who is exposed only through her personal bank. Finally, β_3 captures the *amplification effect* of simultaneously facing individual-side and firm-side credit constraints, as is the case for Worker 3, who is exposed through both her employer's bank and her personal bank.

Figure 2. Evolution of total lending by exposed versus non-exposed banks

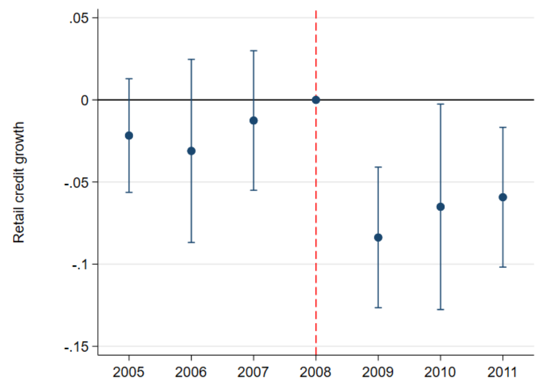


This figure presents the evolution of total lending by exposed banks in red (dashed line) on the right axis and non-exposed banks in blue (solid line) on the left axis before and after the onset of the global liquidity freeze. Exposed banks are defined as those that relied on foreign wholesale funding before the onset of the global liquidity freeze.

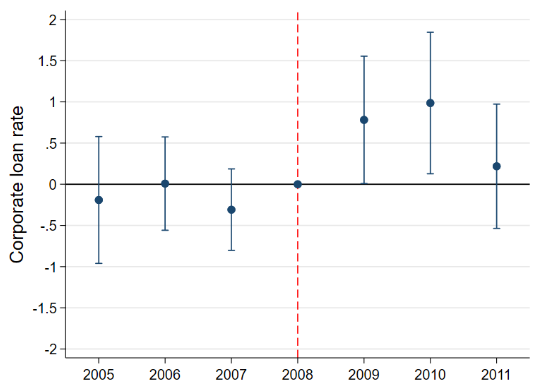
Figure 3. Bank lending by exposed and non-exposed banks following the global liquidity freeze: loan-level dynamic difference-in-differences estimates



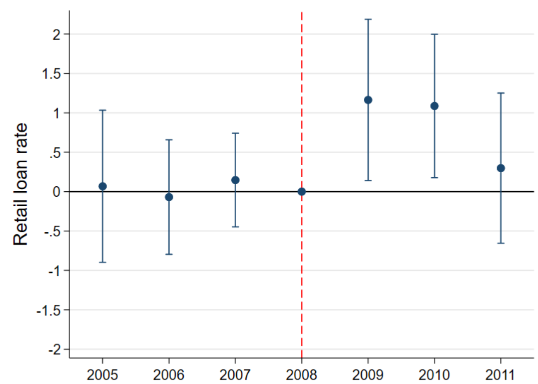
(a) Corporate credit growth



(b) Retail credit growth



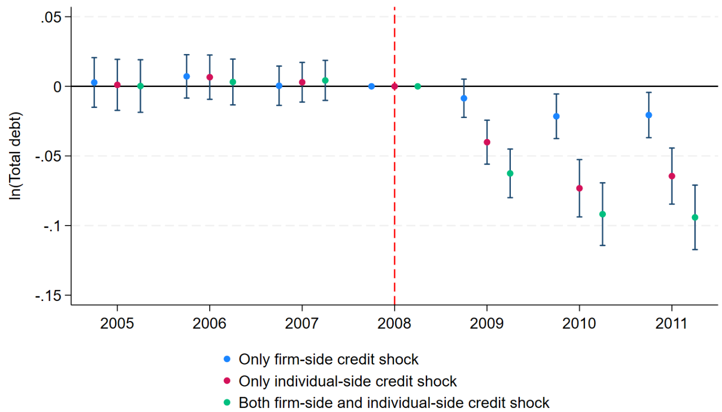
(c) Corporate loan rate



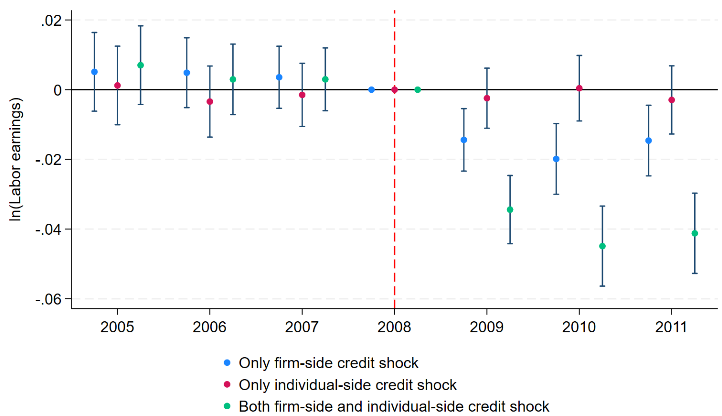
(d) Retail loan rate

This figure presents the dynamic difference-in-differences estimates of the impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks to firms and individuals, after versus before the global liquidity freeze. The y-axis corresponds to the coefficient estimates of β_τ from estimating the following regression model: $y_{b,f,t} = \sum_{\tau=-3,\,\tau\neq 0}^{3} \beta_\tau \cdot (1_{\tau=t} \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_{f,t} + \epsilon_{b,f,t}$ where the dependent variable $y_{b,f,t}$ is credit growth between bank b and firm f from year $t-1$ to year t in Panel (a) and the loan rate charged by bank b to firm f in year t in Panel (c). $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and $\gamma_{f,t}$ are bank and firm-year fixed effects, respectively, and $\epsilon_{b,f,t}$ is the error term. We run similar regressions for bank lending to individuals (in which case index f in the previous equation is replaced by index i). For these regressions, the dependent variables are credit growth between bank b and individual i from year $t-1$ to year t in Panel (b) and the loan rate charged by bank b to individual i in year t in Panel (d). The x-axis corresponds to years. The vertical bars represent confidence intervals at the 95% level. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level.

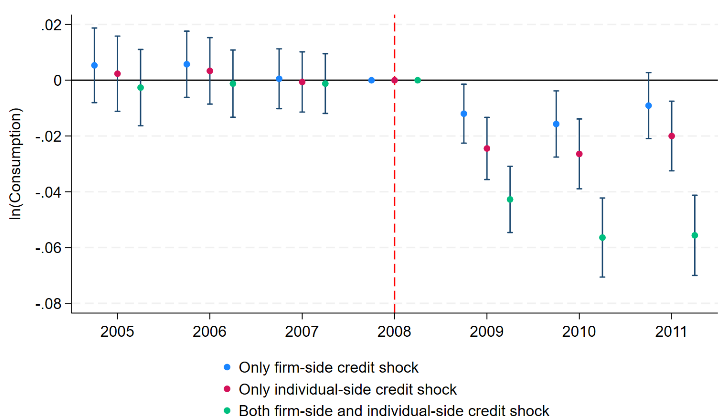
Figure 4. Main results: Dynamic difference-in-differences estimates



(a) ln(Total debt)



(b) ln(Labor earnings)



(c) ln(Consumption)

This figure presents the dynamic difference-in-differences estimates of the impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The y-axis corresponds to the coefficient estimates of β^1_τ (marked in blue), β^2_τ (marked in red), and $\beta^1_\tau + \beta^2_\tau + \beta^3_\tau$ (marked in green) from estimating the following regression model: $y_{i(f),t} = \sum_{\tau=-3,\,\tau\neq 0}^{3} \left[\beta^1_\tau \cdot (1_{\tau=t} \times Treated_f) + \beta^2_\tau \cdot (1_{\tau=t} \times Treated_i) + \beta^3_\tau \cdot (1_{\tau=t} \times Treated_f \times Treated_i)\right] + \gamma_i + \gamma_t + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in Panel (a), labor earnings in Panel (b), and consumption in Panel (c). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_i$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and γ_t are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. The x-axis corresponds to years. The vertical bars represent confidence intervals at the 95% level. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level.

Tables

Table 1. Summary statistics

	N	Mean	Median	SD	P5	P95
Bank variables						
ln(Total assets)	954	15.09	14.71	1.32	13.24	18.02
Equity/TA	954	0.09	0.09	0.03	0.05	0.15
Loans/TA	954	0.84	0.85	0.07	0.66	0.92
ROA	954	0.01	0.01	0.00	0.00	0.01
Loan losses/TA	954	0.00	0.00	0.00	-0.00	0.01
Foreign wholesale funding/TA	954	0.02	0.00	0.09	0.00	0.08
Firm variables						
ln(Total assets)	244246	8.43	8.28	1.55	6.23	11.32
Employees	244246	19.70	6.00	167.58	0.00	249.00
Debt/TA	244246	0.85	0.78	0.69	0.37	1.42
EBIT/TA	244246	0.04	0.06	0.31	-0.34	0.38
Fixed assets/TA	244246	0.33	0.26	0.27	0.02	0.84
# Lending relationships	244246	1.13	1.00	0.65	0.00	2.00
Individual variables						
Age	2085207	42.87	43.00	8.57	29.00	57.00
Female	2085207	0.30	0.00	0.46	0.00	1.00
Married	2085207	0.51	1.00	0.50	0.00	1.00
# Household members	2085207	3.08	3.00	1.37	1.00	5.00
High education	2085207	0.27	0.00	0.45	0.00	1.00
ln(Total assets)	2085207	12.53	12.97	2.37	9.90	14.64
ln(Debt)	2085207	9.78	11.53	4.40	0.00	13.42
Debt-to-income ratio	2085207	1.81	1.37	1.89	0.00	5.25
ln(Labor earnings)	2085207	12.84	12.92	1.11	11.98	13.70
ln(Consumption)	1181157	12.50	12.47	0.87	11.53	13.78
# Lending relationships	2085207	1.40	1.00	1.09	0.00	3.00

This table provides summary statistics for the key bank, firm, and individual-level variables used in our analysis. Table A1 in the Appendix provides information about the variable definitions.

Table 2. Balance test

	Mean	SD	Mean	SD	Normalized Differences
Panel A	Exposed banks		Non-exposed banks		
ln(Total assets)	16.940	1.007	14.240	0.728	3.065
Equity/TA	0.072	0.019	0.105	0.025	-1.514
ROA	0.009	0.003	0.010	0.003	-0.242
Loan losses/TA	0.000	0.002	0.001	0.001	-0.216
Loans/TA	0.856	0.083	0.876	0.056	-0.280
Foreign funding/TA	0.096	0.184	0.000	0.000	0.740
Observations	27		116		143
Panel B	Affected firms		Non-affected firms		
Age	10.720	12.144	8.621	10.019	0.189
ln(Total assets)	8.382	1.509	7.901	1.409	0.329
Employees	20.528	192.902	13.674	62.076	0.048
Debt/TA	0.876	0.500	0.827	0.440	0.103
EBIT/TA	0.043	0.261	0.070	0.283	-0.097
Fixed assets/TA	0.354	0.265	0.306	0.256	0.184
# Lending relationships	1.292	0.572	0.700	0.702	0.925
Observations	23454		15474		38928
Panel C	Affected individuals		Non-affected individuals		
Age	42.356	8.941	41.658	9.139	-0.007
Female	0.299	0.426	0.337	0.480	-0.128
Married	0.465	0.499	0.529	0.500	-0.084
# Household members	3.121	1.384	3.123	1.379	-0.01
High education	0.276	0.440	0.272	0.445	0.009
ln(Total assets)	12.571	2.476	12.486	2.442	0.035
ln(Debt)	9.390	2.748	8.183	6.080	0.256
Debt-to-income ratio	1.750	1.910	1.478	1.715	0.150
ln(Labor earnings)	12.910	0.678	12.816	1.083	0.104
ln(Consumption)	12.443	0.872	12.347	0.896	0.109
# Lending relationships	1.386	0.995	1.102	0.975	0.288
Observations	153578		162239		315817

This table reports the mean and standardized deviation for exposed versus non-exposed banks in Panel A, affected versus non-affected firms in Panel B, and affected versus non-affected individuals in Panel C, as well as the normalized difference test in 2007 (right before the onset of the global liquidity freeze). The latter is a scale-and-sample-size-free estimator proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) proposed a heuristic threshold of 0.25 in absolute value for significant differences. Table A1 in the Appendix provides information about the variable definitions.

Table 3. Bank lending by exposed and non-exposed banks following the global liquidity freeze

	(1)	(2)	(3)	(4)
Panel A	Firms		Individuals	
	Credit growth	Credit growth	Credit growth	Credit growth
$\mathrm{Post_t} \times \mathrm{Treated_b}$	-0.112***	-0.134**	-0.164*	-0.114**
	(0.044)	(0.067)	(0.096)	(0.053)
Observations	192300	40122	2850273	1078614
Adjusted R-squared	0.27	0.48	0.19	0.48
Panel B	Firms		Individuals	
	Loan rate	Loan rate	Loan rate	Loan rate
$\mathrm{Post_t} \times \mathrm{Treated_b}$	0.967***	0.954***	1.284***	1.013***
	(0.329)	(0.412)	(0.352)	(0.299)
Observations	154579	24170	2346451	648439
Adjusted R-squared	0.54	0.55	0.44	0.48
Bank controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Firm FE	Yes	No	No	No
Individual FE	No	No	Yes	No
Firm × Year FE	No	Yes	No	No
Individual × Year FE	No	No	No	Yes

This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks to firms and individuals, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{b,f,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_{f,t} + \epsilon_{b,f,t}$. In Panel A, the dependent variable $y_{b,f,t}$ is credit growth between bank b and firm f from year $t-1$ to year t in columns (1)–(2), and credit growth between bank b and individual i from year $t-1$ to year t in columns (3)–(4). In Panel B, the dependent variable $y_{b,f,t}$ is the loan rate charged by bank b to firm f in year t in columns (1)–(2), and the loan rate charged by bank b to individual i in year t in columns (3)–(4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and $\gamma_{f,t}$ are bank and firm-year fixed effects, respectively, and $\epsilon_{b,f,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 4. Main results: The impact of firm-side credit constraints, individual-side credit constraints, and their interaction on individual outcomes

	ln(Total debt)		ln(Labor earnings)		ln(Consumption)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathrm{Post_t} \times \mathrm{Treated_f}$	-0.018***	–	-0.020***	–	-0.011***	–
	(0.008)	(–)	(0.005)	(–)	(0.004)	(–)
$\mathrm{Post_t} \times \mathrm{Treated_{i(f)}}$	-0.075***	-0.085***	0.000	-0.002	-0.023***	-0.014**
	(0.014)	(0.020)	(0.006)	(0.007)	(0.006)	(0.006)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.011*	-0.012*	-0.034***	-0.032***	-0.025***	-0.033***
	(0.007)	(0.008)	(0.006)	(0.007)	(0.005)	(0.006)
Observations	1889531	1810081	1889531	1810081	1063897	992624
Adjusted R-squared	0.73	0.73	0.36	0.37	0.18	0.19
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Firm × Year FE	No	Yes	No	Yes	No	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_1, β_2, and β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in columns (1)–(2), the natural logarithm of labor earnings in columns (3)–(4), and the natural logarithm of consumption in columns (5)–(6). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table 5. Main results: Displaced workers

	(1) Job search duration (days)	(2) ln(Labor earnings)	(3) Occupational switch	(4) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_{i(f)}}$	-28.005***	-0.091***	-0.036*	-0.141***
	(11.653)	(0.030)	(0.022)	(0.063)
Sample	Displaced workers	Displaced workers	Displaced workers	Displaced workers
Observations	64987	51234	51234	51234
Adjusted R-squared	0.62	0.65	0.45	0.51
Person FE	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on displaced individuals who had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_1 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the number of post-displacement job search in column (1), the natural logarithm of labor earnings in column (2), a binary variable equal to one for workers who switch occupations in column (3), and the natural logarithm of consumption in column (4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table 6. Main results: Non-displaced workers

	(1) Displaced	(2) ln(Labor earnings)	(3) Voluntary switch	(4) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	0.001	-0.019	-0.005**	-0.040***
	(0.002)	(0.014)	(0.002)	(0.015)
Sample	All workers	Non-displaced workers	Non-displaced workers	Non-displaced workers
Observations	1889531	1655871	1655871	885321
Adjusted R-squared	0.21	0.69	0.20	0.48
Person FE	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on non-displaced individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is a binary variable equal to one for displaced workers in column (1), the natural logarithm of labor earnings in column (2), a binary variable equal to one for voluntary job switchers in column (3), and the natural logarithm of consumption in column (4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table 7. Main results: Heterogeneity

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)	(4) ln(Total debt)	(5) ln(Labor earnings)	(6) ln(Consumption)
Panel A	Low liquidity buffer			High liquidity buffer		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.015	-0.046***	-0.050***	-0.013	-0.008	-0.015
	(0.010)	(0.008)	(0.010)	(0.010)	(0.010)	(0.009)
Observations	640582	640582	409206	1069418	1069418	523418
Adjusted R-squared	0.75	0.38	0.19	0.77	0.37	0.18
Panel B	Single bank relationship			Multiple bank relationships		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.011*	-0.043***	-0.040***	-0.002	-0.011	-0.012
	(0.007)	(0.008)	(0.008)	(0.012)	(0.007)	(0.009)
Observations	621533	621533	368451	1084514	1084514	564215
Adjusted R-squared	0.74	0.38	0.19	0.74	0.38	0.18
Panel C	High debt-to-income ratio			Low debt-to-income ratio		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.014*	-0.039***	-0.035***	0.001	0.013	-0.005
	(0.008)	(0.009)	(0.010)	(0.008)	(0.012)	(0.006)
Observations	1341579	1341579	734521	358421	358421	200251
Adjusted R-squared	0.71	0.35	0.18	0.74	0.37	0.18
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze across different sub-samples. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in columns (1) and (4), the natural logarithm of labor earnings in columns (2) and (5), and the natural logarithm of consumption in column (3) and (6). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. Panel A splits the sample based on individuals' liquid assets. Panel B splits the sample based on individuals' number of bank relationships. Panel C splits the sample based on individuals' debt-to-income ratio. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

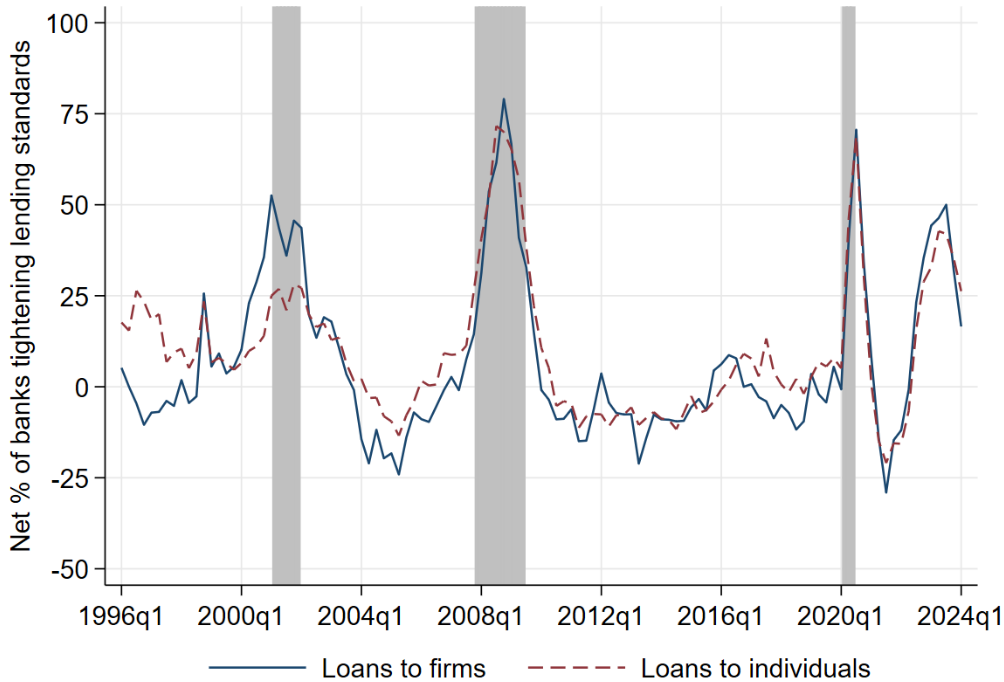
Table 8. Aggregate effects

	(1) ln(Local consumption)	(2) ln(Local output)	(3) ln(Local employment)
$\mathrm{Post_t} \times$ Firm $\mathrm{Exposure_m}$	-0.024*	-0.024*	-0.030***
	(0.014)	(0.012)	(0.011)
$\mathrm{Post_t} \times$ Individual $\mathrm{Exposure_m}$	-0.004	0.010	-0.008
	(0.016)	(0.020)	(0.016)
$\mathrm{Post_t} \times$ Joint $\mathrm{Exposure_m}$	-0.049**	-0.043**	-0.045**
	(0.020)	(0.020)	(0.021)
Observations	755	755	755
Adjusted R-squared	0.87	0.86	0.79
Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

This table presents the estimated effects of credit shocks on municipality-level economic outcomes, as a function of the extent to which firms and individuals in the municipality are exposed to the credit crunch. The table reports the coefficient estimates of δ_1, δ_2, and δ_3 from estimating the following regression model: $y_{m,t} = \delta_1 \cdot (Post_t \times Firm\ Exposure_m) + \delta_2 \cdot (Post_t \times Individual\ Exposure_m) + \delta_3 \cdot (Post_t \times Joint\ Exposure_m) + \gamma_m + \gamma_t + \epsilon_{m,t}$ where the dependent variable $y_{m,t}$ is the natural logarithm of local consumption in column (1), the natural logarithm of local firm output in column (2), and the natural logarithm of local employment in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze. $Firm\ Exposure_m$ and $Individual\ Exposure_m$ capture the share of individuals in municipality m exposed to the credit market disruption through firm-side and individual-side credit shocks, respectively. $Joint\ Exposure_m$ measures the share of individuals in municipality m who were simultaneously exposed to both personal credit shocks and those experienced by their employer. γ_m and γ_t are municipality and year fixed effects, respectively, and $\epsilon_{m,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the municipality level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

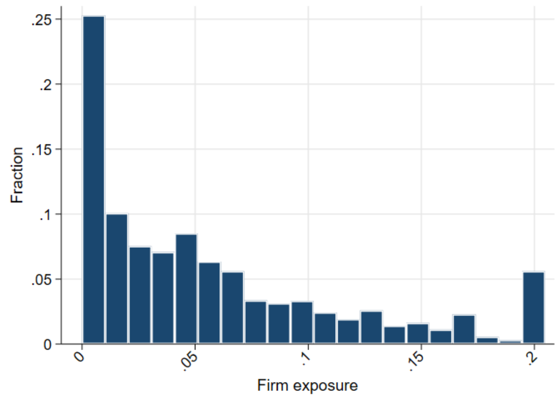
Appendix

Figure A1. The percentage of U.S. banks tightening lending standard to firms and individuals

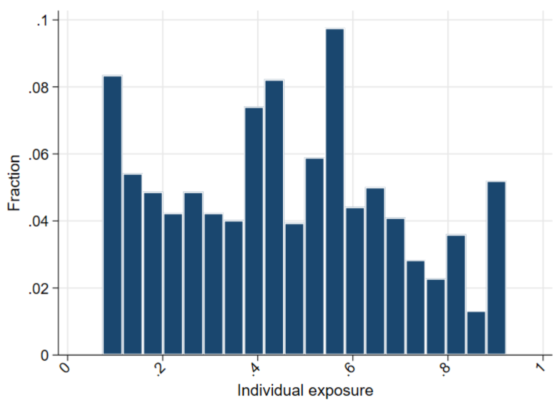


This figure illustrates the percentage of US banks tightening lending standard to firms and individuals according to the Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS) conducted by the Board of Governors of the Federal Reserve System. The firm lending index is an equal-weighted average of lending standards for loans to small businesses and to medium-sized and large businesses. The consumer lending index is computed as an equal-weighted average of lending standards for mortgage loans, credit cards, and other consumer loans. The periods shaded in gray indicate recession periods.

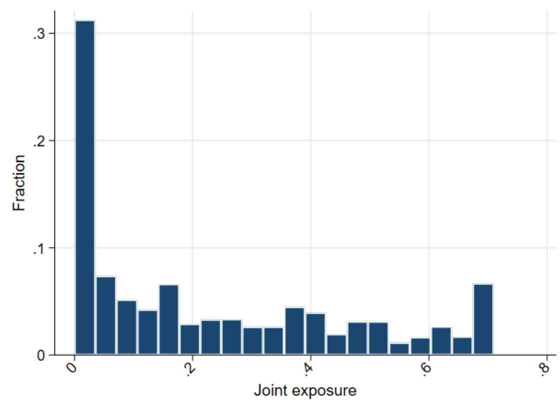
Figure A2. Distribution of municipality-level exposure measures



(a) Firm exposure



(b) Individual exposure



(c) Joint exposure

This figure shows the distribution of the municipality-level credit shock exposure measures defined in Section 5.5. Panel (a) plots the distribution of firms whose relationship bank was exposed to the global liquidity freeze. Panel (b) plots the distribution of individuals whose relationship bank was exposed to the global liquidity freeze. Panel (c) plots the distribution of individuals whose personal relationship bank and whose employer's relationship bank were both exposed to the global liquidity freeze.

Table A1. Variable definitions

Variable	Description
Bank variables	
ln(Size)	The bank's total assets.
Equity/TA	The ratio of equity to total assets.
Loans/TA	The ratio of loans to total assets.
Loan losses/TA	The ratio of loan losses to total assets.
Return on assets (ROA)	The ratio of net profits to total assets.
Foreign wholesale funding/TA	The ratio of foreign wholesale funding in total assets.
Firm variables	
Size	The firm's total assets.
Age	The number of years since the firm's foundation.
ln(Employees)	The natural logarithm of the number of individuals employed by the firm.
Average wage	The ratio of total labor expenses to the number of total employees.
Debt/TA	The ratio of debt to total assets.
Fixed assets/TA	The ratio of physical assets to total assets.
Return on assets (ROA)	The ratio of net profits to total assets.
Municipality	The municipality of the firm.
Industry	The sector in which the firm operates.
Individual variables	
Age	The individual's age.
Female	A dummy variable equal to one for females, and zero otherwise.
Married	A dummy variable equal to one for married individuals, zero otherwise.
Homeowner	A dummy variable equal to one for individuals who own a house, zero otherwise.
Number of household members	The number of household members.
High education	A dummy variable equal to one if the individual has a university or college degree, zero otherwise.
ln(Consumption)	The natural logarithm of total (imputed) consumption.
ln(Debt)	The natural logarithm of total debt.
ln(Labor earnings)	The natural logarithm of income from employment.
ln(Liquid assets)	The natural logarithm of the sum of checking and savings accounts, and holdings of stocks, bonds, and funds.
ln(Income)	The natural logarithm of total income.
Debt-to-income	The ratio of debt to income.
Residential municipality	The municipality of the individual's residence.
Displaced	A dummy equal to one if the individual received non-zero weeks of unemployment benefits in either the year of separation or the following year.

This table provides the variable definitions of the main variables used in our analysis.

Table A2. Main results: Robustness with pre-treatment controls

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.013*	-0.041***	-0.038***
	(0.007)	(0.017)	(0.015)
Observations	1810081	1810081	992624
Adjusted R-squared	0.75	0.38	0.23
Controls	Yes	Yes	Yes
Person FE	Yes	Yes	Yes
Firm $\times$ Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \delta \cdot X_{i,pre} + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. $C_{i,pre}$ is a vector or pre-event individual characteristics interacted with year dummies. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table A3. Aggregate effects: Assuming credit shocks affect only firms or only individuals

	(1) ln(Local consumption)	(2) ln(Local output)	(3) ln(Local employment)
Panel A	Assume credit shocks affect only firms		
$\mathrm{Post_t} \times$ Firm $\mathrm{Exposure_m}$	-0.046***	-0.034**	-0.041***
	(0.013)	(0.014)	(0.012)
Observations	755	755	755
Adjusted R-squared	0.85	0.80	0.78
Panel B	Assume credit shocks affect only individuals		
$\mathrm{Post_t} \times$ Individual $\mathrm{Exposure_m}$	-0.036***	-0.022*	-0.031**
	(0.012)	(0.013)	(0.013)
Observations	755	755	755
Adjusted R-squared	0.81	0.82	0.77
Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

This table presents the estimated effects of credit shocks on municipality-level economic outcomes, as a function of the extent to which firms or individuals in the municipality are exposed to the credit crunch. Panel A reports the coefficient estimates of δ_1 from estimating the following regression model: $y_{m,t} = \delta_1 \cdot (Post_t \times Firm\ Exposure_m) + \gamma_m + \gamma_t + \epsilon_{m,t}$ and Panel B reports the coefficient estimates of δ_2 from estimating the following regression model: $y_{m,t} = \delta_2 \cdot (Post_t \times Individual\ Exposure_m) + \gamma_m + \gamma_t + \epsilon_{m,t}$. In both regressions, the dependent variable $y_{m,t}$ is the natural logarithm of local consumption in column (1), the natural logarithm of local firm output in column (2), and the natural logarithm of local employment in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze. $Firm\ Exposure_m$ and $Individual\ Exposure_m$ capture the share of individuals in municipality m exposed to the credit market disruption through firm-side and individual-side credit shocks, respectively. Panels A and B assume that credit shocks affect economic activity only through firm-side credit constraints or only through individual-side credit constraints, respectively. γ_m and γ_t are municipality and year fixed effects, respectively, and $\epsilon_{m,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the municipality level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table A4. Aggregate effects: Excluding directly affected individuals and firms

	(1) ln(Local consumption)	(2) ln(Local output)	(3) ln(Local employment)
$\mathrm{Post_t} \times$ Firm $\mathrm{Exposure_m}$	-0.014	-0.015**	-0.025***
	(0.009)	(0.008)	(0.011)
$\mathrm{Post_t} \times$ Individual $\mathrm{Exposure_m}$	-0.009	-0.011	0.001
	(0.012)	(0.018)	(0.018)
$\mathrm{Post_t} \times$ Joint $\mathrm{Exposure_m}$	-0.038*	-0.031***	-0.040**
	(0.020)	(0.012)	(0.020)
Observations	755	755	755
Adjusted R-squared	0.86	0.86	0.78
Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

This table presents the estimated effects of credit shocks on municipality-level economic outcomes, as a function of the extent to which firms and individuals in the municipality are exposed to the credit crunch. The table reports the coefficient estimates of δ_1, δ_2, and δ_3 from estimating the following regression model: $y_{m,t} = \delta_1 \cdot (Post_t \times Firm\ Exposure_m) + \delta_2 \cdot (Post_t \times Individual\ Exposure_m) + \delta_3 \cdot (Post_t \times Joint\ Exposure_m) + \gamma_m + \gamma_t + \epsilon_{m,t}$ where the dependent variable $y_{m,t}$ is the natural logarithm of local consumption in column (1), the natural logarithm of local firm output in column (2), and the natural logarithm of local employment in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze. $Firm\ Exposure_m$ and $Individual\ Exposure_m$ capture the share of individuals in municipality m exposed to the credit market disruption through firm-side and individual-side credit shocks, respectively. $Joint\ Exposure_m$ measures the share of individuals in municipality m who were simultaneously exposed to both personal credit shocks and those experienced by their employer. γ_m and γ_t are municipality and year fixed effects, respectively, and $\epsilon_{m,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the municipality level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table A5. Aggregate effects: Heterogeneity

	(1) ln(Local consumption)	(2) ln(Local output)	(3) ln(Local employment)
Panel A		High share of non-tradables	
$\text{Post}_t \times \text{Firm Exposure}_m$	-0.034	-0.035	-0.042*
	(0.022)	(0.027)	(0.024)
$\text{Post}_t \times \text{Individual Exposure}_m$	0.000	0.021	0.011
	(0.022)	(0.028)	(0.026)
$\text{Post}_t \times \text{Joint Exposure}_m$	-0.059**	-0.054*	-0.063**
	(0.030)	(0.034)	(0.031)
Observations	420	420	420
Adjusted R-squared	0.85	0.82	0.78
Panel B		Low share of non-tradables	
$\text{Post}_t \times \text{Firm Exposure}_m$	-0.031	-0.017	-0.021
	(0.029)	(0.014)	(0.017)
$\text{Post}_t \times \text{Individual Exposure}_m$	-0.033	0.004	0.007
	(0.046)	(0.017)	(0.025)
$\text{Post}_t \times \text{Joint Exposure}_m$	-0.042*	-0.026	-0.020
	(0.025)	(0.018)	(0.018)
Observations	335	335	335
Adjusted R-squared	0.84	0.82	0.78
Controls	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

This table presents the estimated effects of credit shocks on municipality-level economic outcomes, as a function of the extent to which firms and individuals in the municipality are exposed to the credit crunch. The table reports the coefficient estimates of δ_1, δ_2, and δ_3 from estimating the following regression model: $y_{m,t} = \delta_1 \cdot (Post_t \times Firm\ Exposure_m) + \delta_2 \cdot (Post_t \times Individual\ Exposure_m) + \delta_3 \cdot (Post_t \times Joint\ Exposure_m) + \gamma_m + \gamma_t + \epsilon_{m,t}$ where the dependent variable $y_{m,t}$ is the natural logarithm of local consumption in column (1), the natural logarithm of local firm output in column (2), and the natural logarithm of local employment in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze. $Firm\ Exposure_m$ and $Individual\ Exposure_m$ capture the share of individuals in municipality m exposed to the credit market disruption through firm-side and individual-side credit shocks, respectively. $Joint\ Exposure_m$ measures the share of individuals in municipality m who were simultaneously exposed to both personal credit shocks and those experienced by their employer. γ_m and γ_t are municipality and year fixed effects, respectively, and $\epsilon_{m,t}$ is the error term. Panels A and B split the sample based on municipalities with high and low shares of firms active in non-tradable industries, respectively (following the industry classification of Mian and Sufi 2014, as explained in Section 5.5). At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the municipality level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

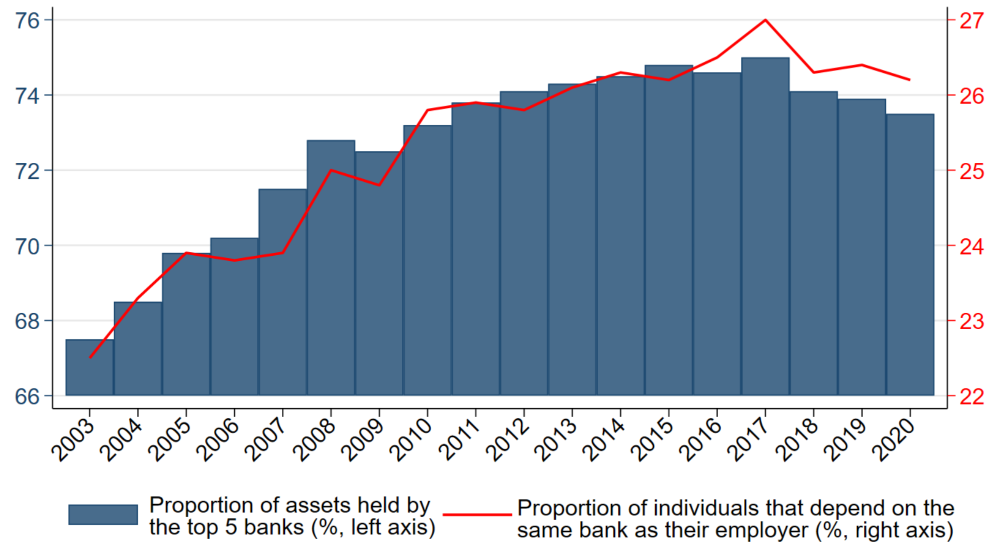
INTERNET APPENDIX

Banks, Firms, and Households: Credit Shock Amplification and Real Effects

Cédric Huylebroek and Jin Cao

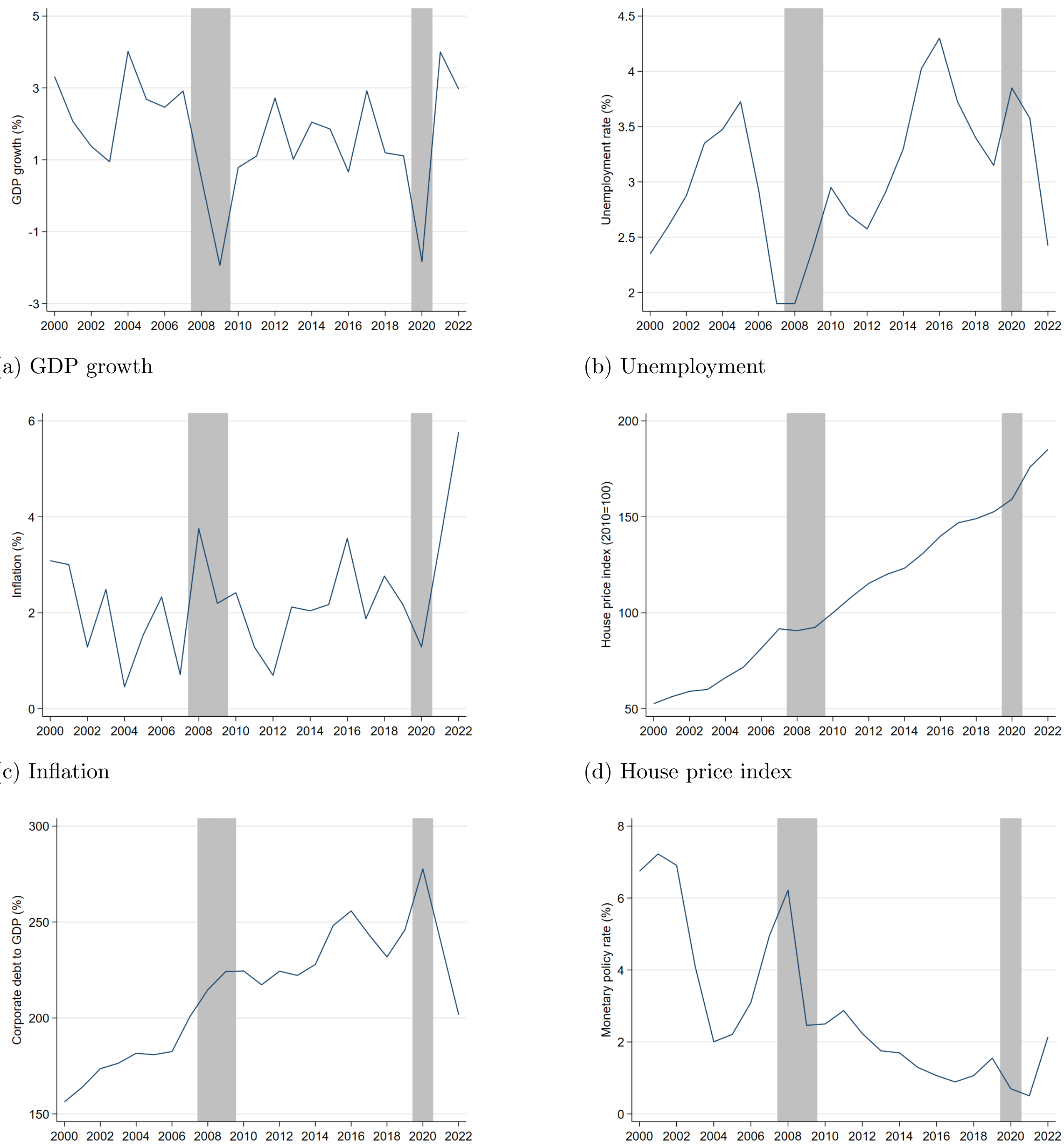
Internet Appendix A

Figure I.A1. The proportion of individuals that maintain a relationship with the same bank as their employer



This figure illustrates the proportion of Norwegian individuals who maintain a relationship with the same bank as their employer (in red) on the right axis and the share assets held by the top five Norwegian banks (in blue) on the left axis over the period 2003–2020. The definition of relationship bank is explained in Section 3.3.

Figure I.A2. Macroeconomic conditions



(a) GDP growth

(b) Unemployment

(c) Inflation

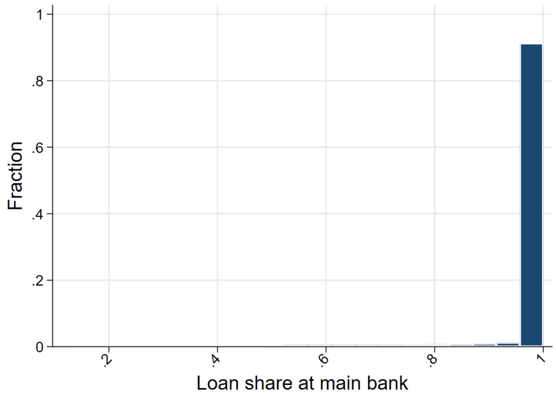
(d) House price index

(e) Corporate debt/GDP

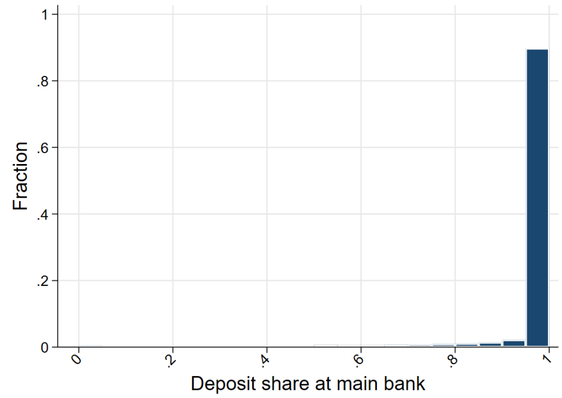
(f) Policy rate

This figure shows the macroeconomic conditions in Norway between 2000 and 2022. Panel (a) plots GDP growth, Panel (b) plots the unemployment rate, Panel (c) plots inflation, Panel (d) plots the house price index, Panel (e) plots corporate debt to GDP, and Panel (f) plots the policy rate. The periods shaded in gray indicate recession periods.

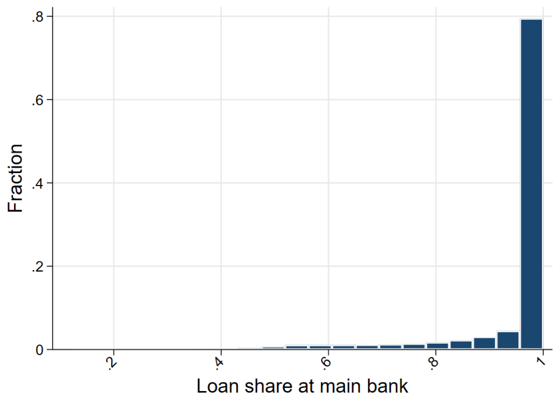
Figure I.A3. Loan and deposit shares held at relationship bank



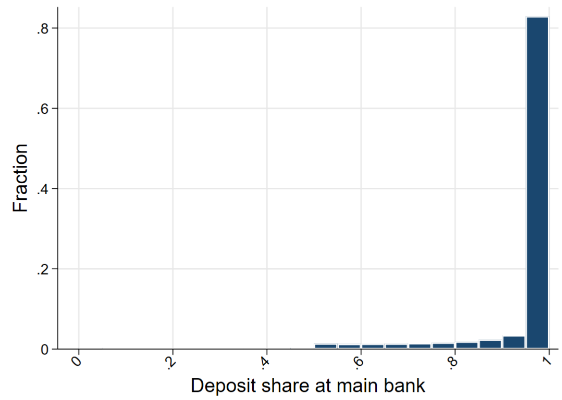
(a) Firms' loan share at their relationship bank



(b) Firms' deposit share at their relationship bank



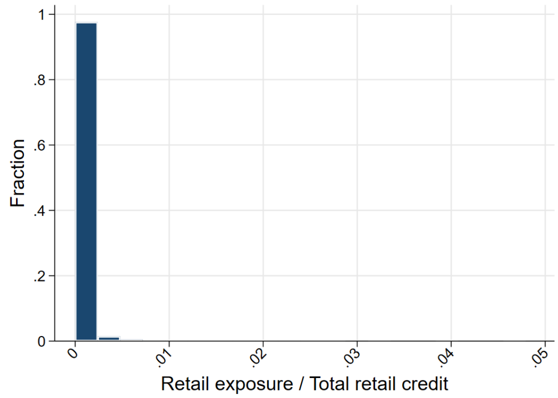
(c) Individuals' loan share at their relationship bank



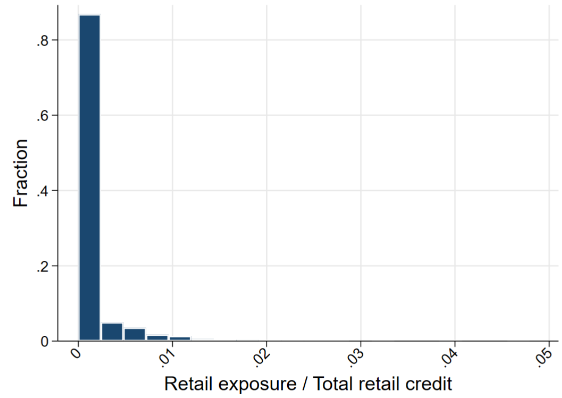
(d) Individuals' deposit share at their relationship bank

This figure shows the loan or deposit share of firms and individuals at their relationship bank. The definition of relationship bank is outlined in Section 3.3. Panels (a) and (b) respectively plot the share of loans held at the relationship bank among firms and individuals with an existing lending relationship. Panels (c) and (d) respectively plot the share of deposits held at the relationship bank among firms and individuals without an existing lending relationship.

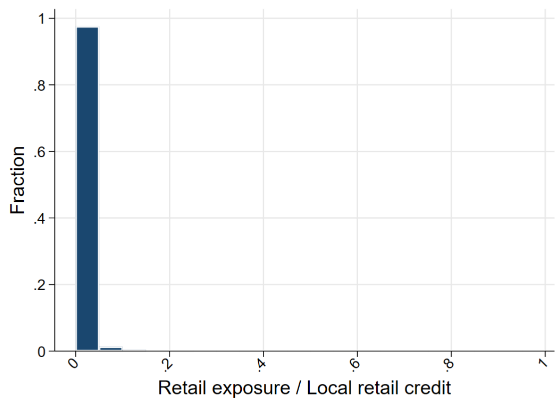
Figure I.A4. Banks' retail credit exposure to the employees of a corporate borrower



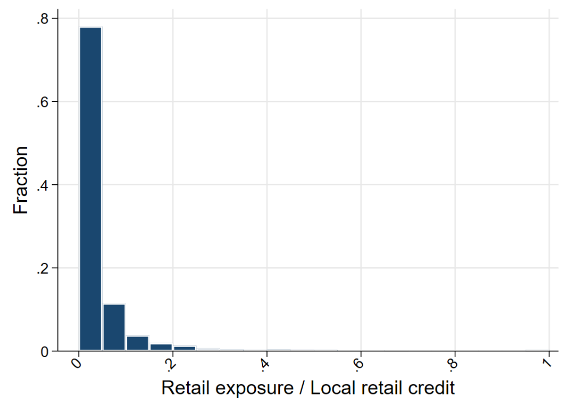
(a) Retail credit exposure / Total retail credit



(b) Retail credit exposure$_{\text{P90}}$ / Total retail credit



(c) Retail credit exposure / Local retail credit



(d) Retail credit exposure$_{\text{P90}}$ / Local retail credit

This figure shows the distribution of retail credit exposure by a bank to the employees of a corporate borrower. Panels (a) plots the distribution of the share of retail credit by a given bank to the employees of a corporate borrower over the bank's total retail credit. Panel (b) plots the distribution of the top decile of the share of retail credit by a given bank to the employees of a corporate borrower over the bank's total retail credit. Panels (c) plots the distribution of the share of retail credit by a given bank to the employees of a corporate borrower over the bank's local retail credit. Panel (d) plots the distribution of the top decile of the share of retail credit by a given bank to the employees of a corporate borrower over the bank's local retail credit.

Table I.A1. Balance test: Affected and non-affected individuals employed by affected and non-affected firms

	Affected individuals at affected firms			Affected individuals at non-affected firms			Non-affected individuals at affected firms			Non-affected individuals at non-affected firms	
	Mean	SD	Normalized Differences	Mean	SD	Normalized Differences	Mean	SD	Normalized Differences	Mean	SD
Age	42.062	8.244	0.078	42.598	8.302	0.142	41.749	8.461	0.040	41.413	8.405
Married	0.505	0.500	-0.022	0.543	0.498	0.054	0.544	0.498	0.056	0.516	0.500
Female	0.243	0.429	-0.189	0.344	0.475	0.034	0.347	0.476	0.040	0.328	0.469
# Household members	3.106	1.371	0.020	3.133	1.364	0.040	3.176	1.353	0.071	3.079	1.367
High education	0.261	0.439	-0.083	0.287	0.452	-0.024	0.242	0.428	-0.126	0.298	0.457
ln(Total assets)	12.570	2.273	0.051	12.572	1.978	0.055	12.519	2.233	0.028	12.458	2.160
ln(Debt)	10.706	2.853	0.588	8.266	4.956	-0.016	7.988	5.080	-0.072	8.346	4.910
Debt-to-income ratio	2.229	1.952	0.412	1.356	1.631	-0.062	1.497	1.877	0.019	1.462	1.764
ln(Labor earnings)	12.908	0.661	0.041	12.911	0.679	0.045	12.739	0.770	-0.200	12.881	0.641
ln(Consumption)	12.481	0.853	0.099	12.411	0.863	0.017	12.289	0.992	-0.115	12.396	0.858
# Lending relationships	1.602	0.927	0.487	1.197	1.022	0.056	1.055	0.906	-0.092	1.141	0.964
Observations	68456			85122			74003			88236	

This table reports the mean and standardized deviation for affected individuals employed by affected firms, affected individuals employed by non-affected firms, non-affected individuals employed by affected firms, and non-affected individuals employed by non-affected firms as well as the normalized difference test in 2007 (right before the onset of the global liquidity freeze). The latter is a scale-and-sample-size-free estimator proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) proposed a heuristic threshold of 0.25 in absolute value for significant differences. The benchmark group for the normalized differences test is always the group of non-affected individuals employed by non-affected firms. Table A1 in the Appendix provides information about the variable definitions.

Table I.A2. Summary statistics: Loan data

	N	Mean	Median	SD	P5	P95
Bank-firm variables						
Credit growth	207208	-0.023	0.014	0.958	-2.000	2.000
Loan rate	198696	8.124	7.389	3.868	2.7663	15.494
Bank-individual variables						
Credit growth	2858040	-0.017	0.034	0.898	-2.000	2.000
Loan rate	2723572	8.788	5.663	7.142	1.242	25.218

This table provides summary statistics for the loan data used in our analysis on bank credit supply following the global liquidity freeze discussed in Section 4.1. Credit growth is computed as $\frac{Credit_{f,b,t} - Credit_{f,b,t-1}}{0.5 \times (Credit_{f,b,t} + Credit_{f,b,t-1})}$ and loan rates are computed as $\frac{Interest\ paid_{f,b,t}}{0.5 \cdot (Credit_{f,b,t} + Credit_{f,b,t-1})}$ where f, b, and t denote firm, bank, and year, respectively. The same approach is applied to construct bank-individual credit growth and loan rates.

Table I.A3. Bank lending by exposed and non-exposed banks following the global liquidity freeze: Robustness with treatment intensity measure

	(1)	(2)	(3)	(4)
Panel A	Firms		Individuals	
	Credit growth	Credit growth	Credit growth	Credit growth
$\mathrm{Post_t} \times$ Foreign wholesale funding/$\mathrm{TA_b}$	-0.301*	-0.578***	-0.361***	-0.497***
	(0.174)	(0.098)	(0.153)	(0.087)
Observations	192300	40122	2850273	1078614
Adjusted R-squared	0.27	0.48	0.19	0.48
Panel B	Firms		Individuals	
	Loan rate	Loan rate	Loan rate	Loan rate
$\mathrm{Post_t} \times$ Foreign wholesale funding/$\mathrm{TA_b}$	5.125***	7.102***	6.787***	6.980***
	(2.364)	(1.988)	(3.005)	(2.521)
Observations	154579	24170	2346451	648439
Adjusted R-squared	0.52	0.55	0.43	0.47
Bank controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Firm FE	Yes	No	No	No
Individual FE	No	No	Yes	No
Firm × Year FE	No	Yes	No	No
Individual × Year FE	No	No	No	Yes

This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks to firms and individuals, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{b,f,t} = \beta \cdot (Post_t \times Foreign\ wholesale\ funding/TA_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_{f,t} + \epsilon_{b,f,t}$. In Panel A, the dependent variable $y_{b,f,t}$ is credit growth between bank b and firm f from year $t-1$ to year t in columns (1)–(2), and credit growth between bank b and individual i from year $t-1$ to year t in columns (3)–(4). In Panel B, the dependent variable $y_{b,f,t}$ is the loan rate charged by bank b to firm f in year t in columns (1)–(2), and the loan rate charged by bank b to individual i in year t in columns (3)–(4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Foreign\ wholesale\ funding/TA_b$ corresponds to banks' ratio of foreign wholesale funding to total assets right before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and $\gamma_{f,t}$ are bank and firm-year fixed effects, respectively, and $\epsilon_{b,f,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table I.A4. Bank lending by exposed and non-exposed banks following the global liquidity freeze: Firm heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
	Credit growth	Credit growth	Credit growth	Credit growth	Credit growth	Credit growth
	Firms					
	Young	Old	Low fixed assets	High fixed assets	Few alternative lenders	Many alternative lenders
$\mathrm{Post_t} \times \mathrm{Treated_b}$	-0.188**	-0.118	-0.183**	-0.121	-0.149**	-0.099*
	(0.097)	(0.078)	(0.098)	(0.087)	(0.077)	(0.060)
Observations	12538	27584	18483	21639	16791	23331
Adjusted R-squared	0.48	0.49	0.49	0.47	0.48	0.48
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks to firms, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{b,f,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_{f,t} + \epsilon_{b,f,t}$ where the dependent variable $y_{b,f,t}$ is credit growth between bank b and firm f from year $t-1$ to year t. $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and $\gamma_{f,t}$ are bank and firm-year fixed effects, respectively, and $\epsilon_{b,f,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table I.A5. Bank lending by exposed and non-exposed banks following the global liquidity freeze: Individual heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
	Credit growth	Credit growth	Credit growth	Credit growth	Credit growth	Credit growth
	Individuals					
	Young	Old	Low housing wealth	High housing wealth	Few alternative lenders	Many alternative lenders
$\mathrm{Post_t} \times \mathrm{Treated_b}$	-0.154***	-0.090*	-0.166**	-0.084**	-0.130***	-0.088
	(0.063)	(0.053)	(0.076)	(0.043)	(0.060)	(0.061)
Observations	490661	588953	406528	671086	512012	566602
Adjusted R-squared	0.48	0.47	0.46	0.45	0.46	0.45
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks to individuals, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{b,i,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_{i,t} + \epsilon_{b,i,t}$ where the dependent variable $y_{b,f,t}$ is credit growth between bank b and individual i from year $t-1$ to year t. $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and $\gamma_{i,t}$ are bank and individual-year fixed effects, respectively, and $\epsilon_{b,i,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table I.A6. Main results: Heterogeneity based on household level measures

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)	(4) ln(Total debt)	(5) ln(Labor earnings)	(6) ln(Consumption)
Panel A	Low liquidity buffer			High liquidity buffer		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.015	-0.046***	-0.050***	-0.013	-0.008	-0.015
	(0.010)	(0.008)	(0.010)	(0.010)	(0.010)	(0.009)
Observations	640582	640582	409206	1069418	1069418	523418
Adjusted R-squared	0.75	0.38	0.19	0.77	0.37	0.18
Panel B	Single bank relationship			Multiple bank relationships		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.011*	-0.043***	-0.040***	-0.002	-0.011	-0.012
	(0.007)	(0.008)	(0.008)	(0.012)	(0.007)	(0.009)
Observations	621533	621533	368451	1084514	1084514	564215
Adjusted R-squared	0.74	0.38	0.19	0.74	0.38	0.18
Panel C	High debt-to-income ratio			Low debt-to-income ratio		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.014*	-0.039***	-0.035***	0.001	0.013	-0.005
	(0.008)	(0.009)	(0.010)	(0.008)	(0.012)	(0.006)
Observations	1341579	1341579	734521	358421	358421	200251
Adjusted R-squared	0.71	0.35	0.18	0.74	0.37	0.18
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze across different sub-samples. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in columns (1) and (4), the natural logarithm of labor earnings in columns (2) and (5), and the natural logarithm of consumption in column (3) and (6). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i,t}$ is the error term. Panel A splits the sample based on households' liquid assets. Panel B splits the sample based on households' number of bank relationships. Panel C splits the sample based on households' debt-to-income ratio. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A7. Main results: Additional sources of heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Total debt)	ln(Labor earnings)	ln(Consumption)	ln(Total debt)	ln(Labor earnings)	ln(Consumption)
Panel A	White-collar workers			Blue-collar workers		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.012	-0.047***	-0.042***	-0.008	-0.021	-0.016
	(0.010)	(0.015)	(0.013)	(0.007)	(0.016)	(0.011)
Observations	78653	78653	409542	902566	902566	481252
Adjusted R-squared	0.75	0.37	0.18	0.74	0.37	0.16
Panel B	Tight labor markets			Loose labor markets		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.011*	-0.043***	-0.040***	-0.010	-0.029**	-0.027***
	(0.007)	(0.008)	(0.008)	(0.098)	(0.017)	(0.012)
Observations	984512	984512	512555	720535	720535	387541
Adjusted R-squared	0.74	0.38	0.19	0.74	0.38	0.18
Panel C	Small firms			Large firms		
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.011**	-0.036***	-0.037***	-0.005	-0.027*	-0.027***
	(0.006)	(0.010)	(0.010)	(0.008)	(0.015)	(0.012)
Observations	543188	543188	315211	1161859	1161859	601232
Adjusted R-squared	0.70	0.32	0.18	0.74	0.37	0.18
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze across different sub-samples. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in columns (1) and (4), the natural logarithm of labor earnings in columns (2) and (5), and the natural logarithm of consumption in column (3) and (6). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i,t}$ is the error term. Panel A splits the sample based on blue- and white-collar workers. Panel B splits the sample based on tight versus loose labor markets. Panel C splits the sample based on small versus large firms. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A8. Main results: Robustness with matched sample

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.018	-0.039***	-0.041***
	(0.020)	(0.018)	(0.019)
Observations	901120	901120	488764
Adjusted R-squared	0.72	0.33	0.16
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze using a matched sample (where individuals are matched based on ex-ante wealth, debt-to-income ratio, age, and location). The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A9. Main results: Falsification test

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	0.018	-0.012	0.005
	(0.021)	(0.018)	(0.015)
Observations	1757442	1757442	906420
Adjusted R-squared	0.70	0.32	0.14
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze using a placebo sample (where individuals, firms, and their banks are randomly linked). The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A10. Main results: Robustness with firm-residential municipality-year fixed effects

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.013	-0.034***	-0.033***
	(0.011)	(0.008)	(0.007)
Observations	1510152	1510152	805650
Adjusted R-squared	0.73	0.36	0.16
Person FE	Yes	Yes	Yes
Firm × Municipality of residence × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,m,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,m,t}$ are individual and firm-residential municipality-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A11. Main results: Renters subsample

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.010	-0.061***	-0.042***
	(0.007)	(0.024)	(0.013)
Sample	Renters	Renters	Renters
Observations	435785	435785	208017
Adjusted R-squared	0.72	0.33	0.20
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze for the subsample of renters. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A12. Main results: Other labor market outcomes

	(1)	(2)	(3)	(4)
	Geographical mobility	Hours worked	Wage volatility	Spousal labor earnings
Panel A	Displaced workers			
$\mathrm{Post_t} \times \mathrm{Treated_{i(f)}}$	-0.007*	0.314	0.002	-0.052
	(0.004)	(0.401)	(0.011)	(0.210)
Observations	51234	51234	51234	12532
Adjusted R-squared	0.53	0.72	0.64	0.78
Panel B	Non-displaced workers			
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.003	-0.014	0.001	0.044
	(0.002)	(0.054)	(0.002)	(0.079)
Observations	1655871	1655871	1655871	564,983
Adjusted R-squared	0.32	0.44	0.55	0.73
Person FE	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. Panel A reports the coefficient estimates of β_1 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot \cdot (Post_t \times Treated_{i(f)}) + \delta \cdot X_{i,pre} + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ on the sub-sample of displaced individuals, and Panel B reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \delta \cdot X_{i,pre} + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ on the sub-sample of non-displaced individuals, where the dependent variable $y_{i(f),t}$ is a binary variable equal to one for individuals who move to a new municipality in column (1), the number of hours worked in column (2), the volatility of wages in column (3), and the natural logarithm of spousal labor earnings in column (4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A13. Main results: Robustness with firm-occupation-year fixed effects

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.017	-0.037***	-0.036***
	(0.026)	(0.010)	(0.009)
Observations	970102	970102	523604
Adjusted R-squared	0.72	0.34	0.16
Person FE	Yes	Yes	Yes
Firm × Occupation × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze excluding firms involved in international trade. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,o,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,o,t}$ are individual and firm-occupation-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A14. Main results: Excluding firms involved in international trade

	(1)	(2)	(3)
	ln(Total debt)	ln(Labor earnings)	ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.003	-0.028***	-0.032***
	(0.019)	(0.008)	(0.007)
Observations	1474522	1474522	802846
Adjusted R-squared	0.72	0.37	0.19
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze excluding firms involved in international trade. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A15. Main results: Excluding oil-sector intensive municipalities

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.011	-0.029***	-0.031***
	(0.008)	(0.008)	(0.009)
Observations	1511601	1511601	642210
Adjusted R-squared	0.70	0.35	0.19
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze excluding oil-sector intensive municipalities. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A16. Main results: Excluding individuals with stock holdings

	(1)	(2)	(3)
	ln(Total debt)	ln(Labor earnings)	ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.012*	-0.032***	-0.042***
	(0.007)	(0.009)	(0.007)
Observations	1168993	1168993	665039
Adjusted R-squared	0.65	0.34	0.16
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze excluding firms involved in international trade. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A17. Main results: Excluding single-branch municipalities

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.010	-0.028***	-0.031***
	(0.008)	(0.007)	(0.007)
Observations	1461355	1461355	806668
Adjusted R-squared	0.73	0.35	0.20
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze excluding firms involved in international trade. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A18. Main results: Robustness with treatment intensity measure

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Exposure_f} \times \mathrm{Exposure_{i(f)}}$	-0.011*	-0.045***	-0.049***
	(0.007)	(0.014)	(0.013)
Observations	1810081	1810081	992624
Adjusted R-squared	0.73	0.36	0.20
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Exposure_f) + \beta_2 \cdot (Post_t \times Exposure_{i(f)}) + \beta_3 \cdot (Post_t \times Exposure_f \times Exposure_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Exposure_f$ is the credit share of exposed banks in firm f's total credit before the onset of the global liquidity freeze, and $Exposure_i$ is the credit share of exposed banks in individuals i's total credit before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A19. Main results: Robustness excluding ex-ante non-borrowing individuals

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.010*	-0.041***	-0.035***
	(0.019)	(0.008)	(0.006)
Observations	1546398	1546398	841024
Adjusted R-squared	0.64	0.35	0.18
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze for the subsample of individuals who actively borrowed before. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A20. Main results: Poisson model

	(1) Total debt	(2) Labor earnings	(3) Consumption
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_{i(f)}}$	-0.009	-0.036***	-0.036***
	(0.007)	(0.013)	(0.012)
Observations	1810081	1810081	992624
Pseudo R-squared	0.79	0.38	0.21
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A21. Main results: Robustness with standard errors clustered by firms' and individuals' bank

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Treated_i}$	-0.012*	-0.032***	-0.033***
	(0.008)	(0.008)	(0.009)
Observations	1810081	1810081	992624
Adjusted R-squared	0.73	0.37	0.19
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Treated_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Treated_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered by firms' and individuals' bank. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A22. Main results: Individual internalization of same bank risk

	(1)
	Same bank
$\mathrm{Post_t} \times \mathrm{Treated_f} \times \mathrm{Same\ Bank_{i(f)}}$	-0.008
	(0.023)
Sample	Displaced workers
Observations	51234
Adjusted R-squared	0.74
Person FE	Yes
Firm × Year FE	Yes

This table reports the estimated impact of the global liquidity freeze on workers who had a pre-existing relationship with the same (exposed) bank as their employer versus a different (non-exposed) bank than their employer, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $Same\ Bank_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Same\ Bank_{i(f)}) + \beta_3 \cdot (Post_t \times Treated_f \times Same\ Bank_{i(f)}) + \delta \cdot X_{i,pre} + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is a binary variable equal to one for individuals who have the same relationship bank as their employer. $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, and $Same\ Bank_i$ is a binary variable equal to one if individual i and her employer f had the same relationship bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.A23. Bank lending by exposed and non-exposed banks to firms with versus without retail clients

Retail Credit$_{b,f}$ measure:	$\frac{\text{Retail credit exposure}}{\text{Total retail credit}}$		$\frac{\text{Retail credit exposure}}{\text{Local retail credit}}$		ln(Retail credit)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post$_t$ × Treated$_b$	-0.144**	–	-0.162**	–	-0.150*	–
	(0.071)	(–)	(0.072)	(–)	(0.082)	(–)
Post$_t$ × Treated$_b$ × Retail Credit$_{b,f}$	-0.032	-0.033	-0.010	-0.003	-0.063	-0.054
	(0.024)	(0.024)	(0.016)	(0.017)	(0.051)	(0.046)
Observations	40122	40122	40122	40122	40122	40122
Adjusted R-squared	0.48	0.50	0.48	0.50	0.49	0.51
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	No	Yes	No	Yes	No
Bank × Year FE	No	Yes	No	Yes	No	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks to firms with versus without employees who are retail clients, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{b,f,t} = \beta_1 \cdot (Post_t \times Treated_b \times Retail\ Credit_{b,f}) + \beta_2 \cdot (Post_t \times Retail\ Credit_{b,f}) + \gamma_{b,t} + \gamma_{f,t} + \epsilon_{b,f,t}$ where the dependent variable $y_{b,f,t}$ is credit growth between bank b and firm f from year $t-1$ to year t. $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze, and $Retail\ Credit_{b,f}$ captures bank b's retail exposure to the employees of firm f. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. $\gamma_{b,t}$ and $\gamma_{f,t}$ are bank-year and firm-year fixed effects, respectively, and $\epsilon_{b,f,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table I.A24. Main results: Same versus different exposed bank

	(1) ln(Total debt)	(2) ln(Labor earnings)	(3) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times$ Same Exposed $\mathrm{Bank_{i(f)}}$	-0.012***	-0.039***	-0.039***
	(0.005)	(0.015)	(0.013)
$\mathrm{Post_t} \times \mathrm{Treated_f} \times$ Other Exposed $\mathrm{Bank_{i(f)}}$	-0.010*	-0.036***	-0.032***
	(0.006)	(0.009)	(0.010)
Observations	1810081	1810081	992624
Adjusted R-squared	0.73	0.37	0.19
Person FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The table reports the coefficient estimates of β_3 from estimating the following regression model: $y_{i(f),t} = \beta_1 \cdot (Post_t \times Treated_f) + \beta_2 \cdot (Post_t \times Same\ Exposed\ Bank_{i(f)}) + \beta_3 \cdot (Post_t \times Other\ Exposed\ Bank_{i(f)}) + \beta_4 \cdot (Post_t \times Treated_f \times Same\ Exposed\ Bank_{i(f)}) + \beta_5 \cdot (Post_t \times Treated_f \times Other\ Exposed\ Bank_{i(f)}) + \gamma_i + \gamma_{f,t} + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), the natural logarithm of labor earnings in column (2), and the natural logarithm of consumption in column (3). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze, $Same\ Exposed\ Bank_{i(f)}$ is a binary variable equal to one if individual i had a relationship with the same exposed bank as her employer before the onset of the global liquidity freeze, and $Other\ Exposed\ Bank_{i(f)}$ is a binary variable equal to one if individual i had a relationship with a different exposed bank before the onset of the global liquidity freeze. γ_i and $\gamma_{f,t}$ are individual and firm-year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered by individual. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Internet Appendix B

Imputation of consumption expenditures

We impute individual-level annual consumption from the income and wealth data obtained from the Norwegian tax register, following the approach of Jensen and Johannesen (2017) and others.[a] Let c_t denote consumption in year t, y_t disposable income, and W_t net wealth. Based on the budget constraint, total consumption can then be imputed as:

$$c_t = y_t - \Delta W_t, \tag{I.1}$$

where y_t is defined as the sum of total income and capital gains from stocks, net of taxes and interest payments. $\Delta W_t = W_t - W_{t-1}$ is the change in net wealth between year t and $t-1$, where net wealth is calculated as the sum of the market value of securities and balances in savings and checking accounts, minus total outstanding debt. Hence, consumption is simply income less net savings.

We apply a set of filters to exclude observations that the literature has identified as potentially problematic for measurement when using the imputation procedure. First, we restrict the sample to stable households by excluding those that experience a change in the number of adults due to events such as marriage or divorce, which typically involve substantial reshuffling of household balance sheets. Second, we omit business owners or farmers, as the valuation of private equity, business assets, farm equipment, and associated income streams is typically difficult to distinguish from personal assets and income. We also exclude individuals with extreme financial returns. Third, we remove individuals who relocate across municipalities or engage in housing market transactions. While housing transactions are observable in the data, discrepancies between the timing of monetary flows (e.g., purchase price, debt uptake) and the recorded date of the transaction can complicate the accurate allocation of financial changes to the correct calendar year.

[a]For Denmark, the quality of this imputation has been investigated by Browning and Leth-Petersen (2003). They compare data from a Danish Expenditure Survey to administrative data for the years around the survey and conclude that the imputed consumption measure aligns with households' self-reported total expenditures. Koijen, Van Nieuwerburgh, and Vestman (2014) find substantial reporting errors in Swedish consumption survey data and argue for the use of imputed register-based consumption instead.

Internet Appendix C

Bank-level evidence of the credit market disruption

The loan-level results presented in the main analysis are consistent with a differential credit supply shock. To further support this interpretation, we complement the analysis with bank-level balance sheet data and examine whether banks exposed to foreign wholesale funding reduced credit more than non-exposed banks following the global liquidity freeze. While this approach does not allow us to control for borrowers' credit demand, it serves as a useful robustness check for the loan-level estimates.

Specifically, we run the following difference-in-differences model at the bank-level:

$$y_{b,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_t + \epsilon_{b,t} \tag{I.2}$$

where $y_{b,t}$ is the natural logarithm of total credit, retail credit, or corporate credit of bank b in year t. $Post_t$ is a binary variable equal to one after 2008, and zero otherwise. $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding in 2007, and zero otherwise. As mentioned earlier, Panel A of Table 2 shows that exposed and non-exposed banks are similar across a wide range of bank characteristics. Nevertheless, we include a vector of pre-treatment bank controls ($X_{b,pre}$) interacted with year dummies to mitigate concerns about omitted variable bias. The bank controls include banks' size, capital ratio, return on assets, and loan loss provisions to total assets. γ_b and γ_t are bank and year fixed effects, respectively. $\epsilon_{b,t}$ is the error term, which is clustered at the bank-level.

The regression results are reported in Table I.C1, which shows a significantly negative coefficient estimate across the different columns. Our estimates imply a decrease in exposed banks' total lending, corporate lending, and retail lending of 13%, 15%, and 16%, respectively, after the global liquidity freeze. These results hold independent of whether or not we include bank controls, as indicated at the bottom of the table, and are consistent with existing studies on bank lending following the global liquidity freeze (e.g., Chava et al. 2023; Cornett et al. 2011; Ivashina and Scharfstein 2010; Iyer et al. 2014; Jensen and Johannesen 2017; Puri, Rocholl, and Steffen 2011). Further, in Table I.C2, we decompose total retail credit into loans backed by residential property (mainly mortgages and HELOCs) and other retail loans (such as credit cards, car loans, leasing, and other consumer loans). This table shows that the decline in total retail credit reflects a reduction in both collateralized and non-collateralized retail lending. Figures I.C1a–I.C1e illustrate that the parallel trends assumption underlying the difference-in-differences estimates is supported.

Table I.C1. Bank lending by exposed and non-exposed banks following the global liquidity freeze

	ln(Total loans)		ln(Corporate loans)		ln(Retail loans)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathrm{Post_t} \times \mathrm{Treated_b}$	-0.134***	-0.124***	-0.161***	-0.153***	-0.178***	-0.162***
	(0.041)	(0.040)	(0.049)	(0.048)	(0.047)	(0.046)
Observations	954	954	954	954	954	954
Adjusted R-squared	0.88	0.88	0.88	0.88	0.88	0.88
Bank controls	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

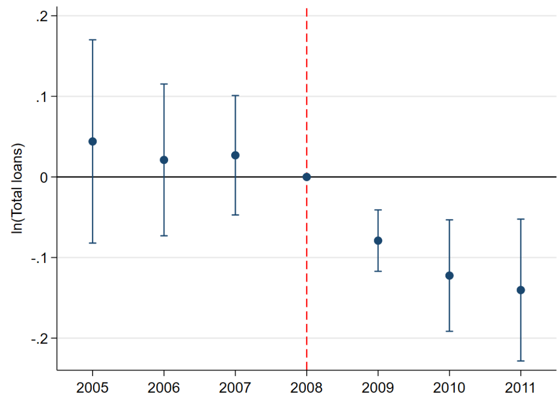
This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{b,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_t + \epsilon_{b,t}$ where the dependent variable $y_{b,t}$ is the natural logarithm of bank b's total lending in columns (1)–(2), the natural logarithm of total corporate lending in columns (3)–(4), and the natural logarithm of total retail lending in columns (5)–(6). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and γ_t are bank and year fixed effects, respectively, and $\epsilon_{b,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table I.C2. Bank lending by exposed and non-exposed banks following the global liquidity freeze: Retail credit decomposition

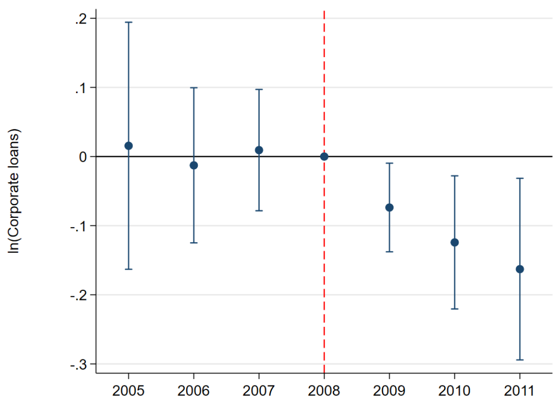
	ln(Retail loans)		ln(Mortgage credit)		ln(Other retail credit)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathrm{Post_t} \times \mathrm{Treated_b}$	-0.178***	-0.162***	-0.170***	-0.161***	-0.182*	-0.177*
	(0.047)	(0.046)	(0.052)	(0.052)	(0.114)	(0.102)
Observations	954	954	954	954	954	954
Adjusted R-squared	0.88	0.88	0.88	0.88	0.81	0.81
Bank controls	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{b,t} = \beta \cdot (Post_t \times Treated_b) + \delta \cdot X_{b,pre} + \gamma_b + \gamma_t + \epsilon_{b,t}$ where the dependent variable $y_{b,t}$ is the natural logarithm of bank b's total retail lending in columns (1)–(2), the natural logarithm of total retail lending backed by residential property (e.g., mortgage credit and HELOCs) in columns (3)–(4), and the natural logarithm of total retail lending not backed by residential property (e.g., credit cards, car loans, leasing, and other retail credit) in columns (5)–(6). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_b$ is a binary variable equal to one for banks that relied on foreign wholesale funding before the onset of the global liquidity freeze. $X_{b,pre}$ is a vector of pre-event bank characteristics interacted with year dummies. γ_b and γ_t are bank and year fixed effects, respectively, and $\epsilon_{b,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

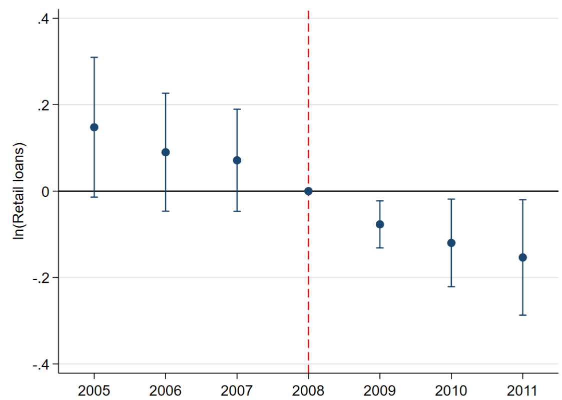
Figure I.C1. Bank lending by exposed and non-exposed banks following the global liquidity freeze: bank-level dynamic difference-in-differences estimates



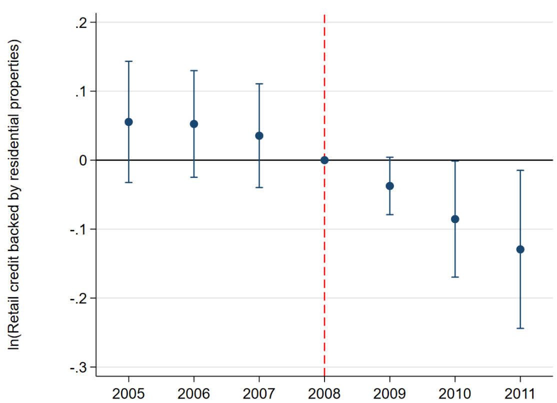
(a) ln(Total loans)



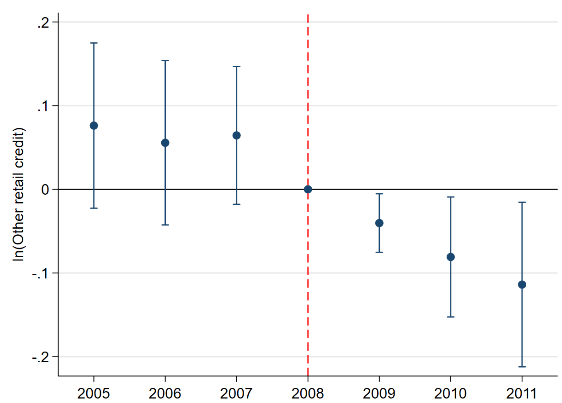
(b) ln(Corporate loans)



(c) ln(Retail loans)



(d) ln(Retail loans backed by residential property)



(e) ln(Retail loans not backed by residential property)

This figure presents the dynamic difference-in-differences estimates of the impact of the global liquidity freeze on bank lending by exposed versus non-exposed banks, after versus before the global liquidity freeze. The y-axis corresponds to the coefficient estimates of β_τ from estimating the following regression model: $y_{b,t} = \sum_{\tau=-3,\,\tau\neq 0}^{3} \beta_\tau \cdot (1_{\tau=t} \times Treated_b) + \gamma_b + \gamma_t + \epsilon_{b,t}$ where the dependent variable $y_{b,t}$ is the natural logarithm of bank b's total loans in Panel (a), the natural logarithm of total corporate loans in Panel (b), the natural logarithm of total retail loans in Panel (c), the natural logarithm of total retail loans backed by residential property (e.g., mortgage credit and HELOCs) in Panel (d), and the natural logarithm of total retail lending not backed by residential property (e.g., credit cards, car loans, leasing, and other retail credit) in Panel (e). $Treated_b$ is a binary variable equal to one if bank b relied on foreign wholesale funding before the onset of the global liquidity freeze. γ_b and γ_t are bank and year fixed effects, respectively, and $\epsilon_{b,t}$ is the error term. The x-axis corresponds to years. The vertical bars represent confidence intervals at the 95% level. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the bank-level.

Internet Appendix D

The impact of credit shocks based on prior empirical approaches

To examine the real impact of disruptions in bank credit, the literature has employed three main approaches, focused on how credit shocks affect firms or individuals. Below, we explain each of these approaches in detail and show that we can replicate their main findings using our data sample, supporting the validity of our empirical setting. However, as explained in our main analysis presented in Section 5, the findings from prior approaches need to be interpreted with caution as they do not account for the fact that credit shocks may *simultaneously* impact both firms and individuals.

First, using firm-level data, we follow the approach of Chodorow-Reich (2014) to show that firms reliant on exposed banks could not compensate the reduction in credit by obtaining credit from other lenders, which led them to reduce employment and investments. To do so, we create a firm-level treatment measure, which is equal to one if a firm's relationship bank is treated, and zero otherwise (where relationship banks are defined as explained in Section 3.3 before). The key idea behind this approach is to leverage the quasi-random exposure of firms to the global liquidity freeze through pre-determined variation in firm-bank relationships. Specifically, we compare otherwise similar firms financed by banks that had differential exposure to the global liquidity freeze. In principle, we can also build a shift-share instrument at the firm-level, in which the shift component is a binary variable equal to one if a bank was exposed to the foreign wholesale funding market and the share components are the shares of a firm's credit with each bank in 2007, but ultimately both approaches yield very similar results as more than 75% of firms in our sample borrow from a single bank. To facilitate the interpretation of the estimates, we therefore use a treatment dummy, though our results are robust to using a treatment intensity measure. Using the firm-level treatment measure, we run the following regression:

$$y_{f,t} = \beta \cdot (Post_t \times Treated_f) + \gamma_f + \gamma_t + \epsilon_{f,t} \tag{I.3}$$

where $y_{f,t}$ corresponds to a set of firm-level outcomes, such as total debt, employment, and investments, and $Treated_f$ is the treatment dummy explained above. γ_f and γ_t are firm and year fixed effects, respectively. The error term, $\epsilon_{f,t}$, is clustered at the firm-level.

The firm-level results are reported in Table I.D1. Column (1) shows that affected firms faced a decline of nearly 9% in total debt following the global liquidity freeze, indicating that they were unable to compensate the reduction in credit by borrowing from new or other lenders. Columns (2)–(4) indicate that this credit cut had real effects, as credit constrained firms reduced investments in fixed assets, employment, and average wages. For instance, the coefficient estimates in Columns (2) and (3) imply that firms reliant on exposed banks reduced investments in fixed assets and employment by 9% and 3%, respectively, which is similar to the elasticities estimated in previous papers (e.g., Chodorow-Reich 2014; Cingano, Manaresi, and Sette 2016; Iyer et al. 2014; Popov and Rocholl 2018).

Second, we study whether firm-level credit constraints affected employees by estimating the following regression model:

$$y_{i(f),t} = \beta \cdot (Post_t \times Treated_f) + \gamma_i + \gamma_t + \epsilon_{i(f),t} \tag{I.4}$$

which is similar to Equation (I.3), with the key distinction that the analysis now examines how firm-level credit shocks affect individual-level outcomes. γ_i and γ_t are individual and year fixed effects, respectively. The error term, $\epsilon_{i(f),t}$, is clustered at the firm-level.

The results are reported in Table I.D2 and show that individuals employed by affected firms are significantly more likely to face layoffs, wage cuts, and a decline in consumption relative to those employed by non-affected firms. The coefficient estimates imply that employees of credit constrained firms are 20% more likely to be laid off, experience wage losses of around 3.5%, and decrease consumption by 1.8%. These estimates are quantitatively consistent with prior research documenting the repercussions of corporate credit supply shocks for both firms and their employees (e.g., see Adamopoulou et al. 2024; Berton et al. 2018). Note that we also find a small decline in individuals' debt, consistent with the notion that banks restrict credit to employees of firms with weak growth or labor market prospects (Carvalho et al. 2023; Correia, Cortes, and Silva 2024).

Third, we examine the real impact of individual-level credit shocks following the approach of Jensen and Johannesen (2017). While the permanent income hypothesis predicts that changes in access to credit should not affect consumption (Carroll 2001), recent studies have documented that bank credit (such as credit cards, HELOCs, and other consumer credit) is an important source of liquidity for consumer spending (e.g., Aydin 2022; Gross and Souleles 2002; Kaplan, Violante, and Weidner 2014).[b] Moreover, given that the vast majority of mortgages in Norway are adjustable-rate, an increase in interest rates raises households' debt service costs, which may consequently lead to a reduction in consumption (Di Maggio et al. 2017). To examine the impact of credit constraints on spending, we create a treatment dummy following the same approach used for firms and run the following individual-level regressions:

$$y_{i,t} = \beta \cdot (Post_t \times Treated_i) + \gamma_i + \gamma_t + \epsilon_{i,t} \tag{I.5}$$

where treatment is now defined based on individuals' personal exposure to the credit shock. γ_i and γ_t are individual and year fixed effects, respectively. The error term, $\epsilon_{i,t}$, is clustered at the

[b]As stated earlier, credit card spending accounted for around 15% of personal consumption expenditures in Norway in 2021, compared to around 25% in the U.S.

individual-level.

The results are reported in Table I.D3. Column (1) shows that individuals reliant on exposed banks experience a 9.9% decline in total debt following the global liquidity freeze, indicating that they cannot offset the reduction in credit by borrowing from alternative lenders. While we cannot distinguish between different types of credit, part of this contraction likely reflects consumption-related credit products—such as reductions in credit card debt and HELOCs (Chava et al. 2023; Puri, Rocholl, and Steffen 2011; Mian and Sufi 2011, 2014)—rather than solely a reduction in mortgage debt. Column (2) further shows that credit constrained individuals reduce consumption by 2.8% relative to non-constrained individuals after the credit crunch. These results are consistent with those from Jensen and Johannesen (2017) for Danish individuals in the aftermath of the global liquidity freeze and, more broadly, confirm that bank credit shocks affect individuals' consumer spending (e.g., also see Agarwal and Qian 2017; Benmelech, Meisenzahl, and Ramcharan 2017; Di Maggio et al. 2017; Mian, Sufi, and Verner 2017).

Figures I.D1–I.D3 presented below show that the parallel trends assumption underlying each of the difference-in-differences models is supported. Further, unreported results show that the results hold using alternative measures of treatment, alternative fixed effects structures, and after including a large set of control variables.

Table I.D1. The impact of firm-side credit constraints on firm outcomes

	(1) ln(Total debt)	(2) ln(Fixed assets)	(3) ln(Employment)	(4) ln(Average wages)
$\mathrm{Post_t} \times \mathrm{Treated_f}$	-0.084***	-0.087***	-0.026***	-0.043***
	(0.030)	(0.009)	(0.005)	(0.004)
Observations	244246	244246	244246	244246
Adjusted R-squared	0.92	0.87	0.89	0.72
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on firms that had a pre-existing relationship with exposed versus non-exposed banks, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{f,t} = \beta \cdot (Post_t \times Treated_f) + \gamma_f + \gamma_t + \epsilon_{f,t}$ where the dependent variable $y_{f,t}$ is the natural logarithm of total debt in column (1), the natural logarithm of total fixed assets in column (2), the natural logarithm of total employees in column (3), and the natural logarithm of average wages in column (4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_f$ is a binary variable equal to one if firm f had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_f and γ_t are firm and year fixed effects, respectively, and $\epsilon_{f,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the firm-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.D2. The impact of firm-side credit constraints on individual outcomes

	(1) ln(Total debt)	(2) Displaced	(3) ln(Labor earnings)	(4) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_f}$	-0.022***	0.004***	-0.035***	-0.018***
	(0.008)	(0.001)	(0.004)	(0.004)
Observations	1889531	1889531	1889531	1063897
Adjusted R-squared	0.69	0.22	0.35	0.18
Person FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

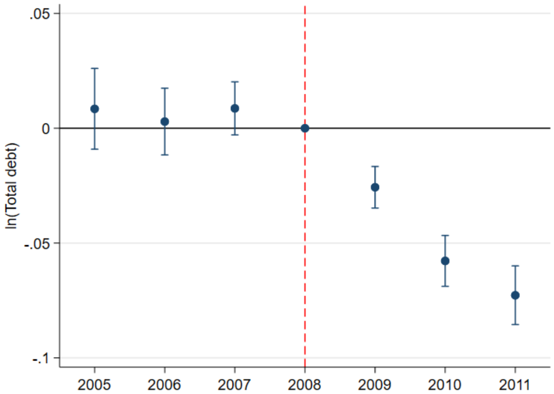
This table reports the estimated impact of the global liquidity freeze on employees of firms that had a pre-existing relationship with exposed versus non-exposed banks, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{i(f),t} = \beta \cdot (Post_t \times Treated_f) + \gamma_i + \gamma_t + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in column (1), a dummy variable equal to one for displaced workers in column (2), the natural logarithm of labor earnings in column (3), and the natural logarithm of consumption in column (4). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and γ_t are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

Table I.D3. The impact of individual-side credit constraints on individual outcomes

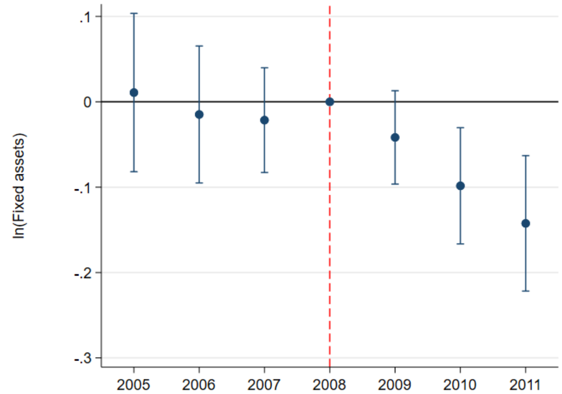
	(1) ln(Total debt)	(2) ln(Consumption)
$\mathrm{Post_t} \times \mathrm{Treated_i}$	-0.099***	-0.028***
	(0.008)	(0.009)
Observations	1889531	1063897
Adjusted R-squared	0.73	0.18
Person FE	Yes	Yes
Year FE	Yes	Yes

This table reports the estimated impact of the global liquidity freeze on individuals who had a pre-existing relationship with exposed versus non-exposed banks, after versus before the global liquidity freeze. The table reports the coefficient estimates of β from estimating the following regression model: $y_{i,t} = \beta \cdot (Post_t \times Treated_i) + \gamma_i + \gamma_t + \epsilon_{i,t}$ where the dependent variable $y_{i,t}$ is the natural logarithm of total debt in column (1), and the natural logarithm of consumption in column (2). $Post_t$ is a binary variable equal to one after the onset of the global liquidity freeze, and $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and γ_t are individual and year fixed effects, respectively, and $\epsilon_{i,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

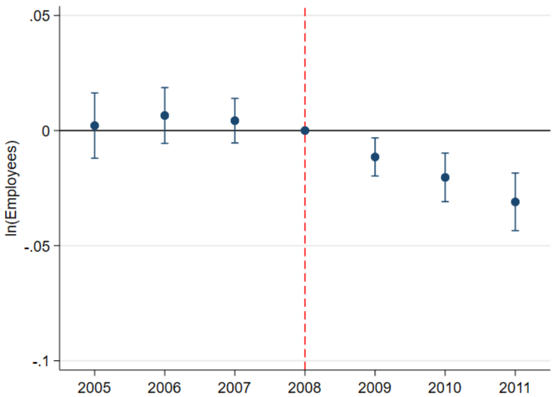
Figure I.D1. The impact of firm-side credit constraints on firm outcomes: Dynamic difference-in-differences estimates



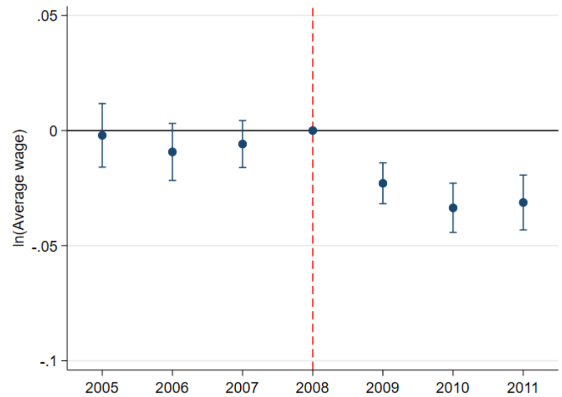
(a) ln(Total debt)



(b) ln(Fixed assets)



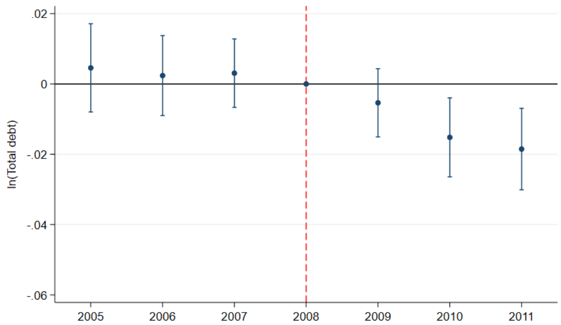
(c) ln(Employees)



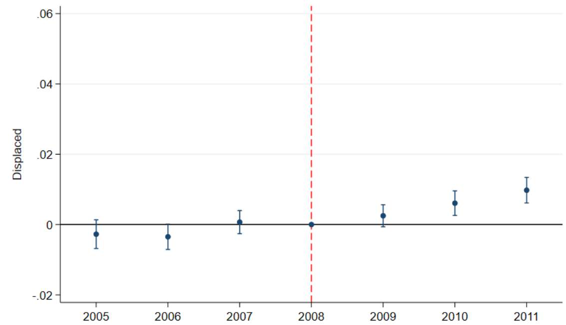
(d) ln(Average wage)

This figure presents the dynamic difference-in-differences estimates of the impact of the global liquidity freeze on firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The y-axis corresponds to the coefficient estimates of β_τ from estimating the following regression model: $y_{f,t} = \sum_{\tau=-3,\,\tau\neq 0}^{3} \beta_\tau \cdot (1_{\tau=t} \times Treated_f) + \gamma_f + \gamma_t + \epsilon_{f,t}$ where the dependent variable $y_{f,t}$ is the natural logarithm of total debt in Panel (a), fixed assets in Panel (b), total employees in Panel (c), and average wage in Panel (d). $Treated_f$ is a binary variable equal to one if firm f had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_f and γ_t are firm and year fixed effects, respectively, and $\epsilon_{f,t}$ is the error term. The x-axis corresponds to years. The vertical bars represent confidence intervals at the 95% level. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the firm-level.

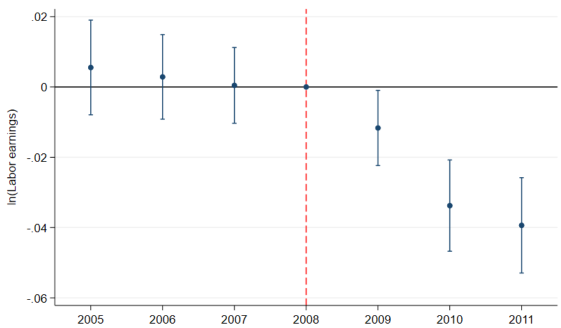
Figure I.D2. The impact of firm-side credit constraints on individual outcomes: Dynamic difference-in-differences estimates



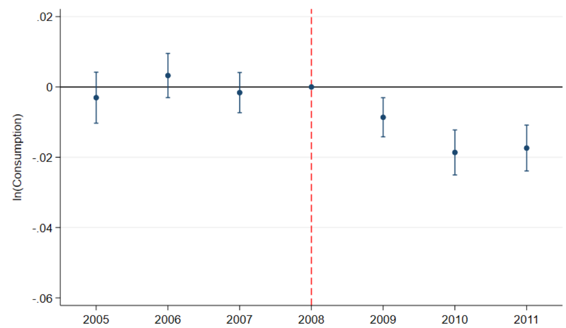
(a) ln(Total debt)



(b) Displaced



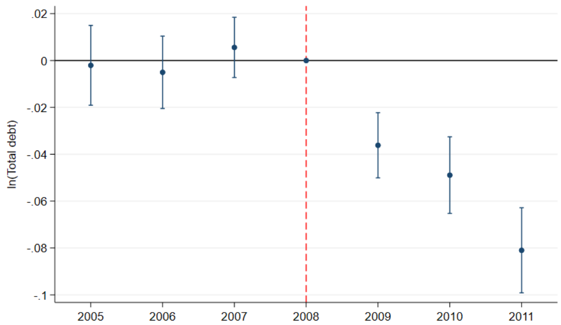
(c) ln(Labor earnings)



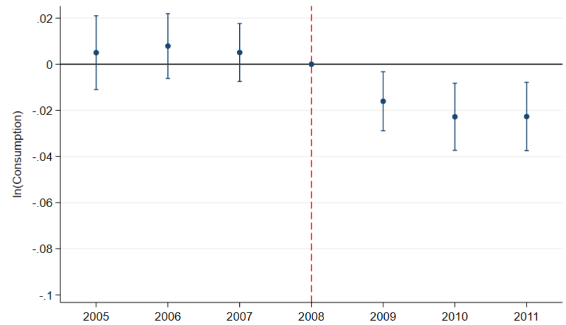
(d) ln(Consumption)

This figure presents the dynamic difference-in-differences estimates of the impact of the global liquidity freeze on individuals employed by firms that had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The y-axis corresponds to the coefficient estimates of β_τ from estimating the following regression model: $y_{i(f),t} = \sum_{\tau=-3,\,\tau\neq 0}^{3} \beta_\tau \cdot (1_{\tau=t} \times Treated_f) + \gamma_i + \gamma_t + \epsilon_{i(f),t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in Panel (a), a binary variable equal to one if the individual is displaced in Panel (b), the natural logarithm of labor earnings in Panel (c), and the natural logarithm of consumption in Panel (d). $Treated_f$ is a binary variable equal to one if individual i's employer f had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and γ_t are individual and year fixed effects, respectively, and $\epsilon_{i(f),t}$ is the error term. The x-axis corresponds to years. The vertical bars represent confidence intervals at the 95% level. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level.

Figure I.D3. The impact of individual-side credit constraints on individual outcomes: Dynamic difference-in-differences estimates



(a) ln(Total debt)



(b) ln(Consumption)

This figure presents the dynamic difference-in-differences estimates of the impact of the global liquidity freeze on individuals who had a pre-existing relationship with an exposed versus non-exposed bank, after versus before the global liquidity freeze. The y-axis corresponds to the coefficient estimates of β_τ from estimating the following regression model: $y_{i,t} = \sum_{\tau=-3,\,\tau\neq 0}^{3} \beta_\tau \cdot (1_{\tau=t} \times Treated_i) + \gamma_i + \gamma_t + \epsilon_{i,t}$ where the dependent variable $y_{i(f),t}$ is the natural logarithm of total debt in Panel (a), the natural logarithm of labor earnings in Panel (b), and the natural logarithm of consumption in Panel (c). $Treated_{i(f)}$ is a binary variable equal to one if individual i had a relationship with an exposed bank before the onset of the global liquidity freeze. γ_i and γ_t are individual and year fixed effects, respectively, and $\epsilon_{i,t}$ is the error term. The x-axis corresponds to years. The vertical bars represent confidence intervals at the 95% level. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the individual-level.

Internet Appendix E

Cross-country evidence

We examine whether the mechanism uncovered in our baseline results can explain heterogeneity in the macroeconomic impact of historical banking crises (Braun and Larrain 2005; Dell'Ariccia, Detragiache, and Rajan 2008; Kroszner, Laeven, and Klingebiel 2007). To do so, we combine data from the Global Macro Database (Müller et al. 2025) and the Global Credit Project (Müller and Verner 2024). The former contains harmonized country-level information on economic activity and the timing of banking crises, among others. The latter contains credit aggregates for households and the non-financial sector. Our final sample consists of an unbalanced panel of approximately 1,500 country-year observations covering 46 countries over the period 1948–2014. Table I.E1 provides an overview of the countries included in the sample, their respective sample periods, and the number of banking crisis years within each country's sample period.

Using these data, we test whether banking crises lead to more pronounced declines in economic activity in economies where both firms and households are ex-ante more reliant on (bank) debt.[c] Specifically, we estimate the following regression model:

$$
\begin{aligned}
y_{c,t} = &\delta_1 \cdot \big(Banking\ crisis_t \times Corporate\ Debt/GDP_{c,pre}\big) + \\
&\delta_2 \cdot \big(Banking\ crisis_t \times Household\ Debt/GDP_{c,pre}\big) + \\
&\delta_3 \cdot \big(Banking\ crisis_t \times Corporate\ Debt/GDP_{c,pre} \times Household\ Debt/GDP_{c,pre}\big) + \\
&\lambda \cdot C_{c,pre} + \gamma_c + \gamma_t + \epsilon_{c,t}
\end{aligned}
\tag{I.6}
$$

where $y_{c,t}$ denotes total consumption, GDP, investment, or unemployment in country c and year t. $Banking\ crisis_t$ is an indicator equal to one in the year a banking crisis begins and in the subsequent two years (as in Dell'Ariccia, Detragiache, and Rajan 2008). $Corporate\ Debt/GDP_{c,pre}$ and $Household\ Debt/GDP_{c,pre}$ measure pre-crisis credit to non-financial businesses and households, respectively, scaled by GDP. $C_{c,pre}$ is a vector of pre-crisis controls, including population, inflation, the real exchange rate, long-term interest rates, government debt-to-GDP, exports-to-GDP, imports-to-GDP, and indicators for sovereign debt crises and currency crises. γ_c and γ_t are country and year fixed effects, respectively. Standard errors are clustered at the country-level.

The coefficient of interest is δ_3. If banking crises are episodes in which credit supply contracts broadly across the economy, their real effects should be particularly severe in countries where both firms and households enter the crisis with high exposure to bank debt. Consistent with the amplification mechanism documented in our micro-level and municipality-level analyses, we

[c] A potential limitation of the cross-country data is that we cannot separately observe bank debt and non-bank debt. Nevertheless, interpreting corporate and household debt-to-GDP as proxies for reliance on bank credit is plausible in our context since banks have historically been the main providers of credit to both firms and households, while non-bank lenders played a comparatively minor role over most of our sample period (see Ivashina et al. 2024).

therefore expect δ_3 to be economically large and statistically significant.

Descriptive statistics for the variables used in the cross-country analysis are reported in Table I.E2. Around 3% of the sample years correspond to banking crisis years, implying that nearly 10% of observations fall in years during or immediately following a banking crisis. On average, corporate debt-to-GDP and household debt-to-GDP ratios are approximately 0.4 and 0.3, respectively. The correlation between the two measures is moderate, at around 0.5.

The results from estimating Equation (I.6) are reported in Table I.E3. Overall, the cross-country evidence supports our baseline results, as we find that banking crises generate the most severe real effects in economies where both firms and households are ex-ante more reliant on debt. In particular, Table I.E3 shows that only the triple-interaction coefficient, δ_3, is statistically significant. This indicates that banking crises are associated with large declines in consumption, GDP, and investment precisely in countries where firm-side and household-side debt dependence is jointly high. While the coefficient estimate in column (4) is not statistically significant, it is economically large and positive, suggesting that the same mechanism is also associated with larger increases in unemployment.

Table I.E1. List of countries included in the cross-country sample

Country	Sample period	Banking crisis years
Argentina	1994–1997	1
Australia	1960–2014	1
Austria	1965–2014	1
Belgium	1960–2014	1
Brazil	1997–2014	0
Canada	1960–2014	1
Chile	2004–2014	0
Colombia	2003–2014	0
Cyprus	1997–2014	1
Denmark	1951–2014	2
Fiji	2001–2014	0
Finland	1960–2014	1
France	1960–2014	1
Germany	1970–2014	1
Ghana	2006–2013	0
Greece	1973–2014	1
Hungary	1999–2014	1
Iceland	1970–2014	3
India	2000–2012	0
Ireland	1970–2014	1
Israel	1997–2014	0
Italy	1948–2014	2
Japan	1960–2014	3
Luxembourg	1999–2014	1
Malaysia	1992–2014	1
Mauritius	2002–2014	0
Mexico	1991–2014	1
Morocco	1997–2014	0
Nepal	2004–2014	0
Netherlands	1990–2014	1
New Zealand	1960–2014	0
Norway	1960–2014	2
Pakistan	1982–2014	0
Philippines	1994–2014	1
Portugal	1960–2014	2
South Africa	1994–2014	0
South Korea	1973–2014	1
Spain	1948–2014	2
Sri Lanka	2009–2014	0
Sweden	1950–2014	3
Switzerland	1970–2014	2
Thailand	2000–2014	0
Turkey	2005–2014	0
United Kingdom	1960–2014	3
United States	1960–2014	3
Venezuela	2001–2013	1

This table reports the countries included in the sample used for our cross-country analysis, the sample period for which the variables of each country are observed, and the number of banking crisis years over that period.

Table I.E2. Summary statistics for the cross-country sample

	N	Mean	Median	SD	P5	P95
ln(Consumption)	1518	11.563	11.695	2.120	8.396	14.629
ln(GDP)	1518	11.853	11.998	2.132	8.681	14.868
ln(Investment)	1518	10.444	10.518	2.048	7.171	13.377
Unemployment rate (%)	1518	6.297	5.487	4.532	1.100	15.100
Banking crisis	1518	0.030	0.000	0.170	0.000	0.000
Corporate debt/GDP	1518	0.392	0.347	0.238	0.126	0.809
Household debt/GDP	1518	0.294	0.254	0.235	0.022	0.738
ln(Population)	1518	2.959	2.851	1.314	0.667	5.182
Inflation rate (%)	1518	5.460	3.596	5.401	0.596	16.560
Real effective exchange rate (2010=100)	1518	104.498	100.403	21.333	73.691	147.370
Long-term interest rate (%)	1518	7.717	6.817	3.968	2.927	15.400
Government debt/GDP (%)	1518	44.131	39.643	28.382	8.221	98.540
Exports/GDP (%)	1518	32.186	27.142	20.561	9.316	75.629
Imports/GDP (%)	1518	31.833	28.474	17.540	9.802	67.512
Sovereign debt crisis	1518	0.001	0.000	0.036	0.000	0.000
Currency crisis	1518	0.021	0.000	0.144	0.000	0.000

This table provides summary statistics for the key variables used in our cross-country analysis.

Table I.E3. Cross-country evidence based on banking crises and heterogeneity in corporate and household debt-to-GDP

	(1) ln(Consumption)	(2) ln(GDP)	(3) ln(Investments)	(4) Unemployment
Banking crisis_{t}	-0.05	-0.06	-0.12	0.51
	(0.07)	(0.06)	(0.08)	(1.17)
Banking crisis_{t} × Corporate debt/$\text{GDP}_{\text{c,pre}}$	0.19	0.23	0.18	-1.71
	(0.17)	(0.15)	(0.21)	(2.10)
Banking crisis_{t} × Households debt/$\text{GDP}_{\text{c,pre}}$	0.16	0.17	0.35	-0.35
	(0.13)	(0.14)	(0.24)	(2.38)
Banking crisis_{t} × Corporate debt/$\text{GDP}_{\text{c,pre}}$ × Households debt/$\text{GDP}_{\text{c,pre}}$	-0.40^{*}	-0.43^{**}	-0.60^{**}	2.36
	(0.17)	(0.15)	(0.22)	(3.28)
Observations	1518	1518	1518	1518
Adjusted R-squared	0.95	0.95	0.94	0.76
F-stat	11.48	12.27	25.98	70.20
Prob>F	0.00	0.00	0.00	0.00
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

This table reports the estimated impact of banking crises on country-level measures of economic activity, comparing countries in which non-financial businesses and households are more versus less reliant on debt, after versus before a banking crisis. The table reports the coefficient estimates of β_1, β_2, and β_3 from estimating the following regression model: $y_{c,t} = \delta_1 \cdot \big(Banking\ crisis_t \times Corporate\ Debt/GDP_{c,pre}\big) + \delta_2 \cdot \big(Banking\ crisis_t \times Household\ Debt/GDP_{c,pre}\big) + \delta_3 \cdot \big(Banking\ crisis_t \times Corporate\ Debt/GDP_{c,pre} \times Household\ Debt/GDP_{c,pre}\big) + \lambda \cdot C_{c,pre} + \gamma_c + \gamma_t + \epsilon_{c,t}$ where the dependent variable $y_{f,t}$ is the natural logarithm of total consumption in column (1), the natural logarithm of total GDP in column (2), the natural logarithm of total investments in column (3), and the unemployment rate in column (4). $Banking\ crisis_t$ is an indicator equal to one in the year a banking crisis begins and in the subsequent two years (as in Dell'Ariccia, Detragiache, and Rajan 2008). $Corporate\ Debt/GDP_{c,pre}$ and $Household\ Debt/GDP_{c,pre}$ capture pre-crisis credit to non-financial businesses and households, respectively, scaled by GDP. $C_{c,pre}$ is a vector of pre-crisis controls. γ_c and γ_t are country and year fixed effects, respectively, and $\epsilon_{f,t}$ is the error term. At the bottom of the table, we indicate the set of fixed effects included in the corresponding regressions. Table A1 in the Appendix provides information about the variable definitions. Standard errors are clustered at the country-level. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level

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