ESSAYS ON FINANCIAL INTERMEDIATION AND HOUSEHOLD FINANCE

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To my parents,

Geeta Paradkar and Dilip Paradkar,

my brother, Nitish,

and my fiancée, Nehal,

for their endless love, support, and encouragement.

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SUMMARY

This dissertation consists of three essays on the intersection of financial intermediation and household finance. In the first essay, using proprietary account-level data from a major credit bureau, I examine the impact of stress tests on bank risk-taking in the U.S. consumer credit card market. I decompose credit supply and demand effects by exploiting credit card–level data on limits and balances matched to both consumers and banks. For the *same* consumer, I examine the lending response of banks experiencing higher stress test–induced capital requirements (i.e., high-exposure banks) relative to less-exposed banks. I find that the earlier rounds of stress tests induced high-exposure banks to sharply reduce credit limits, especially for ex-ante risky borrowers. In contrast, in later rounds of stress tests, high-exposure banks increased limits for risky consumers. Consistent with higher bank risk-taking in later rounds, cards issued by highly exposed banks have a higher ex-post likelihood of default. Additionally, I document that more affected non-prime borrowers are more likely to default subsequently, and that this effect is markedly pronounced for the low-income and less-educated consumer segments. My findings suggest that stress test–induced increases in capital requirements can encourage higher bank risk-taking, with distributional consequences for consumer creditworthiness.

In the second essay, using comprehensive credit bureau data, we study how obtaining marketplace lending (MPL) credit impacts consumers' future borrowing capacities and outcomes. We find that MPL borrowers' credit scores improve temporarily after loan origination relative to observably similar bank borrowers and borrowers with unmet credit demand, but MPL borrowers default at higher rates subsequently. We show that the initial improvement in capacities is somewhat mechan-

ical, while the subsequent deterioration in outcomes indicates MPLs' screening is weaker relative to banks. MPL screening relative to banks is especially weaker when banks have relationship-based information and when MPL platforms provide less information to MPL investors.

In the third essay, using comprehensive credit card–borrower–bank matched data of approximately 500 million credit cards in the U.S., we analyze how a sharp unexpected decline in banks' short-term wholesale funding in 2008 affected their consumers. We decompose credit supply and demand effects using the sudden dry-up of short-term wholesale funding (which accounted for 17.8% of bank funding pre-2008) and account-level data on credit card limits and balances. For the same consumer, credit card issuers experiencing a 10% greater decline in wholesale funding reduced credit limits by 0.9% more relative to other issuers. Consumers' aggregate card balances decreased by 0.32% for a 1% reduction in aggregate limits induced by the wholesale funding liquidity shock. We document significant heterogeneity in the pass-through of the bank liquidity shocks with banks cutting credit limits by more for credit-constrained consumers (e.g., lower credit-score and higher credit utilization consumers). These consumers respond by cutting their consumption as they are less able to borrow from alternate sources. Moreover, this consumption decline is long-lasting for these credit-constrained consumers. Our results highlight that when banks face liquidity shocks, they are more likely to pass on these shocks to consumers who are least able to hedge against them. Consequently, our results show *who* bears the real costs of fragile bank funding structures.

CHAPTER 1

REDUCING RISK OR REACHING FOR YIELD? IMPACT OF STRESS TESTS ON CREDIT CARD LENDING

1.1 Introduction

In the post-crisis period, stress tests have emerged as the primary tool of macroprudential policy in the enhanced supervision of systematically important institutions. Stress tests are inherently forward-looking capital requirements implemented by the Federal Reserve. They gauge the resiliency of large bank holding companies (BHCs) to withstand hypothetical adverse economic scenarios through adequate capitalization and continue their lending activities. The stated intention behind stress tests is to promote financial stability, increase transparency in the banking sector, and improve market discipline ([1, 2, 3]). In this paper, I document that stress test–induced increases in capital requirements may have had the unintended effect of increasing bank risk-taking in the U.S. consumer credit card market.

Prior theoretical and empirical literature provides mixed inferences regarding the impact of higher capital requirements on the incentives of regulated banks to engage in risky lending. By requiring banks to have more "skin in the game" through higher capital against their risks, stress tests may reduce banks' incentives to engage in excessive risk-taking ([4, 5]). Higher capital is also associated with increased charter values, which lowers the likelihood of future failure, and thus leads to reductions in risky lending ([6]). On the other hand, higher capital could result in increases in risky lending due to strengthened monitoring incentives ([7]), incentives to trade off reduced leverage risk against higher credit risk ([8, 9, 10]), or incentives to search for yield ([11]).

Using proprietary credit card account–level data from a major credit bureau, I find that, over time, stress test–induced increases in capital requirements have progressively encouraged increased bank risk-taking. Using a *within*-consumer empirical design, I show that the earlier rounds of stress tests induced highly exposed banks to sharply reduce limits for ex-ante risky consumers. However, in later rounds, more exposed banks have *increased* limits for the risky consumer segment. I find evidence suggesting that this transition from risk-reduction to risk-seeking behavior is consistent with banks adapting to stress tests over time. Consistent with higher bank risk-taking in later cycles, I show that cards issued by highly exposed banks have a higher ex-post likelihood of default. Importantly, I document that non-prime borrowers who have a larger percentage of their total credit card limits issued by banks with larger implied capital requirements default at higher rates compared to less-exposed borrowers. As a result, more affected non-prime consumers are also more likely to experience credit score declines when compared to non-prime consumers with no exposure to stress-tested banks. Lastly, my findings suggest that such effects are more pronounced for consumers with lower incomes, lower education levels, and jobs that do not require sophisticated skill sets.

For several reasons, the U.S. consumer credit card market is an ideal experimental setting to study how changes in capital requirements affect bank risk-taking behavior, and examine any resulting implications for households. Given the unsecured nature of credit card lending, card-issuing banks bear significant exposure to borrowers' credit risk, since unsecured claims are generally completely wiped out in the event of consumer bankruptcy. Reflecting their intrinsically risky nature, credit cards also have the highest charge-off rates of any products offered by banks ([12]).[1] Moreover, credit card lending is almost completely standards-based, with a heavy emphasis on consumer credit scores ([13]). As a result, I can more cleanly identify bank risk-taking behavior in a setting with a limited role for soft information. Furthermore, in terms of bank commitments, the market is also economically significant, with approximately $4.3 trillion in aggregate extended credit card lines.[2]

[1]Moreover, credit card charge-off rates have increased significantly in recent years. Credit card charge-offs made up approximately 83% of all charge-offs, up from 67% three years ago. Source: `https://www.bloomberg.com/news/articles/2019-05-01/u-s-banks-bad-debt-pile-creeps-higher-with-credit-card-losses`.

[2]Source: `https://files.consumerfinance.gov/f/documents/cfpb_consumer-credit-`

The credit card market is also important from the perspective of consumers. With nearly 170 million U.S. consumers owning at least one credit card, this market is the largest U.S. consumer lending market in terms of the number of users. Moreover, credit cards are an important source of marginal financing for most U.S. households.[3] Lastly, there is wide heterogeneity in consumers' marginal propensity to borrow on credit cards across different consumer segments (see [14, 15]). As a result, adjustments in bank risk-taking behavior in response to changing capital requirements can have a significant impact on consumers. Moreover, the consequences of bank risk-taking can be distributed *unequally* across different consumer demographics.

The primary identification challenge is to isolate changes in credit supply from changes in credit demand when studying credit card lending. It is possible that the economic forces that affect a bank's exposure to a particular stress test cycle can also affect consumer demand. However, I use account-level data on credit card limits, which more cleanly reflect a bank's credit supply function. As a result, I am able to tease out the marginal propensity to lend (MPL) of banks in response to stress tests, where I measure this MPL through changes in account-level limits. This approach, thus, requires me to estimate the extent to which banks are affected by stress tests conducted by the Federal Reserve.

I approximate banks' vulnerability to stress tests by constructing the measure of *stress test exposure* described in [16]. This measure captures cross-sectional variation in the extent to which banks are impacted by the stress test in any given testing cycle.[4] This stress test exposure measure equals the difference between the BHC's current capital ratio at the outset of any stress test cycle and the lowest capital ratio implied under a hypothetical, forward-looking severely adverse economic sce-

card-market-report_2019.pdf

[3]In a recent report on the economic well-being of consumers, researchers at the Federal Reserve noted that 40% of U.S. households cannot cover an unexpected emergency expense of $400. Moreover, 43% of these fragile consumers stated they would use credit cards to cover such unexpected expenses, and pay them off over time. Source: https://www.federalreserve.gov/publications/files/2017-report-economic-well-being-us-households-201805.pdf.

[4]Note that I restrict my analysis to banks that undergo stress tests implemented by the Federal Reserve. This approach helps circumvent potentially problematic comparisons between stress-tested and non-tested banks, which differ on many dimensions other than size.

nario specified by the Fed. It is thus intended to capture the potential decline in bank capital under stressed economic conditions. Banks facing higher declines are more likely to experience regulatory interference. The key underlying identification assumption is that stress-tested banks are not able to perfectly predict their performance in the hypothetical scenarios modeled by the Fed.[5]

In each testing cycle, I focus on individuals who have credit cards from multiple stress-tested banks to implement a fixed-effects methodology similar to that of [17]. This fixed-effects methodology compares how the credit limits on credit cards issued to the *same* individual in a given cycle change as a function of the issuing bank's exposure to the stress test conducted in that particular cycle. Effectively, I compare credit limit changes *within*-consumer and *within*–stress test cycle. Thus, I control for any time-varying individual-specific demand factors within a stress test cycle (e.g., changes in credit score or income) that can affect a bank's credit extension to an individual. Moreover, this *within*-consumer empirical design helps capture the impact of the *relative* increases in stress test–induced capital requirements of multiple banks lending to the *same* consumer.

As part of my analysis, I first group consumers into 20-point credit score bins based on their credit scores at the outset of each testing cycle. Next, I examine the credit card lending response of high-exposure banks to consumers in different score bins separately for each cycle.[6] This helps identify whether the lending response of high-exposure banks to consumers of varying creditworthiness changes over time. My results reveal an interesting pattern. Consistent with the risk-reduction channel of higher capital requirements, I find that earlier rounds of stress tests induced high-exposure banks to reduce limits extended to risky consumers. Indeed, for the 2009 and 2012 rounds of stress tests (where bank performance was publicly disclosed), I find that borrowers with low creditworthiness experienced larger reductions in credit card limits. However, for more recent rounds of stress

[5]Note that this assumption is likely to be met since stress-tested banks are not privy to the internal models implemented by the Fed to simulate bank performance in stressed economic scenarios. Thus, stress test outcomes always contain a significant non-predictable component for stress-tested banks. More details about stress tests and the construction of the stress test exposure measure can be found in Section 1.2 and Section 1.3.2, respectively.

[6]Throughout the paper, I label *high-exposure* banks as those that have a higher stress test exposure than *low-exposure* banks.

tests, my results are more consistent with the reaching-for-yield incentives generated by higher capital requirements. For these later stress tests, I find that high-exposure banks continue to reduce limits for prime consumers, but they *increase* limits for the risky, non-prime segment.

I attempt to uncover the mechanism that mediates the transition of stress-tested banks from risk-reduction to risk-seeking behavior in response to higher implied capital requirements. I do so by first restricting my analysis to only the credit card issuers that have undergone all the Comprehensive Capital Analysis and Review (CCAR) stress test exercises conducted from 2012 to 2016. This step mitigates concerns that my results are driven by the addition of new banks to the stress-testing regime in more recent cycles. In addition, I apply the bank-level stress test exposure measure as computed in the 2012 CCAR exercise to all subsequent cycles. This ensures that my findings are not driven by changes in the *relative sorting* of distinct banks' exposures to stress tests across different cycles. I find that my pattern of findings is largely unaffected by these modifications. Moreover, the transition appears to be gradual, with increasing magnitudes of risky lending over time. Overall, these findings suggest that banks adapt to stress test–induced changes in capital requirements with each passing cycle.

I also test whether my results hold in a pooled setting by concatenating the five CCAR stress tests conducted from 2012 to 2016. Next, I progressively saturate the specification with the equivalent of bank$\times$year-month$\times$stress-test-cycle and consumer$\times$year-month$\times$stress-test-cycle fixed effects to absorb all time-varying, observed and unobserved, bank and consumer heterogeneity in a particular stress test cycle. This implies that I analyze the supply of credit card limits from banks with varying stress test exposure to the *same* consumer in the *same* stress test cycle. I find that a bank with a one standard deviation larger stress test exposure reduced its credit supply by approximately \$418, based on the pooled-sample average. However, this reduction in credit supply is borne almost entirely by the prime (i.e., non-risky) consumer segment. I find that this "within-individual-stress test cycle" result is robust to a battery of robustness tests. I show that my results are not likely to be driven by bank-specific individual demand or by any particular bank. Moreover, I show that these results are robust to alternative definitions of stress test exposures and alternative measures of consumer constraints. Thus, my results suggest that stress tests induce high-exposure banks to tilt the *composition* of their credit supply toward risky, non-prime consumers.

Issuing higher limits on credit cards supplied to non-prime consumers will not necessarily indicate greater risk-taking if extended limits remain unused. However, I find that, on average, a $1 stress test exposure–induced change in card-level limits is associated with a significant 79.3 cent change in card-level balances for non-prime consumers. In contrast, prime consumers increase balances by only 16.8 cents for a one-dollar increase in card-level limits induced by stress tests.[7] I also examine the respective balance response on cards issued by high-exposure banks across consumers grouped in 20-point credit score bins in each stress test cycle. Consistent with my analysis of account-level limits, I find that non-prime consumers reduced balances on cards issued by high-exposure banks in the earlier rounds of stress tests. However, in later cycles, non-prime consumers increased balances on high-exposure credit cards.

Consistent with higher risk-taking, I find that not only do high-exposure banks extend higher limits to ex-ante risky consumers, but that the cards issued by high-exposure banks also face a higher likelihood of default in the future. Using the fixed effects *within*-individual estimator, I find that non-prime consumers are approximately 6–8% more likely to default on high-exposure cards relative to cards issued by low-exposure banks to the *same* individual. This analysis is conducted at the credit card account level with a *within*-consumer empirical design; therefore, any changes in individual-level characteristics (e.g., job loss, income declines, or medical problems) are unlikely to explain non-prime consumers' relatively higher probability of defaulting on cards issued by high-exposure banks.

Given the near-complete hard information–driven nature of credit card lending, banks may op-

[7]Note that this is consistent with the general finding that non-prime consumers are significantly more likely to carry balances on credit cards, while prime consumers are more likely to use credit cards for transactional purposes. Source: `https://files.consumerfinance.gov/f/documents/bcfp_data-point_credit-card-revolvers.pdf`

timally respond to the stress test–induced increase in capital requirements by searching for yield in the non-prime consumer segment. However, if additional credit is extended to consumers whose sophistication levels are not wholly explained by credit scores, then it is possible that the associated ex-post credit card delinquencies are unequally distributed across different consumer demographics. Thus, I proxy for consumer sophistication through income, education, and occupation. I find that consumers who have lower income, lack a college education, or have jobs that do not require sophisticated skills are most likely to default on cards issued by high-exposure banks. Importantly, this finding continues to hold even within the non-prime consumer segment, which indicates that income, education, and occupation are not perfectly correlated with credit scores.

Next, I document some possible adverse consequences of higher consumer reliance on credit cards issued by high-exposure banks on borrower creditworthiness. Using the sample of *all* credit cards issued to borrowers in my sample, I construct a consumer-level *weighted exposure* measure on the basis of card-level limits weighted by the issuing banks' exposure to stress tests. Consumers who have a more significant percentage of their total limits issued by banks that are more adversely impacted by stress tests thus have a larger weighted exposure. I find that such high-exposure consumers are more likely to default ex post on credit cards, and that this effect is significantly larger in the non-prime segment. Moreover, I also show that non-prime consumers with the median level of *weighted exposure* are approximately 5–6% less likely to cross the industry-standard credit score threshold of 680 and turn prime compared to non-prime consumers with no exposure to stress-tested banks. These credit score thresholds have been shown to matter for bank lending decisions ([18]). More generally, changes in credit scores are also known to mediate a large number of dynamic responses in credit markets (see [19, 14, 20, 21]). Lastly, I show that these adverse effects on creditworthiness are magnified for consumers with lower income, less education, and unsophisticated jobs.

My paper contributes to several strands of literature. First, this paper connects to research that analyzes the effects of stress testing on credit supply, which has yielded mixed evidence. While

[22] find that the annual CCAR stress test exercises do not affect bank loan growth in general, other papers such as [23] and [16] show that stress tests have resulted in reduced small business lending, with possibly negative consequences for entrepreneurship and innovation ([24]). In the syndicated loan market, existing research finds that banks appear to lend more prudently following the initiation of stress tests by cutting credit supply to non–investment grade firms (see [5, 25, 26]). Using European data, [27] similarly document that banks responded to the 2011 EBA capital exercise by reducing risky lending. In addition, [28] offer evidence that stress-tested banks cut back on jumbo mortgage lending. I add to this strand of research on two dimensions. First, I document the impact of stress tests on consumer credit card lending. More importantly, I document that banks' response to stress tests varies with time, and identify the effect of stress test–induced increases in capital requirements on the *compositional* supply of bank credit.

My paper adds to the relatively scarce empirical literature on capital regulation and bank risk-taking. [29] document that the introduction of a countercyclical capital buffer in Switzerland induced increased bank risk-taking. Using Spanish data, [30] find that higher provision requirements induced banks with more substantial exposures to focus their credit supply to firms with higher ex-ante interest paid and leverage, and with higher likelihoods of ex-post default, which is consistent with higher risk-taking. Finally, using U.K. data, [31] analyze the impact of capital requirements on mortgage loan size and risk-shifting behavior. They find that riskier borrowers are not affected by the reduction in the loan size offered by affected banks. I contribute to this literature by documenting that banks can respond to increased capital requirements by both reducing or increasing risk-taking, and that this effect is not consistent over time. Moreover, I examine the impact of such risk-taking behavior on borrower outcomes.

Broadly, my paper also relates to research investigating the impact of regulation on the incentives of bank managers. An extensive literature in accounting focuses on how regulatory capital influences managers' discretion in loan loss provisions and charge-offs (see [32, 33, 34, 35, 36]). In the context of U.S. stress tests, [37] find evidence suggesting that stress-tested banks manage financial performance and invest in political spending in order to improve their chances of passing stress tests, thus reducing the efficacy of stress tests over time.

1.2 Institutional background

1.2.1 General overview of stress tests in the United States

The financial crisis of 2008 led to significant regulatory changes that increased the supervision and oversight of large financial institutions in the U.S. One such oversight measure was the introduction of stress tests for large and systematically important financial institutions. Conducted by the Federal Reserve, stress tests are simulations designed to determine the ability of stressed banks to withstand a (hypothetical) scenario of prolonged macroeconomic distress.

The very first federally-administered stress testing effort was the Supervisory Capital Assessment Program (SCAP) in 2009 for BHCs with consolidated assets greater than $100 billion. Since 2011, stress tests have become a regular annual exercise under the Comprehensive Capital Analysis and Review (CCAR). For the 2011–2013 CCAR cycles, only BHCs that had consolidated assets greater than $100 billion underwent stress tests conducted by the Fed. Starting in 2014, the Fed began stress testing BHCs that had assets greater than $50 billion.[8] Moreover, starting in 2013, the Fed began implementing two concurrent versions of stress tests: one based on the CCAR framework, and the other based on compliance with the Dodd–Frank Act (DFAST). The main difference between CCAR and DFAST is that under the CCAR framework, BHCs' planned capital distributions are incorporated into the stressed-scenario projections; while under the DFAST framework, BHCs' future planned distributions have no effect on stressed-scenario projections. From 2012–2015, reports summarizing the results of the CCAR and DFAST exercises were released in March. The release date was moved to June in 2016.

In the U.S., the federal regulator provides banks with three scenarios: *baseline*, *adverse*, and

[8] The decision to reduce the asset size threshold was made in 2012. However, BHCs with assets between $50 billion and $100 billion were stress-tested by the Fed only after 2013.

severely adverse. Each of these scenarios simulates an increasingly hostile economic environment. These scenarios focus on aggregate risks that affect the entire economy (e.g., rising unemployment and falling house prices) rather than bank-specific idiosyncratic risks. The goal of these tests is to ensure that (a) banks remain adequately capitalized during a severe economic crisis and (b) maintain their capacity to provide credit. To this end, the Fed develops proprietary models that map the effect of hypothetical economic scenarios on banks' capital ratios over the duration of the forecast. The inputs of these models include data on individual bank capitalization, investments, and exposure to various loan markets. Thus, based on bank data at the outset of the test, the Fed uses its internally developed models and economic scenarios to determine whether banks pass or fail the stress test in any given year.[9]

1.2.2 Importance of stress test outcomes

The results of stress tests are of particular interest to regulators and bank managers, since poor performance or failure can force poorly performing BHCs to alter their planned capital distributions. The disclosure of stress test results are also of interest to investors, since the announcement date is associated with abnormal stock returns for the BHCs being tested ([38, 39, 40]). [41] document that stress test results convey both positive and negative news to investors, and price volatility and volume both increase around disclosure dates. Importantly, stress tests also appear to influence bank practices. According to both bank regulators and senior officers, banks have largely implemented heightened risk management techniques after the financial crisis as a direct response to increased regulation.[10]

[9] Note that the economic scenarios and underlying assumptions incorporated into the Fed's models are applied to all BHCs to a similar extent. Thus, the only difference comes from the asset and investment mix of the BHCs undergoing stress tests conducted by the Fed.

[10] Source: MIT Golub Center for Finance and Policy and GrantThornton (2017) survey report.

1.3 Identification challenges and empirical methodology

In order to examine the impact of stress tests on bank lending in the consumer credit card market, I must estimate a regression of the following general form:

$$CreditCardLending_{b,t} = \alpha + \beta StressTestExposure_{b,t-1} + \epsilon_{b,t} \tag{1.1}$$

where the subscripts *b* and *t* reference the bank and time, respectively.

However, estimating the above regression at the bank-level is problematic for at least two reasons. First, a given bank's credit card lending is the observed outcome of both supply and demand factors. Thus, I must disentangle changes in bank-level credit supply from consumer demand effects. Second, I must create a measure that quantifies the extent to which BHCs are exposed to the annual CCAR/DFAST exercise conducted by the Federal Reserve. In this section, I explain my empirical methodology to overcome such identification concerns.

1.3.1 Isolating changes in credit supply from changes in credit demand

The primary challenge pertains to disentangling demand and supply effects in the credit card market. My data offers a comparative advantage in this regard; namely, I observe data on both credit card limits and credit card balances. Credit card limits measure the amount that a lender is willing to lend to a consumer (i.e., limits measure the supply-side of credit to a consumer). On the other hand, credit card balances reflect a consumer's credit demand. Note that credit limits can also reflect consumer demand if the account holder requests an increase in limits. However, it is less likely that an account holder would request a reduction in credit limits on an open credit card. In addition, while account holders can request terminations of open credit cards, it is often unclear whether it is in their best interest to do so.[11]

[11] From the consumer's perspective, higher credit card limits are generally preferable even if these higher limits come from unused credit cards, since higher limits provide greater financial slack. Moreover, for a given level of total

Although credit limits are a cleaner measure of credit supply, this empirical approach could still be subject to potential endogeneity concerns if credit card issuers change credit limits in anticipation of changes in credit demand. My identification strategy allows for mitigating such concerns. I rely on the unpredictable nature of stress test results as a shock to banks. Ex-ante, banks are aware of their inclusion in a particular CCAR/DFAST cycle as well as the broad economic scenarios being modeled by the Federal Reserve. However, stress-tested BHCs are not privy to the proprietary models the Fed uses to project bank performance under these scenarios. As a result, stress-tested BHCs cannot perfectly predict their performance in the economic scenarios modeled by the Fed.

I use granular credit card account–level data to estimate the impact of stress test exposure on the credit limits extended by banks to their credit card borrowers. For identification, I construct my tests similar to [17] (see also [42, 43, 15]), where I estimate changes in account-level limits in the presence of *Individual* fixed effects. For any single stress testing cycle, the empirical specification I estimate is:

$$\Delta CreditLimit_{i,c,b} = \alpha + \beta StressTestExposure_b + f(\mathbf{X}_{i,c,b}) + \eta_i + \epsilon_{i,c,b}, \qquad (1.2)$$

where *i*, *c*, and *b* index individuals, credit cards, and banks, respectively. $\Delta CreditLimit_{i,c,b}$ is the change in credit limits for individual *i*'s credit card *c* with bank *b* in the window around the public release of results for the stress testing cycle under consideration.

I compute the change in credit limits for each credit card by first collapsing the time-series credit card–level data by averaging across time to obtain a single credit card–level cross-section separately in both the period before data collection (the *pre-collection* period) and the period after the results are released (the *post-release* period). The pre-collection and post-release windows are symmetric; each consists of two semiannual archives. Each archive is a snapshot of both the credit limit and

individual-level credit card balances, higher credit limits translate to lower utilization ratios. The combination of low utilization, a greater number of accounts, and more creditors indicates higher consumer creditworthiness, and thus it generally coincides with higher credit scores.

the current balance on each credit card at a given time in my sample. The pre-collection window is chosen such that it ends immediately before bank information is collected for a particular stress test cycle. Similarly, the post-release window is chosen such that it starts after the public release of stress test results. For example, for the 19 BHCs included in the 2012 CCAR, the Federal Reserve modeled forward-looking scenarios using bank data as of 2011Q3. The results of these stress test analyses were made public in March 2012. Thus, for the 2012 CCAR, the pre-collection (post-release) period window includes the semiannual archives for January 2011 and July 2011 (July 2012 and January 2013).

The variable $StressTestExposure_b$ measures the exposure of a bank to a particular stress test cycle. β measures the impact of a bank's stress test exposure on the credit limits it extends to its borrowers. η_i represents the vector of *Individual* fixed effects, which control for confounding individual-specific demand factors (e.g., income changes) that could bias my results. The inclusion of these fixed effects allows me to compare changes in limits across multiple cards issued to the *same* consumer, where the banks issuing these cards differ in their exposure to the stress test under consideration. Finally, $f(\mathbf{X}_{i,c,b})$ is a vector of control variables at the bank level, the consumer–bank level, and the account level, all of which are measured (and averaged) in the pre-collection period. The bank-level control variables control differences in credit card issuers that could confound my analysis, such as size, performance, and lending quality. I also account for bank-specific demand factors by controlling for the age of the credit card, card-level utilization, and the total number of credit-related accounts between the bank and the borrower.

An alternate approach to conducting the same analysis would be to use the time-series panel data on credit card accounts, and include a vector of *Individual* $\times$ *Archive* fixed effects. However, this is more challenging as it entails a large number of observations and fixed effects. Further, collapsing the time series and estimating cross-sectional regressions mitigates the econometric issues related to the underestimation of standard errors in panel data that have short time dimensions ([44, 45]). Thus, my approach provides conservative standard errors.

As described above, Equation (1.2) is useful in estimating how banks respond to a single stress testing cycle. In particular, it allows me to study whether banks respond differently to stress tests over time by implementing the specification separately for each testing cycle. However, in order to estimate the average effect across all testing cycles, I estimate the following specification, which pools together multiple stress testing cycles:

$$\Delta CreditLimit_{i,c,b,t} = \alpha + \beta StressTestExposure_{b,t} + f(\mathbf{X}_{i,c,b,t}) + \eta_{i,t} + \epsilon_{i,c,b,t}, \tag{1.3}$$

where *i*, *c*, *b*, and *t* index individuals, credit cards, banks, and the CCAR/DFAST exercise conducted in year *t*, respectively. $\eta_{i,t}$ represents a vector of *Individual* $\times$ *Stress Test Cycle* fixed effects, which allows me to compare changes in limits across multiple cards issued to the same consumer in a particular testing cycle. All other variables are defined as in Equation (1.2).

Importantly, this pooled specification allows me to address whether stress tests have an effect on bank risk-taking in the credit card market. To identify how stress test exposure affects the *composition of bank credit supply*, I estimate the following regression specification:

$$\begin{aligned} \Delta CreditLimit_{i,c,b,t} = \alpha + \beta StressTestExposure_{b,t} \times NonPrime_{i,t} \\ + f(\mathbf{X}_{i,c,b,t}) + \zeta_{b,t} + \eta_{i,t} + \epsilon_{i,c,b,t} \end{aligned} \tag{1.4}$$

where $NonPrime_{i,t}$ is a dummy variable that equals 1 for consumers with credit scores under 680 at the outset of a given stress test cycle, and 0 otherwise. $\zeta_{b,t}$ is a vector of bank-by-time fixed effects, which helps capture general bank-level credit supply trends across time. As described earlier, $\eta_{i,t}$ captures individual-specific demand factors in a given stress test cycle. Including both $\zeta_{b,t}$ and $\eta_{i,t}$ in the same specification thus helps suppress concurrent supply and demand factors at the bank and consumer level, respectively. The remaining variation is therefore pertinent to the bank–consumer matching process, and it is helpful in identifying compositional changes in the supply of credit at the bank–consumer level.

One implicit assumption behind this *within*-individual implementation is that consumers have a general demand for credit and are indifferent across issuers. In reality, bank-specific consumer demand can still confound my analysis, especially if consumers are offered incentives to use certain credit cards to make specific purchases. For instance, consumers may be rewarded in the form of cash-back or points for using cards issued by certain issuers towards the purchase of specific goods (e.g., gasoline). In such cases, changes in the demand for these goods could drive changes in credit limits. I address this concern in Section 1.5.2 by constructing a "leave-out" mean credit limit for each bank–consumer pair. This measure captures the average credit limits extended by the bank across all credit cards after excluding the bank's credit limit extended to that consumer.

1.3.2 Identifying exposure to stress tests

A secondary challenge is quantifying the extent to which bank holding companies (BHCs) are exposed to the annual CCAR/DFAST exercise conducted by the Federal Reserve. I avoid potentially problematic comparisons between stress-tested and non-tested banks by restricting my analysis to the sample of stress-tested banks. For each bank undergoing the CCAR in a given cycle, I compute the measure of stress test exposure described in [16] using annual stress test results publicly disclosed by the Fed. These results disclose the resiliency of BHC capital to hypothetical, severely adverse economic scenarios over the following nine quarters. Specifically, these reports contain information on the minimum implied Tier 1 capital ratio, total risk-based capital ratio, and Tier 1 leverage ratio under stressed conditions.

BHCs with larger declines in modeled forward-looking capital ratios relative to their current capital ratios are expected to experience larger declines in equity capital, and are thus more likely to face interference from regulatory authorities. Regulatory interference can take the form of restrictions in planned capital distributions as well as increased pressure on poorly performing BHCs to reduce risks in their portfolio of assets.

For a given testing cycle, the stress test exposure measure is thus defined as:

$$\Delta CapRatio_{BHC} = CapRatio_{BHC,Outset} - CapRatio_{BHC,Severe}, \tag{1.5}$$

where $CapRatio_{BHC,Outset}$ is the BHC's starting capital ratio at the beginning of the time horizon covered by that particular testing cycle, and $CapRatio_{BHC,Severe}$ is the lowest implied capital over the nine-quarter forward-looking horizon under the severely adverse stress scenario. Consistent with [16], the exposure measure reflects only *quantitative* changes in BHC portfolios due to the stress test scenarios and does not capture *qualitative* changes associated with the effect of planned capital distributions. Thus, while the sample contains only CCAR banks, the exposure measure is computed using disclosures under the Dodd–Frank Act, which do not incorporate a bank's capital distribution plan. BHCs with high values of this exposure measure in a given stress test cycle have portfolios with the greatest downside risk in the severely adverse economic scenario described in that particular cycle.

My baseline measure of stress test exposure is computed as the change in the Tier 1 capital ratio of stress-tested banks as identified by the Dodd–Frank annual disclosures. However, in additional robustness tests, I also define stress test exposure in terms of the total risk-based capital ratio and the Tier 1 leverage ratio (both measures are constructed using data from DFAST disclosures). Moreover, I also compute declines in bank capitalization through CCAR disclosures, which account for the capital distribution plans of stress-tested banks. Lastly, I use the strict BHC size threshold considered for CCAR inclusion, and I compare credit card lending by BHCs that fall on either side of this threshold in a difference-in-differences setting. The size threshold was \$100 billion for stress tests conducted before 2014, and the threshold has been \$50 billion since the 2014 CCAR/DFAST. Thus, for stress tests conducted before (since) 2014, I choose the control group of non-tested banks that have consolidated assets in the \$50 billion to \$100 billion (\$10 billion to \$50 billion) range.[12] My

[12]This approach is popular among recent papers that study the lending implications of stress tests, such as [25, 24, 26, 22], among many others. However, a significant concern with this method of comparing stress-tested banks to non-

results and inferences remain unaffected by these different definitions of stress test exposure.

1.4 Data and summary statistics

1.4.1 Data sources

The majority of my analysis focuses only on banks that are included in the annual CCAR/DFA stress tests conducted by the Federal Reserve from 2012 through 2016. Given that my analysis focuses on studying banks' lending decisions, I am interested only in the quantitative component of stress tests, which does not capture the effect of banks' capital distribution plans. Thus, I collect data on banks' stress test performance from the annual DFAST disclosure. Banks' performance is measured through the evolution of their Tier 1 capital ratio, their total risk-based capital ratio, and their Tier 1 leverage ratio under the *baseline*, *adverse*, and *severely adverse* scenarios. Similar to [16], I define *stress test exposure* as a bank's capital ratio at the outset of any particular stress test cycle minus the lowest implied ratio under the severely adverse economic scenario.

I gather credit card–level data from one of the three major credit bureaus in the U.S. All the data described below are used purely for academic purposes, and they contain anonymized information. The credit bureau's data provide comprehensive records of the various credit accounts opened by every U.S. resident. These accounts span credit cards, mortgages, auto, and student loans, and several other trade types.

Of the 35 bank holding companies (BHCs) that have undergone stress tests performed by the Federal Reserve between 2009 and 2016, I identify 23 banks that both issue credit cards and report to the credit bureau. These 23 BHCs account for 90% of the market in terms of open credit cards. Importantly, the dataset covers eight of the top 10 issuers, which comprise 75% of the market, and it accounts for all six of the largest issuers.

tested banks is the implicit assumption that the only difference between the two groups of banks is asset size. However, stress-tested and non-tested banks differ in their investment activities, the products and services they offer, and their systemic importance.

As described earlier, for the 2011–2015 CCAR/DFAST stress test cycles, the Federal Reserve modeled forward-looking BHC capital ratios using current BHC data as of Q3 of the previous year, and it publicly announced the results of the stress tests in March of that year.[13] Starting from the 2016 CCAR/DFAST, the Federal Reserve began modeling BHC performance using current data as of Q4 of the previous year, and they began to release results in June. Thus, for each stress test cycle, I identify all individuals who have active credit card accounts issued by at least one of the stress-tested issuers in that particular cycle at the time the issuer submits data to Federal Reserve. I then obtain information on their credit card limits and balances.

I limit my analysis to credit cards that have at least one non-missing observation before bank data submission to the Federal Reserve (pre-collection period) and one non-missing observation after the public announcement of stress test results (post-release period). I winsorize limits at the 1% and 99% levels. Finally, I average the credit limits on credit cards separately in the pre- and post- periods to capture changes in the average credit supply on individual credit cards after the announcement of stress test results.

The credit card–level analysis is conducted with individual fixed effects, and thus relies on comparing *within*-individual changes in credit card limits. Therefore, my analysis includes only individuals who have two or more credit cards issued by stress-tested banks in a particular stress test cycle, where the issuers differ in their exposure to the stress tests. Thus, my sample contains 10.1 million credit cards issued to 3.27 million individuals across five stress test cycles.

1.4.2 Descriptive statistics

I present bank-level summary statistics in Panel A of Table 1.1. I find that the average implied decline in the Tier 1 ratios of stress-tested banks is approximately 3.6 percentage points (pp). Similarly, the average implied decline in the total risk-based capital ratio (Tier 1 leverage ratio) is ap-

[13] For example, for the 2012 CCAR, the Federal Reserve modeled BHC performance using data from 2011Q3 and publicly released stress test results in March 2012.

proximately 3.8 (2.7) pp. These implied declines are large, and they correspond to a 25–35% change in capital for banks in my sample. Moreover, there is substantial variation in performance across banks, with a standard deviation ranging from 1.2–2.0 percentage points. Since my analysis is restricted to stress-tested banks, I cover a set of large banks, with average assets greater than $260 billion. The other bank financial variables are largely in line with the literature.

In Panel B, I present descriptive statistics of credit bureau data. Since my analysis is restricted to individuals with multiple credit cards issued by different stress-tested institutions, I find that my sample contains high–credit quality borrowers. The average credit score of borrowers in my sample is approximately 745. However, there is some variation in borrower creditworthiness, with a standard deviation of approximately 80 points in credit scores. Their average monthly income is approximately $4,330. In addition, their utilization ratio (approximately 27%) is consistent with the national average.

Lastly, I also provide descriptive statistics at the credit card level. The average credit card in my sample has a limit of approximately $9,850. However, there is substantial variation in issued limits, with a standard deviation of approximately $8,600. The average utilization of credit cards in my sample is 23.5%. However, the median is only approximately 4%, which suggests that balances are not carried over among a large portion of the credit cards in my sample.

Table 1.1: Summary statistics

This table reports summary statistics at the credit card–issuing bank level. Panel A uses quarterly BHC Y-9C regulatory filings data in the pre–stress test period for each stress test cycle. The pre- period for each testing cycle consists of the four quarters before the outset of that particular stress test cycle. The outset period for the 2012–2015 stress test cycles is Q3 of the preceding year. The outset period for the 2016 stress test cycle is 2015Q4. Panel B uses credit bureau data in the pre–stress test period, defined analogously to Panel A. These data are first collapsed to obtain a single bank-level cross-section in the pre–stress test period by averaging across time. Descriptive statistics are then reported for these time-averaged values. The variables in Panel A are reported as a fraction of total assets unless specified otherwise. The table reports means, medians, and standard deviations.

Panel A: Bank characteristics

	Mean	Median	Standard Deviation
Stress test exposure measures			
Stress test exposure #1 (Tier 1 capital ratio)	3.551	3.300	1.844
Stress test exposure #2 (Total risk-based capital ratio)	3.780	3.600	1.974
Stress test exposure #3 (Tier 1 leverage ratio)	2.722	2.700	1.233
Other			
Assets (log)	19.392	18.989	1.166
Equity capital ratio	0.121	0.120	0.018
Liquid assets	0.259	0.236	0.085
Business mix			
Mortgage loans	0.155	0.165	0.072
C&I loans	0.133	0.129	0.063
Consumer loans	0.139	0.092	0.158
Credit card loans	0.073	0.012	0.143
Performance			
ROE	0.053	0.048	0.038
Non-performing loans	0.011	0.007	0.010
Risk-based capital ratio	0.154	0.151	0.022

Panel B: Credit bureau data

	Mean	Median	Standard Deviation
Borrower creditworthiness			
Credit score	745	769	79
Monthly income ($)	4,329.81	3,917.00	1,751.25
Debt-to-income ratio	28.292	24.000	25.175
Borrower debt fundamentals			
Credit card utilization	27.030	10.710	32.414
Credit card balance ($)	6,118.57	2,383.00	9,893.68
Mortgage balance ($)	210,015.90	155,526.00	220,232.77
Auto balance ($)	18,131.10	14,480.00	18,963.08
Credit card characteristics			
Limits ($)	9,842.52	8,000.00	8,572.72
Balances ($)	1,740.79	285.29	3,351.31
Utilization (%)	23.507	3.930	33.060

1.5 Main findings: Trade-level analysis

1.5.1 Impact of stress tests on bank risk-taking in the credit card market

In this section, I use granular credit card–level data to estimate the impact of stress test results on a bank's credit supply, and I study any compositional effects. As described in Section 1.3.1, I isolate changes in credit limits at the account level using consumer fixed effects in each stress test cycle.

I begin my analysis by grouping consumers into 20-point credit score bins separately for each stress testing cycle. The riskiest (least risky) credit score bin covers the credit score range of 560–580 (820–840). Next, I study the relative lending response of high-exposure banks by running the single-cycle regression specification in Equation (1.2) for each of these bins separately within each stress test cycle. Doing so allows me to examine whether the lending response of high-exposure banks changes across time to consumers of varying creditworthiness.

The main independent variable of interest equals the starting value of the (risk-weighted) Tier 1 ratio at the outset of any given cycle minus the lowest Tier 1 ratio implied by the severely adverse stressed scenario implemented for that cycle. This measure of stress test exposure has been standardized for ease of interpretation. Moreover, all bank-level control variables are constructed by averaging over the four quarters immediately preceding the outset of any stress test cycle. The dependent variable is credit card limit growth at the account level. In order to mitigate the effect of large outliers, I normalize the change in card-level limits by the midpoint between the pre-collection and post-release periods for each stress test cycle as follows:

$$\Delta CC\ Limit_{i,c,b} = \frac{CC\ Limit_{i,c,b,post} - CC\ Limit_{i,c,b,pre}}{\frac{1}{2} \times (CC\ Limit_{i,c,b,post} + CC\ Limit_{i,c,b,pre})},$$

where *i*, *c*, and *b* reference individuals, credit cards, and banks, respectively. The results of this analysis are presented in the form of bar graphs in Figure 1.1.

For the 2012 stress test cycle, I find that high-exposure banks respond to stress tests by aggressively cutting credit card limits across all credit score bins. The cuts in limits appear to be slightly larger for the riskier segment of the consumer base, but only marginally. For the 2013 CCAR/DFAST, I find that high-exposure banks cut credit card lending, but only for the prime segment of consumers; the non-prime segment experienced no differential decline in credit limits on cards issued by high-exposure banks. Starting from the 2014 cycle, I observe two opposing patterns. First, consistent with previous CCAR/DFAST exercises, I find that high-exposure banks cut credit limits on cards issued to prime consumers. At the same time, however, I find that high-exposure banks appear to *increase* credit supply to the non-prime consumer segment. Importantly, for any given non-prime credit score bin, I find that high-exposure banks supply higher credit limits with each passing stress test cycle.

Figure 1.1: Impact of stress tests on bank credit card lending across different cycles and consumer segments

This figure examines how banks have responded to stress tests over time. It documents how the coefficient β associated with the variable of interest, *StressTestExposure*, from Equation (1.2) has varied across different stress test cycles for different consumer segments. For each stress test cycle, consumers who have multiple credit cards issued by stress-tested banks are grouped into 20-point credit score bins ranging from 560 through 840. Next, the regression specification in Equation (1.2) is run for each of these credit score bins within each stress test cycle. The associated point estimate on the key independent variable, *StressTestExposure*, is presented in the form of bar graphs across different stress test cycles.

Thus, taken together, the findings in Figure 1.1 suggest that, over time, the stress testing exercises have become less effective in reducing bank risk-taking. Indeed, the earlier rounds of stress tests encouraged banks to reduce limits to the non-prime segment, but in subsequent cycles, the rate of growth in the credit supply extended to the non-prime segment by high-exposure banks has steadily increased with each passing cycle. In this sense, my findings are more consistent with [46], whose findings suggest that stress tests have become less informative over time.

Next, I test for evidence consistent with bank risk-taking in the pooled sample of all CCAR/DFAST exercises conducted from 2012 to 2016. The main advantage of the pooled sample is that it allows me to include vectors of both *Bank* × *Stress Test Cycle* and *Individual* × *Stress Test Cycle* fixed effects, which suppress concurrent bank credit supply and individual demand factors. Thus, the only remaining variation pertains to the bank–consumer matching process, which allows for cleaner identification of bank risk-taking behavior. The pooled sample consists of approximately 10.1 million credit cards issued to approximately 3.3 million individuals across five stress test cycles. The results of this analysis are presented in Table 1.2.

In Column (1), I report the results of the OLS specification without any fixed effects and control variables. The results suggest that banks with a one standard deviation higher exposure to stress tests respond by reducing credit card limits, although the result is only marginally significant at the 10% level. In Column (2), I include a vector of *Individual* × *Stress Test Cycle* fixed effects, which helps capture variation across credit cards issued to the *same* consumer in a given stress test cycle in which the banks issuing these cards differ in their exposure to the stress tests. The coefficient of interest remains negative, but it is now significant at the 5% level and largely unaffected in terms of economic magnitude. This implies that individual demand factors appear to be largely uncorrelated with the stress test exposure measure.

In Column (3), I interact the stress test exposure measure with an individual-level dummy variable that equals 1 for non-prime consumers (i.e., consumers with credit scores under 680) and 0 for prime consumers. I find that the base effect is negative and significant, while the coefficient on the

Table 1.2: How do banks adjust credit card supply in response to stress tests?

This table shows the relation between stress test exposure and the change in credit card limits using credit bureau data. Each pre-collection and post-results period for any given stress test cycle consists of two semiannual archives. Credit card–level data are first collapsed to obtain a single credit card–level cross section separately in the pre– and post–stress test periods by averaging across time. Then, the dependent variable is constructed as the growth in limits at the credit card–level from the pre– to the post–stress test period. $Exposure$ is computed as the difference between the starting value of banks' Tier 1 ratio at the outset of the stress test and the lowest capital ratio implied by the severely adverse stress scenario. *Non-Prime* is an indicator variable that equals 1 if an individual's credit score is below 680 at the outset of a given stress test cycle, and 0 otherwise. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: Δ CC Limit	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-4.713*	-5.382**	-5.893**	-4.676**	-4.925***	
	(-1.72)	(-2.45)	(-2.50)	(-2.12)	(-3.68)	
Exposure × Non-prime			3.018***	3.010***	3.868***	3.617***
			(2.75)	(2.79)	(4.43)	(4.19)
Consumer × ST Cycle FE		✓	✓	✓	✓	✓
Bank × ST Cycle FE						✓
Observations	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409
Adj. R^2	0.005	0.169	0.169	0.181	0.193	0.213
Trade-level controls				✓	✓	✓
Bank-level controls					✓	

interaction term is positive and significant at the 1% level. Thus, the result in Column (3) suggests that banks with more stress test exposure respond by cutting credit card limits, but these cuts are concentrated in the prime consumer segment. The results continue to hold when accounting for trade-level controls (Column (4)) and both trade-level and bank-level controls (Column (5)). The results presented in Column (5) are economically meaningful, and they suggest that banks with a one standard deviation larger exposure to stress tests respond by cutting limits by approximately \$418, and this decline is driven largely by reducing the credit supply extended to prime consumers.

Finally, in Column (6), I report results for the full specification that includes both bank-by-time and individual-by-time fixed effects. Including both sets of fixed effects helps account for both bank credit supply and individual demand factors. As a result, I can only estimate the coefficient on the interaction term between the stress test exposure measure and the non-prime dummy indicator. I find that this coefficient is unaffected in both economic and statistical magnitudes by the fully saturated specification. The takeaway remains that higher stress test exposure induces banks to shift the composition of its credit supply towards a riskier segment of consumers.

My baseline analysis described above focuses on the five CCAR/DFAST exercises conducted between 2012 and 2016. I now examine how banks responded to the very first stress test conducted in the post-crisis period, the 2009 Supervisory Capital Assessment Program (SCAP). Given the significant differences in the implementation details between the 2009 SCAP and the subsequent CCAR/DFAST exercises, I do not include the SCAP testing cycle in my pooled analysis. Instead, I separately examine the impact of the SCAP on banks' risk-taking, and report my findings in Table 1.3.

Table 1.3: Impact of 2009 SCAP on bank risk-taking

This table examines whether stress tests conducted in the pre–CCAR/DFAST era induced banks to reduce risky credit card lending. The pre–CCAR/DFAST stress test considered is the 2009 Supervisory Capital Assessment Program (SCAP). In Columns (1) and (2), $Failed$ is a dummy indicator equal to 1 for banks that failed the 2009 SCAP, and 0 for banks that passed the SCAP. In Columns (3) and (4), *SCAP Buffer* is the amount of capital that SCAP–tested banks were required to raise scaled by their 2008Q4 risk-weighted assets. *Non-Prime* is an indicator variable that equals 1 if an individual's credit score is below 680, and 0 otherwise. The standard errors are clustered at the bank level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: Δ CC Limit	(1)	(2)	(3)	(4)
Failed	-9.384***			
	(-14.44)			
Failed × Non-prime	-5.085***	-5.119***		
	(-3.88)	(-3.92)		
SCAP Buffer			-2.775***	
			(-5.35)	
SCAP Buffer × Non-prime			-2.136***	-2.135***
			(-5.01)	(-5.00)
Consumer FE	✓	✓	✓	✓
Bank FE		✓		✓
Observations	2,195,458	2,195,458	2,195,458	2,195,458
Adj. R^2	0.067	0.067	0.067	0.067

In Columns (1) and (2), I compare credit card lending by banks that failed the 2009 SCAP to that of passing banks. I continue to capture within-consumer changes in lending through the inclusion of consumer fixed effects. The results indicate that failing banks reduced credit card limits in reponse to the 2009 SCAP. Importantly, however, these cuts in limits were significantly *larger* for the constrained, risky, non-prime segment of the population. In Columns (3) and (4), I replace the binary passed/failed indicator with the continuous SCAP capital buffer measure. This measure is defined as the amount of equity capital the SCAP–failing bank was required to raise scaled by its risk-weighted assets as of 2008Q4. Even using this continuous measure, I find that in response to the 2009 SCAP, banks with greater exposure cut limits on credit cards by a larger amount for observably risky borrowers.

Taken together, my findings suggest that more-exposed banks responded to the 2009 SCAP by reducing risk-taking, but progressively increased risk-taking in the subsequent CCAR/DFAST exercises. The sharp reduction in risky lending for the 2009 testing cycle can be attributed to the "surprise" aspect of the SCAP, which was announced by the Federal Reserve in February 2009 and implemented in the following month. Given the novelty of the stress-testing regime at the time, more-exposed banks perhaps sharply reduced risky lending to ease capital requirements. Moreover, banks were included in the SCAP based on ex-ante 2008:Q4 asset size, which mitigated concerns of self-selection into or out of the test. In contrast, banks are more informed about the timelines of the CCAR/DFAST process. Probably more importantly, the timing of SCAP was more crucial, since incentives to take risks were lower in the immediate aftermath of the crisis.

1.5.2 Robustness checks

Are these results driven by bank-specific consumer demand?

One concern with the regression specification in Equation (1.3) is that I implicitly assume that individuals with multiple credit cards have no differential demand across their cards. However, it is possible that consumers may prefer certain credit cards over others. Thus, such confounding demand factors at the credit card level could still bias my results, which account only for endogenous demand factors at the individual level.

To mitigate such card-specific endogeneity concerns, I construct a "leave-out mean" credit limit change for each credit card. For every individual *i*'s credit card c issued by bank b, I compute $CreditLimit_{i,-c,b}$ as the average credit limit using all the credit cards issued by bank b except credit card c. Next, I compute the change in this "leave-out mean" credit limit for each credit card. By construction, this measure excludes the credit limit changes made by a bank due to individual demand factors such as individual requests for increases in credit limits. Moreover, this measure continues to capture a bank's average change in credit supply through its credit cards.

The results of this analysis are presented in Table A.1. In Column (1), I present the baseline result. I continue to find that high-exposure banks cut credit limits on credit cards. In Column (2), I interact the stress test exposure measure with an indicator variable that equals 1 for non-prime consumers and 0 otherwise. I continue to find that the high-exposure banks cut credit supply to the unconstrained, prime consumer segment. These findings are not altered by the inclusion of *Bank* $\times$ *Stress Test Cycle* fixed effects (Column (3)). This further suggests that bank-specific individual demand factors are less likely to influence my results, and individual fixed effects (within each stress test cycle) adequately control for confounding demand-related factors.

Are these results driven by particular banks?

I show that my results are not driven by any particular bank in my sample. I re-estimate the specification in Table 1.2, Column (6), by excluding one bank from the analysis each time and estimating the regression on the sample consisting of the remaining 22 banks. Consequently, there are 23 such regressions, and I plot the 23 estimated coefficients on $Exposure \times NonPrime$ (along with their standard errors) in Figure A.1. As can be seen, across all specifications, the estimated interaction term coefficients are all positive and largely stable, which suggests that no single bank is driving my finding that banks cut safe, and not risky, lending in response to stress tests.

Alternative measures of consumer constraints

In Table A.2, I consider alternate measures of riskiness. In Panel A, I explore the cross-sectional cuts across credit cards that have different utilization ratios. I group the credit cards in my sample into three groups based on their utilization at the outset of a particular stress test cycle: *low* ($\leq 50\%$), *high* (50–90%), and *very high* ($> 90\%$). The results suggest that banks with higher exposure to stress tests reduce credit limits to low–utilization ratio credit cards (i.e., low-risk credit cards). In contrast, there appears to be little to no cuts in limits to high- and very high–utilization ratio credit cards. A similar pattern emerges when I perform cross-sectional cuts based on the consumer's total utilization ratio, which is defined as the ratio of the individual's total credit balance to the individual's total credit limit across all her credit cards (Panel B). Lastly, my inferences remain unchanged when I proxy consumer constraints through the debt-to-income ratio (Panel C).

Alternative measures of stress test exposure

In Table A.3, I document the robustness of my results to alternative measures of bank stress test exposure. In Panel A (Panel B), I define stress test exposure as the difference between the starting value of the bank's total risk-based capital ratio (Tier 1 leverage ratio) at the outset of the test and the lowest equivalent ratio implied in the severely adverse stress scenario under DFAST guidelines. Both measures are reported in the annual CCAR/DFAST disclosures. My results are robust to these alternative definitions. In Panel C, I define stress test exposure in terms of the Tier 1 risk-based capital ratio under CCAR guidelines, and find that my inferences remain unchanged.

Lastly, in Panel D, I report results that compare stress-tested issuers to non–stress-tested issuers. While the earlier tests focus on individuals with multiple cards from stress-tested issuers, this robustness check focuses on individuals that have at least one card from a stressed issuer and one card from a non-stressed issuer in each cycle. Since the 2012–2013 (2014–2016) CCAR stress test cycles included banks with consolidated assets over \$100 billion (\$50 billion), the control group for these cycles consists of banks with assets in the \$50 billion to \$100 billion (\$10 billion to \$50 billion) range. Thus, for each stress test cycle, the treated group consists of banks being stress-tested in that cycle, while the control group consists of banks that fall just under the asset-size threshold used to demarcate inclusion in stress tests by the Federal Reserve. Even for this test, I find that relative to non-stressed issuers, stressed issuers cut credit card limits mainly for prime consumers, consistent with my baseline analysis.

Accounting for bank-level cross-sectional dependencies

In Table A.4, I document the robustness of my baseline results in Table 1.2 to a more stringent clustering technique. In this table, I cluster standard errors at the bank level. The bank-level cluster is larger, and it can completely account for any within-bank correlations; however, the size distribution of this clustering level is heavily skewed owing to the highly concentrated nature of the credit card industry. Moreover, my sample consists of only 23 banks, and reported standard errors could be biased with too few clusters ([45, 47]). Regardless, I find that the statistical significance of my results is unaffected by the level of clustering.

Additional tests

Thus far, my findings capture changes in credit limits along both the intensive and extensive margin, and I examine whether the results hold separately along both these margins. The results of this analysis are presented in Table A.5.

In Panel A Table A.5, I focus on credit limit changes that occur along the intensive margin – i.e., I focus on limit changes in cards that remain open up to one year after stress test results are disclosed. Importantly, intensive margin changes in limits are arguably more effective in isolating demand and supply factors, since while consumers can possibly terminate credit cards (i.e., through a decrease in demand), they are much less likely to request limit decreases on open cards.[14] I run the

[14]Note that it is not always in the best interests of consumers to request credit card limit cuts or even credit card

specification in Equation (1.3) along the intensive margin, and I find that high-exposure banks tilt the supply of credit card limits towards non-prime consumers, consistent with my baseline results. In Panel B, I report results for extensive margin closures. I find that highly exposed banks close credit cards issued to prime consumers but not for non-prime consumers.

1.5.3 What mediates the transition from risk-reduction to risk-seeking in response to stress tests?

The results presented in the previous section document that banks with larger exposure to stress tests have increasingly issued higher limits to ex-ante risky consumers. Given my *within*-consumer empirical design, my identification comes from changes in credit card lending to the *same* consumer by stress-tested banks that differ in their stress test exposure. As a result, the *relative* sorting of different banks that lend to the same consumer is of utmost importance. However, in the empirical analysis conducted thus far, this relative sorting can differ across cycles given the construction of the stress test exposure measure. Moreover, the number of stress-tested banks has also increased over time. Thus, it is not possible to categorically attribute the documented findings to any economic mechanism.

In this section, I attempt to identify the channel that mediates the transition of stress-tested banks from risk-reduction to risk-seeking behavior in response to stress tests. To do so, I first restrict the analysis to only the 13 credit card issuers that have been included in each of the five CCAR/DFAST cycles from 2012 to 2016. Next, I apply the stress test exposure measure constructed for the 2012 CCAR cycle to all subsequent cycles. These two steps help in ensuring that my baseline results are not driven by the addition of new banks to the CCAR/DFAST process and changes in the relative sorting of banks across time, respectively.

terminations. From the consumer's perspective, higher credit card limits are generally favorable, even if they come from unused credit cards, since higher limits provide greater financial slack. Moreover, having more credit cards, along with relatively low utilization, indicates high creditworthiness, which is reflected through higher credit scores.

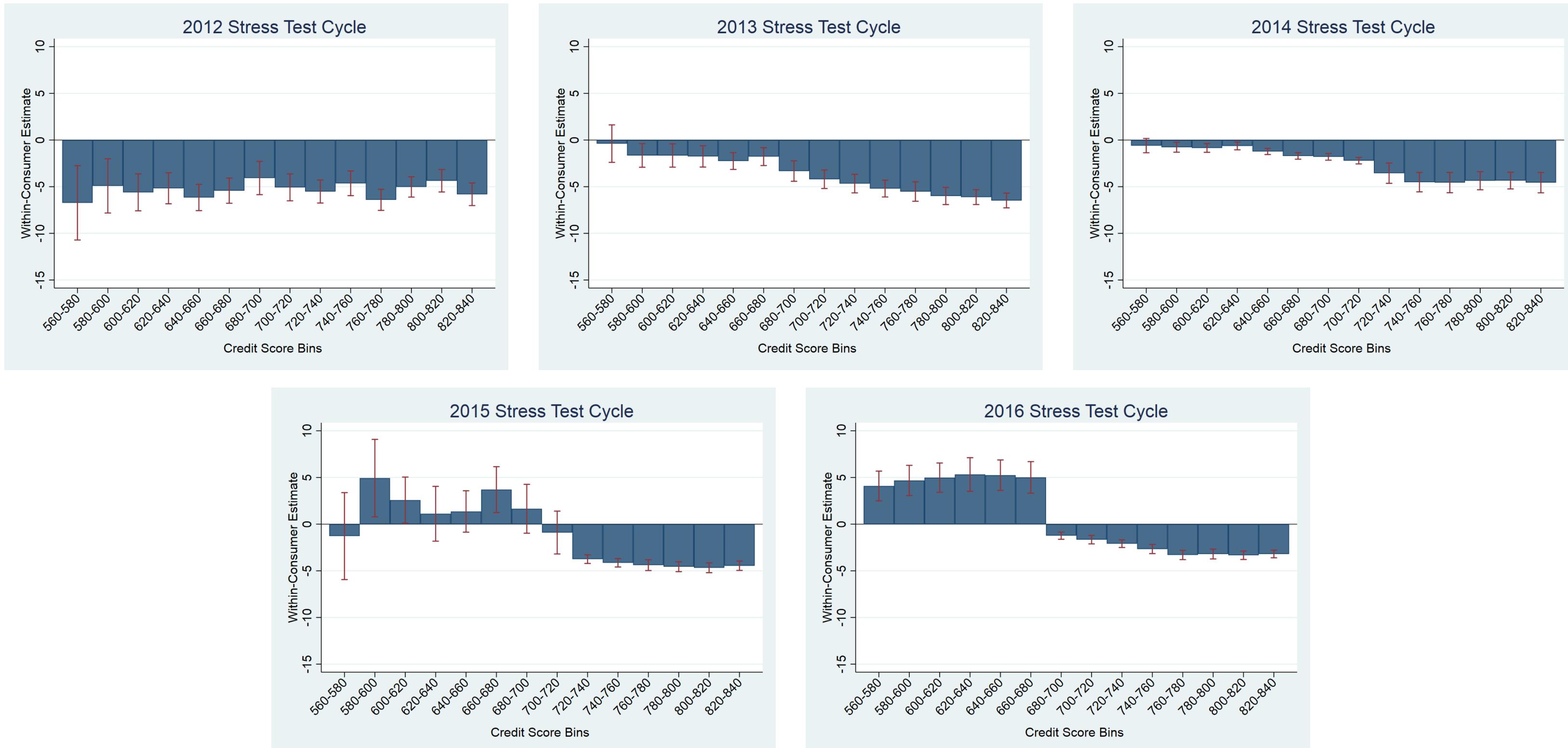


Figure 1.2: Are banks adapting to stress tests over time?

This figure examines whether banks are adapting to stress tests over time, consistent with a learning mechanism. It documents how the coefficient β associated with the variable of interest, *StressTestExposure*, from Equation (1.2) has varied across different stress test cycles for different consumer segments. For each stress test cycle, consumers who have multiple credit cards issued by stress-tested banks are grouped into 20-point credit score bins ranging from 560 through 840. Next, the regression specification in Equation (1.2) is run for each credit score bin within each stress test cycle. The associated point estimates on the key independent variable, *StressTestExposure*, are presented in the form of bar graphs across different stress test cycles. The key difference from Figure (1.1) is that the analysis is restricted to the 13 credit card issuers that have undergone each CCAR/DFAST exercise conducted by the Federal Reserve between 2012 and 2016. Moreover, banks' exposure to the 2012 CCAR exercise to all subsequent cycles. These two steps help ensure that the baseline results are not driven by the addition of new banks to the CCAR/DFAST exercises or by changes in the relative sorting of banks across time, respectively.

Next, similar to earlier tests, I first group consumers into 20 point credit score bins for each stress test cycle. I then study the respective credit card limit changes in response to stress tests for each of these bins within each stress test cycle. The results of this analysis are presented in Figure 1.2. I find that this pattern of results largely follows the results presented in Figure 1.1 — i.e., over time, high-exposure banks have transitioned from reducing risky lending to searching-for-yield in the non-prime credit card market.

For the above test, it is important to note that, by construction, the relative sorting of banks is held constant across cycles. Thus, I effectively document that the same high-exposure bank that reduced credit card limits to risky consumers in 2012 issues higher limits to the risky segment in 2016. Moreover, this transition appears to be gradual, with more risky lending over time. Thus, these findings appear to be consistent with banks adjusting to stress tests over time.

1.5.4 What is the impact on credit card balances?

In this section, I examine how the stress test–induced change in the composition of the supply of credit card limits affects card-level balances. Prior theoretical work provides mixed suggestions with regard to the possible relationship between changes in credit card limits and changes in credit card balances. Under the permanent income hypothesis, an increase in credit limits should not affect credit balances if permanent income remains unchanged. However, if liquidity constraints are binding currently, or are expected to be binding in the future, then credit limit changes can lead to changes in credit balances and consumption.

Elasticity of credit card balances with respect to credit card limits

In Table 1.4, I examine how log-changes in credit card limits affect log-changes in credit card balances. Panel A reports results for the full sample of credit cards issued to all consumer segments. Only credit cards with at least one nonmissing pre-collection period observation and one nonmissing post-release period observation are retained for analysis. In Column (1), I estimate the OLS regression to obtain the relation between credit limit changes and credit balance changes at the credit card level. I find that a 1% change in limits leads to a 0.48% change in balances. It is important to note that Column (1) does not include *Individual*$\times$*ST Cycle* fixed effects, and I thus capture cross-sectional variation across individuals and across stress test cycles.

However, endogenous demand factors can bias the estimate in Column (1) because consumer demand is the primary driver of changes in credit card balances. For example, it is possible that consumers request increased limits with the intention of increasing utilization in the future. It is also possible that banks can change limits in response to anticipated (future) changes in demand. Thus, in Column (2), I re-estimate the specification in Column (1), but in the presence of *Individual*$\times$*ST Cycle* fixed effects. The fixed effects estimator suggests that a 1% change in card-level limits leads to a 0.60% change in card-level balances. This positive relationship between credit limit changes and balance changes is consistent with [48].

In Column (3) (Column (4)), I implement the OLS (fixed effects) specification to estimate the impact of stress test exposure on card-level balances. Both columns suggest that banks with higher stress test exposure experience declines in credit card balances (relative to cards issued by banks with lower stress test exposure). Comparing the estimates in Columns (3) and (4) reveals that the individual demand factors are positively correlated with stress test exposure, resulting in a positively biased coefficient in Column (3). This suggests that omitting demand factors works against finding a negative relationship between the exposure of a given bank to stress tests and the relative reduction in the balances on credit cards issued by said bank.

In Column (5), I estimate a 2SLS specification where I regress the change in card-level balances on the change in card-level limits, where the latter is instrumented with the stress test exposure measure. This estimate, which captures the local average treatment effect (LATE), shows that a 1% change in credit limits due to stress test exposure changes credit card balances by approximately 1.5%.

Table 1.4: Impact of stress tests on credit card balances

This table shows the relation between stress test exposure and changes in credit card balances at the account level. Each pre-collection and post-results period for any given stress test cycle consists of two semiannual archives. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately in the pre– and post–stress test periods by averaging across time. Then, the dependent variable is constructed as the growth at the credit card–level from the pre– to the post–stress test period. $Exposure$ is computed as the difference between the starting value of banks' Tier 1 ratio at the outset of the stress test and the lowest capital ratio implied by the severely adverse stress scenario. Panel A reports results for the full sample. In Column (5), Δ*CC Limit* is instrumented by the *Exposure* variable. In Panel B, elasticities are reported across consumer segments, with Sub-Panels B.1, B.2, and B.3 reporting results for the full sample, the prime consumer segment, and the non-prime consumer segment, respectively. Columns (1), (3), and (5) report percent elasticities, while Columns (2), (4), and (6) report results for dollar regressions. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Full sample

Depvar: Δ CC Balance	(1)	(2)	(3)	(4)	(5)
Δ CC Limit	0.476**	0.597***			
	(2.39)	(2.97)			
Exposure			-0.739	-2.982**	
			(-0.29)	(-2.27)	
Δ CC Limit (Instru.)					1.526**
					(2.35)
Consumer × ST Cycle FE		✓		✓	✓
Trade-level controls	✓	✓	✓	✓	✓
Bank-level controls	✓	✓	✓	✓	✓
Observations	8,518,673	8,518,673	8,518,673	8,518,673	8,518,673
Adj. R^2	0.032	0.120	0.028	0.114	0.106
F-stat (Excl. instr.)					45.09

Panel B: Elasticities and dollar sensitivities across consumer segments

	B.1: Full sample		**B.2**: Prime		**B.3**: Non-Prime	
	% Δ CC Bal	$ Δ CC Bal	% Δ CC Bal	$ Δ CC Bal	% Δ CC Bal	$ Δ CC Bal
Depvar: Δ CC Balance	(1)	(2)	(3)	(4)	(5)	(6)
Δ CC Limit (Instru.)	1.526**	0.248***	1.406**	0.168***	3.208***	0.793***
	(2.35)	(7.69)	(2.39)	(5.78)	(2.68)	(7.95)
Consumer × ST Cycle FE	✓	✓	✓	✓	✓	✓
Trade-level controls	✓	✓	✓	✓	✓	✓
Bank-level controls	✓	✓	✓	✓	✓	✓
Observations	8,518,673	8,518,673	6,738,697	6,738,697	1,779,976	1,779,976
Adj. R^2	0.106	0.074	0.094	0.140	0.152	0.144
F-stat (Excl. instr.)	45.09	22.29	35.07	23.75	78.91	17.78

Heterogeneity in elasticity across consumer segments

Estimating the sensitivity of credit card balance changes to credit limit changes across all consumer segments in a pooled setting conceals the wide variation in elasticities across consumers. Thus, in this section, I examine the respective response sensitivities of prime and non-prime consumers to changes in limits. In addition, I also estimate dollar regressions to provide a cleaner measure of how consumers respond to a $1 change in limits. My findings are presented in Panel B of Table 1.4.

In Sub-Panel B.1, I first report elasticities and dollar sensitivities for the full sample of consumers. In Column (1), I re-state the 2SLS estimate presented in Column (5) of Panel A. In Column (2), I report the dollar regression 2SLS estimate. I find that in the full sample of consumers, a $1 stress test exposure–induced change in card-level limits is associated with a 24.8-cent change in card-level balances. In Sub-Panel B.2, Column (3), I find that the prime segment experiences a 1.4% change in balances for a 1% change in limits induced by the stress test exposure measure. In dollar terms, this translates to a 16.8-cent change in balances for every $1 of limit changes induced by the exposure measure (Column (4)). Finally, in Sub-Panel B.3, I report results for the non-prime consumer segment. I find that non-prime consumers respond strongly to changes in limits. Non-prime consumers increase balances by approximately 3.2% for every 1% increase in limits induced by stress test exposure (Column (5)). This translates to a 79.3-cent balance increase for every $1 stress test exposure–induced increase in credit card limits.

Relationship between credit card balances and stress test exposure across cycles

Thus far, the credit card balance analysis is reported for a concatenation of all stress test cycles. This masks important variation in the evolution of balances on cards issued by stress-tested banks across different cycles. Thus, similar to the analysis of card-level limits, I group consumers into credit score bins of 20 points, and report the effect of stress test exposure on the balances on credit cards issued to consumers that fall within each bin separately for each stress test cycle.

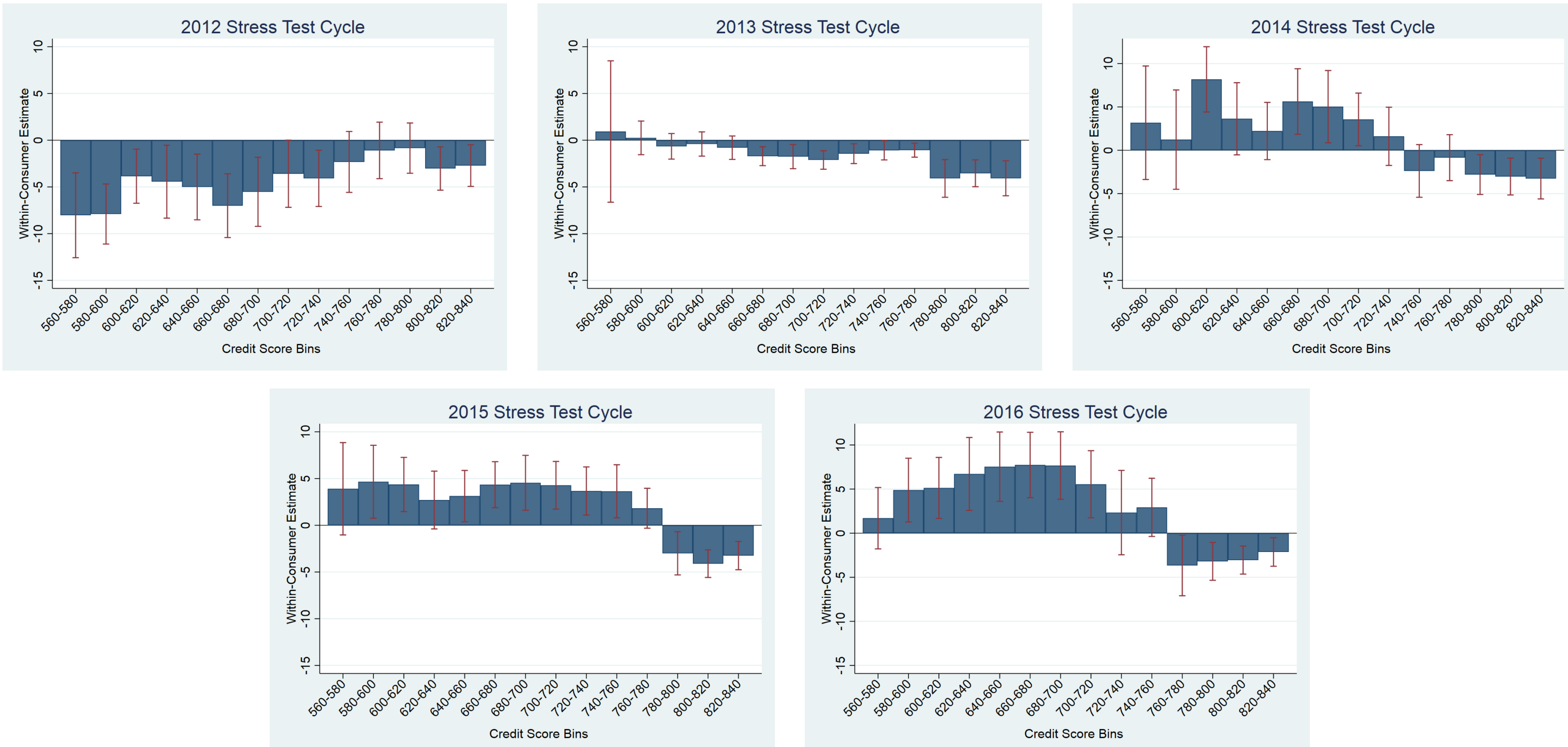


Figure 1.3: How has the evolving response of banks to stress tests affected credit card balances?

This figure examines how banks' changing response to stress tests over time in terms of credit card supply through credit card limits affects credit card balances. The figure reports the regression results of Equation (1.2), in which the dependent variable is replaced with card-level changes in balances. For each stress test cycle, consumers who have multiple credit cards issued by stress-tested banks are grouped into 20-point credit score bins ranging from 560 through 840. Next, the regression specification in Equation (1.2) is run for each credit score bin within each stress test cycle. The associated point estimates on the key independent variable, *StressTestExposure*, are presented in the form of bar graphs across different stress test cycles.

My findings are presented in Figure 1.3. I find that the balance results largely mirror the limit results presented in Figure 1.1. For the 2012 stress test cycle, non-prime consumers reduced balances on cards issued by high-exposure banks, consistent with high-exposure banks cutting limits on cards issued to such individuals. However, in later cycles, non-prime consumers increase balances on cards issued by high-exposure banks. At the same time, high-exposure banks experience declines in balances on cards issued to prime consumers.

1.5.5 Performance of credit cards issued by stress-tested banks

In Table 1.5, I study how higher stress test exposure affects the likelihood of future delinquency on credit cards issued by high-exposure banks. In earlier sections, I have shown that, compared to banks with low stress test exposure, high-exposure banks increase the supply of credit card limits offered to risky, non-prime consumers. This, in turn, induces risky borrowers to increase their balances on cards issued by high-exposure banks. In this subsection, I analyze whether high-exposure banks are more likely to grant additional credit to consumers that default more *ex-post*. This analysis of ex-post default outcomes serves as a complementary measure of bank risk-taking.

In Panel A, I examine trade-level performance at the 6-month horizon after the disclosure of stress test results. I estimate the regression specification in Equation (1.4), except without the *Bank* $\times$ *Stress Test Cycle* fixed effects. The dependent variable is an indicator variable that equals 1 if the credit card has a past due amount at the 6-month mark after the disclosure of stress test results, and 0 otherwise. The results are reported in Column (1).

The findings suggest that prime consumers are less likely to default on cards issued by high-exposure banks, although this estimate is only marginally significant at the 10% level. However, non-prime consumers are approximately 0.36 pp more likely to default on credit cards issued by high-exposure banks (relative to cards issued by low-exposure banks to the *same* consumer). Relative to the mean credit card delinquency rate of 6.07% for non-prime consumers, this estimate suggests that banks with a one standard deviation higher stress test exposure are approximately 5.93% more likely

Table 1.5: Consumer performance on credit cards issued by stress-tested banks

This table reports results for the relation between banks' stress test exposure and credit card–level performance. $Exposure$ is computed as the difference between the starting value of banks' Tier 1 ratio at the outset of the stress test and the lowest capital ratio implied by the severely adverse stress scenario. *Non-Prime* is an indicator variable that equals 1 if an individual's credit score is below 680 at the outset of a given stress test cycle, and 0 otherwise. Account-level performance is tracked at various intervals after the public disclosure of stress test results. In Panel A, card-level performance is measured at the 6-month mark after the public disclosure of stress test results. Credit cards that are delinquent 6 months after the public disclosure of stress test results are coded as 1, and 0 otherwise. Similarly, in Panels B and C, card-level performance is measured at the 12-month and 24-month horizons, respectively. The standard errors are clustered at the bank–year level. T-statistics are presented in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

	Panel A: $\frac{1}{2}$ year horizon		**Panel B**: 1 year horizon		**Panel C**: 2 year horizon	
Depvar: $\mathbb{1}$(Delinquency)	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.053*		-0.068		-0.092	
	(-1.78)		(-1.54)		(-1.54)	
Exposure × Non-prime	0.356***	0.361***	0.447***	0.453***	0.450**	0.460**
	(3.13)	(3.22)	(2.69)	(2.78)	(2.14)	(2.23)
Consumer × ST Cycle FE	✓	✓	✓	✓	✓	✓
Bank × ST Cycle FE		✓		✓		✓
Trade-level controls	✓	✓	✓	✓	✓	✓
Bank-level controls	✓		✓		✓	
Observations	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409
Adj. R^2	0.581	0.581	0.587	0.587	0.576	0.577

to experience defaults on cards issued to the non-prime segment. Notably, it is on these non-prime credit cards that high-exposure banks extend higher credit limits, thus inducing borrowers to carry higher balances. These results remain unaffected after the further inclusion of *Bank* × *Stress Test Cycle* fixed effects (Column (2)).

It is important to note that the specifications reported in Columns (1) and (2) are conducted at the credit card account level in the presence of individual fixed effects for each stress test cycle. Thus, for each consumer, the specification captures the probability of delinquency on cards issued by high stress test exposure banks *relative* to the probability of delinquency on cards issued by low-exposure banks to the *same* consumer. Furthermore, since this analysis is conducted at the trade level with individual fixed effects, any changes in individual-level characteristics (e.g., job loss, income declines, medical problems) are unlikely to explain the non-prime consumer segment's relatively higher probability of defaulting on cards issued by high-exposure banks.

In Panels B and C, I examine the respective trade-level performance at the 12-month horizon and the 24-month horizon after the disclosure of stress test results. At these longer horizons, I find no evidence suggesting that prime consumers are relatively more or less likely to default on cards issued by high-exposure banks. However, I continue to find that non-prime consumers are more likely to default on cards issued by high-exposure banks. In terms of economic magnitude, my estimates suggest that non-prime consumers are approximately 6–8% more likely to default on credit cards issued by high-exposure banks 1–2 years after the disclosure of stress test results.

In Figure 1.4, I examine the evolution of card-level defaults across different stress test cycles. I find that, across stress test cycles, non-prime consumers are increasingly more likely to default on credit cards issued by high-exposure banks. These findings thus largely track the earlier account-level limits analysis, where high-exposure banks increasingly issued higher limits to ex-ante risky consumers with each passing stress test cycle.

In sum, I find statistically robust and economically relevant evidence suggesting that over time, higher exposure to stress tests has induced banks to increase the supply of credit card limits extended to risky, non-prime consumers. Doing so induces non-prime consumers to increase the balances carried on cards issued by high-exposure banks. However, these non-prime consumers are also significantly more likely to default on cards issued by high-exposure banks in the future.

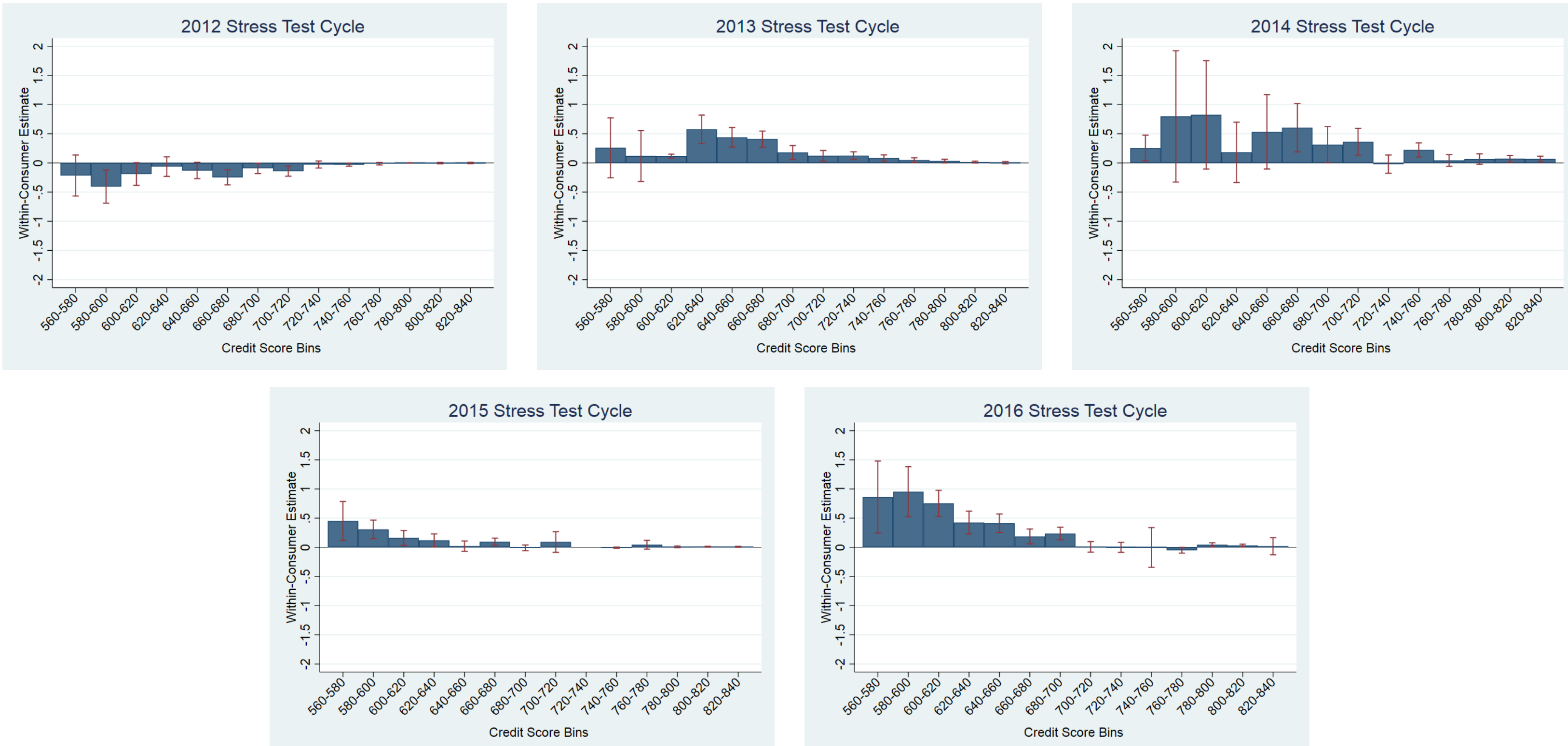


Figure 1.4: Evolution of ex post credit card defaults across stress test cycles

This figure examines how banks' changing response to stress tests over time in terms of credit card supply through credit card limits relates to ex post card-level defaults. The figure reports the regression results of Equation (1.2), in which the dependent variable is replaced with an indicator that equals 1 if the credit card is in default at the 12-month mark after stress test results release, and 0 otherwise. For each stress test cycle, consumers with multiple credit cards issued by stress-tested banks are grouped into 20-point credit score bins ranging from 560 through 840. Next, the regression specification in Equation (1.2) is run for each credit score bin within each stress test cycle. The associated point estimates on the key independent variable, *StressTestExposure*, are presented in the form of bar graphs across different stress test cycles.

1.5.6 Are credit card delinquencies related to consumer demographics?

The results discussed thus far show that banks respond to higher stress test exposure by issuing higher credit card limits to ex-ante risky consumers, and these consumers are more likely to default on cards issued by high-exposure banks in the future. Given the nature of credit card lending, which is almost entirely driven by hard information, banks may optimally respond to higher stress test–induced capital requirements by searching-for-yield in the credit card market. However, if additional credit is extended to consumers whose sophistication levels are not perfectly explained by credit scores, it is possible that the associated ex-post credit card delinquencies are driven by consumer characteristics that are not correlated with credit scores. In this section, I proxy for consumer sophistication through monthly income, education, and occupation. The results are presented in Table 1.6.

In Panel A, I proxy for consumer sophistication using monthly income, where higher income signals higher sophistication. In Column (1), I examine whether income plays a role in explaining ex-post defaults. I find that low-income consumers are significantly more likely to default on credit cards issued by high-exposure banks. However, it is possible that credit scores are highly positively correlated with income. Thus, in Column (2), I examine how card-level performance relates to consumer income, even in the segment of non-prime consumers. I find that even in the non-prime segment, low-income consumers are significantly more likely to default on cards issued by high-exposure banks.

In Panel B, I proxy for consumer sophistication using an indicator variable that equals 1 for consumers without a college degree and 0 for college degree–holders. I find that less-educated consumers are more likely to default on cards issued by high-exposure banks (Column (1)). Even in the segment of non-prime consumers, I find that non–college educated consumers are approximately 24.3% (0.090/0.370) more likely to default on credit cards issued by high-exposure banks (Column (2)). Lastly, in Panel C, I proxy for sophistication using occupation. I construct an indicator variable

Table 1.6: Types of consumers most negatively affected: Trade-level analysis

This table reports results examining how the relationship between banks' stress test exposure and credit card–level performance is affected by consumer characteristics. $Exposure$ is computed as the difference between the starting value of banks' Tier 1 ratio at the outset of the stress test and the lowest capital ratio implied by the severely adverse stress scenario. *Non-prime* is an indicator variable that equals 1 if an individual's credit score is under 680 at the outset of a given stress test cycle, and 0 otherwise. Panels A examines whether consumer income impacts card-level performance. Panel B examines the role of consumer education, where *No College* is an indicator that equals 0 if the consumer holds a college degree, and 1 otherwise. Lastly, Panel C examines the role of consumers' job sophistication, where *Non-Soph Job* is an indicator variable that equals 1 if the consumer holds a job that does not require sophisticated skills, and 0 otherwise. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Monthly income			**Panel B**: Education		
Depvar: $\mathbb{1}$(Delinquency)	(1)	(2)	*Depvar:* $\mathbb{1}$(Delinquency)	(1)	(2)
Exp × Income (log)	-0.143***	-0.020*	Exp × No college	0.041***	0.001
	(-4.87)	(-1.86)		(4.08)	(0.25)
Exp × Non-Prime		1.874**	Exp × Non-Prime		0.370***
		(2.40)			(9.61)
Exp × Non-Prime × Income (log)		-0.175*	Exp × Non-Prime × No college		0.090**
		(-1.93)			(2.04)
N	8,082,699	8,082,699		8,088,998	8,088,998
Adj. R^2	0.578	0.578		0.577	0.577

Panel C: Occupation		
Depvar: $\mathbb{1}$(Delinquency)	(1)	(2)
Exp × Non-soph job	0.011	0.004
	(0.87)	(0.51)
Exp × Non-Prime		0.355***
		(11.48)
Exp × Non-Prime × Non-soph job		0.032
		(0.47)
N	2,786,075	2,786,075
Adj. R^2	0.547	0.547

that equals 1 for consumers that hold jobs that do not require sophisticated skills, and equals 0 for consumers with sophisticated jobs.[15] My findings suggest that consumers with unsophisticated jobs are marginally more likely to default on high-exposure credit cards, but this effect is not statistically significant.[16]

1.6 Discussion: How are consumers affected by higher exposure to stress-tested banks?

So far, I have provided evidence suggesting that, over time, banks have responded to the stress test–induced increase in capital requirements by reaching-for-yield in the non-prime segment of the consumer credit card market. The issuance of higher credit limits to non-prime consumers leads to higher balances being carried on issued cards. Further consistent with higher risk-taking, I also show that these non-prime consumers are significantly more likely to default on cards issued by high-exposure banks. In this section, I examine whether higher consumer-level exposure to banks that are more adversely impacted by stress tests has implications for consumers' overall creditworthiness.

1.6.1 Impact on consumer delinquencies

In this section, I consider whether consumers that have greater exposure to banks that are adversely impacted by stress tests are more likely to experience credit card delinquencies in the future. For each consumer in my sample, I set an indicator variable equal to 1 if the consumer is delinquent on credit card payments 12 months after the public disclosure of stress test results, and 0 otherwise.

Next, I construct a weighted average measure at the consumer level called *Weighted Exposure*, which measures the reliance of consumers on high-exposure banks through their credit limits. *Weighted Exposure* is constructed by weighting the bank's stress test exposure associated with a

[15]The credit bureau does not have data on the occupations of all consumers. As a result, the results in Panel C are derived from a smaller sample of consumers in which I can distinguish between consumers with sophisticated versus non-sophisticated jobs.

[16]My inferences remain unchanged even if I run a specification where I horse-race the non-prime indicator variable (that identifies non-prime consumers) with measures of consumer income, education, and occupation. This assuages concerns that I am simply re-sorting the data on demographic characteristics that are highly correlated with credit scores. The results of this analysis are presented in Table A.6.

credit card by its credit limit as a proportion of the consumer's total credit limit. I restrict my analysis to consumers who have credit cards issued by banks with over \$10 billion in assets, which helps sidestep potentially problematic comparisons between consumers of banks and non-banks.

For the credit card–issuing banks that are not among the 23 banks in my *within*-consumer sample, I assume that their stress test exposure is zero. Relying on this assumption results in the misclassification of banks as having zero exposure when, in fact they could be affected by stress tests.[17] As a result, some individuals could be misclassified as unexposed or low-exposure individuals when they are actually high-exposure individuals. However, such a misclassification will only underestimate my findings.

I present my results in Table 1.7. In Column (1), I examine whether the *Weighted Exposure* measure is predictive of consumer default. The coefficient estimate is positive and significant, which suggests that consumers who are more reliant on high-exposure banks for credit cards are more likely to default in the 12 months after the public disclosure of stress test results.

It is important to note that in Table 1.7, I do not control for *Consumer*×*ST Cycle* fixed effects because the unit of observation is at the individual level. However, to control for unobserved confounding factors, in Column (2), I include controls for the individual's credit quality, such as the individual's credit score, debt-to-income ratio, credit card utilization, credit card and mortgage balance, and the number of credit-related accounts (all measured at the outset of a given stress test cycle). I find that my estimate drops by approximately 80% in economic magnitude after accounting for consumer characteristics, but remains significant at the 1% level. Unsurprisingly, this indicates that consumers' propensity to default is highly correlated with measures of credit quality.

Next, in Column (3), I include vectors of *5-digit ZIP code* and *ST Cycle* fixed effects, and find that my results are not significantly affected by their inclusion. In Column (4), I include *5-digit ZIP code* × *ST Cycle* fixed effects. This vector of fixed effects allows me to compare the future default

[17]Note that, under the Dodd-Frank Act, BHCs with over \$10 billion in assets are required to undergo internal company-run stress tests under the hypothetical, forward-looking scenarios created by the Fed and publicly report their performance.

Table 1.7: Effect of stress test exposure on consumer-level delinquencies

This table shows the relation between stress test exposure and consumer-level delinquencies. The dependent variable is a dummy variable that equals 1 if the consumer is delinquent (i.e., 90 days past due) on credit cards at any point in the year after the disclosure of stress test results. The key independent variable, *Weighted Exposure*, is constructed using account-level data. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately at the outset of a given stress test cycle, and after the public disclosure of stress test results for that cycle, by averaging across time. *Weighted Exposure* is computed at the individual level by aggregating the weighted *Exposure* measure at the credit card–level, where the weights assigned to a credit card are proportional to its credit limit. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: $\mathbb{1}$(Delinquency)	(1)	(2)	(3)	(4)	(5)
Weighted Exposure	0.195***	0.042***	0.058***	0.056***	0.014**
	(3.79)	(3.03)	(5.08)	(4.96)	(2.03)
Non-prime					0.945***
					(3.21)
Weighted Exposure × Non-prime					0.191***
					(3.22)
N	3,158,142	3,158,142	3,158,142	3,158,142	3,158,142
Controls		✓	✓	✓	✓
Fixed effects			*ST,Z5*	*ST×Z5*	*ST×Z5*
Adj R^2	0.000	0.077	0.079	0.081	0.081

likelihood of two consumers who live in the same ZIP code at the outset of a particular stress test cycle, but differ in their exposure to high–stress test exposure banks. Moreover, this set of fixed effects controls for common shocks at the ZIP code level (e.g., changes in house prices, changes in unemployment) that can affect the consumer's propensity to default. My findings remain unaffected by the inclusion of these fixed effects.

Lastly, in Column (5), I interact the *Weighted Exposure* measure with an indicator variable for non-prime consumers in the presence of *5-digit ZIP code*×*ST Cycle* fixed effects. Again, unsurprisingly, the coefficient estimate on *Non-Prime* suggests that non-prime consumers are significantly more likely to default on credit cards relative to prime consumers. However, the coefficient estimate on *Weighted Exposure* × *Non-Prime* is also positive and significant, indicating that even within the non-prime segment, higher reliance on cards issued by stress-tested banks with larger implied capital requirements (i.e., high-exposure banks) is associated with higher propensities of ex-post defaults.

My findings suggest that one additional unit of weighted exposure is associated with approximately a 20% higher default probability for non-prime consumers.

1.6.2 Impact on perceived consumer creditworthiness

In the previous section, I document that consumers with greater reliance on cards issued by high-exposure banks are more likely to experience credit card delinquencies in the future, and this effect is significantly larger for the non-prime segment. In this section, I examine whether this higher propensity for delinquencies affects the relative probability of non-prime consumers becoming prime consumers by crossing the industry-standard credit score threshold of 680 in the months following the disclosure of stress test results.[18]

The analysis is restricted to the sample of non-prime consumers. The dependent variable in the analysis is an indicator variable that equals 1 for non-prime consumers who turn (and remain) prime in the 12 months following the conclusion of a stress test cycle, and 0 otherwise. I present my findings in Table 1.8.

Similar to the analysis shown in Table 1.7, I progressively saturate my econometric specification with ex-ante measures of borrower quality, vectors of *5-digit ZIP code* and *ST Cycle* fixed effects, and finally, a vector of *5-digit ZIP code* $\times$ *ST Cycle*. Despite the addition of all controls, I find that non-prime consumers who are more reliant on credit cards issued by high-exposure banks are significantly *less* likely to become prime in the months following stress test disclosure. The estimate in Column (4), which contains the full set of controls and fixed effects, suggests that a non-prime consumer who has the median level of weighted exposure is approximately 5.3% less likely to become prime when compared to a non-prime consumer with no credit extended from high–stress test exposure banks.

[18] These thresholds have been shown to matter in credit markets ([18]).

Table 1.8: Impact of stress test exposure on consumer creditworthiness

This table examines how the perceived creditworthiness of borrowers is affected by having a high percentage of credit card limits issued by banks more negatively exposed to stress tests. Broadly, this table examines the relationship between stress test exposure and the probability of improved borrower creditworthiness in the non-prime consumer segment. The dependent variable is a dummy variable that equals 1 if the non-prime consumer transitions to prime status (i.e., has a credit score over 680) at any point in the year after stress test results are disclosed. The key independent variable, *Weighted Exposure*, is constructed using account-level data. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately at the outset of a given stress test cycle, and after the public disclosure of stress test results for that cycle, by averaging across time. *Weighted Exposure* is computed at the individual level by aggregating the weighted *Exposure* measure at the credit card–level, in which the weights assigned to a credit card are proportional to its credit limit. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: $\mathbb{1}$(Non-prime–Prime)	(1)	(2)	(3)	(4)
Weighted Exposure	-0.352***	-0.150***	-0.125***	-0.125***
	(-4.14)	(-3.26)	(-3.42)	(-3.32)
N	830,929	830,929	830,929	830,929
Controls		✓	✓	✓
Fixed effects			*ST*, *Z5*	*ST*$\times$*Z5*
Adj R^2	0.001	0.053	0.058	0.058

1.6.3 Heterogeneity across consumer demographics

In this section, I examine whether the above documented adverse consequences of higher consumer reliance on high-exposure banks are driven by measures of consumer sophistication not perfectly correlated with credit scores. I present my findings in Table 1.9.

In Panel A, I study consumer delinquencies. I find that the adverse outcomes of future defaults associated with higher reliance on high-exposure banks is driven by low-income consumers (Sub-Panel A.1). In Sub-Panel A.2, I examine whether education plays a moderating role. I find that higher reliance on high-exposure banks is not associated with higher future default likelihood in the sample of college-educated consumers. However, high-exposure consumers with no college degree are significantly more likely to be delinquent in the future. Lastly, in Sub-Panel A.3, I proxy for consumer sophistication based on occupation. As earlier, I set an indicator variable that equals 1 for consumers who hold jobs that do not require a sophisticated skillset, and 0 otherwise. I find that, on average, for a given level of weighted exposure, consumers with non-sophisticated jobs are approximately three times more likely to be delinquent in the future.

In Panel B, I study the impact of greater reliance on high-exposure banks on consumer creditworthiness. I find that high-exposure non-prime consumers with lower incomes are less likely to transition to prime status within one year of the conclusion of a given stress test cycle (Sub-Panel B.1). In Sub-Panel B.2, I find that non–college educated non-prime consumers with a 1-unit higher weighted exposure are approximately 17% less likely to transition to prime status in the future when compared to non–college educated non-prime consumers with no exposure to stress-tested banks. Lastly, in Sub-Panel B.3, I find that non-prime consumers with non-sophisticated jobs are also less likely to transition to prime status, but this effect is not statistically significant.

Thus, the results discussed in this section provides suggestive evidence that higher consumer reliance on credit cards issued by banks that are more exposed to stress tests has possible adverse consequences on consumers' financial well-being. Not only are high-exposure non-prime consumers significantly more likely to experience ex-post credit card defaults, they are also less likely to transition to prime status in the future when compared to low-exposure non-prime consumers. Given that credit scores mediate a large number of dynamic responses in credit markets (see [19, 14, 20, 21]) and that lenders use certain credit score thresholds in lending decisions ([18]), my findings suggest that high-exposure individuals are less likely to enjoy the benefits associated with higher credit scores. This is especially stark given that average consumer credit scores have increased in the years following the financial crisis.[19] Moreover, I also find that these potentially adverse effects are driven largely by consumers with lower income, lower education, and non-sophisticated jobs.

[19]Using credit bureau data, I plot average consumer credit score trends in Figure 1.5. I find that consumer credit scores have increased by approximately 10 points, on average, in the 10 years since the financial crisis of 2008.

Table 1.9: Heterogeneity across consumer demographics

This table examines the role of consumer demographics in explaining ex-post consumer creditworthiness. Panel A examines ex-post delinquencies, while Panel B studies the probability of non-prime consumers transitioning to prime status following the disclosure of stress test results. The dependent variable in Panel A is an indicator variable that equals 1 if the consumer is 90 days past due on credit cards one year after the disclosure of stress test results, and 0 otherwise. The dependent variable in Panel B is an indicator variable that equals 1 for non-prime consumers who transition to prime status after the disclosure of stress test results, and 0 for non-prime consumers who do not transition. The dependent variable is a dummy variable that equals 1 if the non-prime consumer transitions to prime status (i.e., has a credit score over 680) at any point in the year after stress test results disclosure. The key independent variable, *Weighted Exposure*, is constructed using account-level data. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately at the outset of a given stress test cycle, and after the public disclosure of stress test results for that cycle, by averaging across time. *Weighted Exposure* is computed at the individual level by aggregating the weighted *Exposure* measure at the credit card–level, in which the weights assigned to a credit card are proportional to its credit limit. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Impact of consumer demographics on delinquencies

B.1: Monthly income		**B.2**: Education		**B.3**: Occupation	
Depvar: $\mathbb{1}$(Delinquency)	(1)	*Depvar:* $\mathbb{1}$(Delinquency)	(2)	*Depvar:* $\mathbb{1}$(Delinquency)	(3)
Weighted Exp × Income (log)	-0.073**	Weighted Exp × No college	0.074***	Weighted Exp × Non-soph job	0.097***
	(-2.16)		(4.66)		(4.09)
Weighted Exp	0.645**	Weighted Exp	0.007	Weighted Exp	0.034***
	(2.25)		(0.64)		(2.87)
Income (log)	-0.852***	No college	-0.096*	Non-soph job	-0.173**
	(-8.81)		(-1.72)		(-2.02)
N	3,158,142		3,158,142		1,000,435
Fixed effects	*ST*×*Z5*		*ST*×*Z5*		*ST*×*Z5*
Adj R^2	0.101		0.101		0.088

Panel B: Impact of consumer demographics on perceived consumer creditworthiness

B.1: Monthly income		**B.2**: Education		**B.3**: Occupation	
Depvar: $\mathbb{1}$(Non-prime–Prime)	(1)	*Depvar:* $\mathbb{1}$(Non-prime–Prime)	(2)	*Depvar:* $\mathbb{1}$(Non-prime–Prime)	(3)
Weighted Exp × Income (log)	0.359***	Weighted Exp × No college	-0.213**	Weighted Exp × Non-soph job	-0.103
	(3.46)		(-2.12)		(-0.57)
Weighted Exp	-3.232***	Weighted Exp	-0.120	Weighted Exp	-0.399***
	(-3.90)		(-1.14)		(-2.97)
Income (log)	5.566***	No college	-1.271***	Non-soph job	0.041
	(13.90)		(-3.27)		(0.06)
N	830,929		830,929		222,340
Fixed effects	*ST*×*Z5*		*ST*×*Z5*		*ST*×*Z5*
Adj R^2	0.143		0.194		0.140

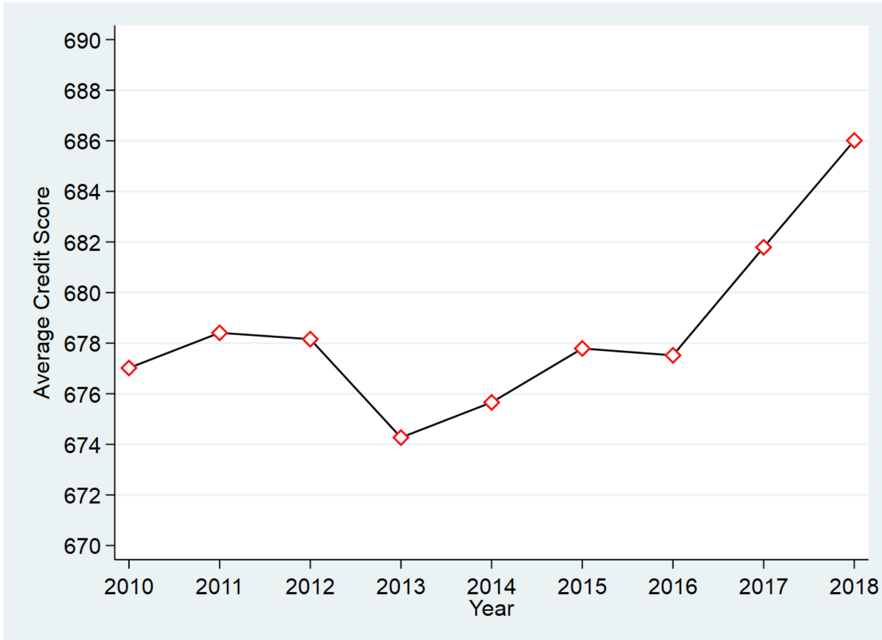


Figure 1.5: Average credit scores over time

This figure shows the evolution of consumer credit scores over time in the U.S. The sample covers the period 2010 through 2018. Consumer credit scores are measured using the Vantage 3.0 credit scoring model, which ranges from a low of 300 to a high of 850. The average credit scores for all consumers in the United States across time are represented in the form of red diamond-shaped symbols. These symbols are connected by a solid black line.

1.7 Conclusion

In this paper, I examine how banks respond to stress test–induced increases in capital requirements in the form of risk-taking in the consumer credit card market. Using detailed account-level data on credit card limits and balances, I implement a *within*-consumer empirical specification, which allows me to study the effects of stress tests on the credit card limits offered to the *same* consumer by two or more banks that have differential exposure to stress tests. I find that the earlier rounds of stress tests induced banks with larger implied capital requirements to sharply reduce limits for risky consumers, consistent with the risk-reduction effect of higher capital requirements. However, in later rounds of stress tests, I find that the same highly exposed banks have increasingly issued higher limits to ex-ante risky consumers. Thus, in later rounds, my findings are more consistent with the reach-for-yield incentives generated by higher capital requirements. This transition from risk-reduction to risk-seeking behavior appears to be mediated through banks adapting to stress tests over time. Furthermore, I find that issuing higher limits to risky consumers does indeed increase the riskiness of bank lending. I document that non-prime consumers, who have a high marginal propensity to borrow, respond to increased credit limits by increasing carried-over balances on cards issued by high-exposure banks.

Consistent with higher bank risk-taking in later rounds, I document that cards issued by high-exposure banks have higher ex-post likelihoods of default relative to cards issued by low-exposure banks to the *same* individual. Moreover, this higher relative propensity to default on cards issued by high-exposure banks is driven entirely by the non-prime consumer segment. Credit card lending is, by nature, driven almost entirely by hard information. Thus, banks may optimally respond to higher stress test–induced capital requirements by searching for yield in the non-prime consumer segment in the credit card market. However, if additional credit is extended to consumers whose sophistication levels are not perfectly explained by their credit scores, it is possible that the associated ex-post credit card delinquencies are distributed unequally across different consumer demographics. Consistently, I find that consumers with lower income, low education levels, and unsophisticated jobs are significantly more likely to default subsequently.

Lastly, I find suggestive evidence that greater reliance on credit cards issued by high-exposure banks can have potentially unfavorable implications for consumer credit outcomes. I document that consumers who have a larger percentage of their total credit card limits issued by high-exposure banks are significantly more likely to default when compared to consumers with no exposure to stress-tested banks. Moreover, this effect is driven almost entirely by the non-prime consumer segment. Consistently, I find that more affected non-prime consumers are thus significantly less likely to cross the credit score threshold of 680 ex-post (thus turning prime) compared to low-exposure non-prime consumers. Moreover, these effects are more pronounced for the low-income and less-educated consumer segments. Thus, taken together, I find that stress test–induced increases in capital requirements can induce higher risk-taking by banks. However, such potentially profit-maximizing "reaching-for-yield" behavior on the part of some banks can have adverse consequences on consumer creditworthiness, which are distributed unequally across different consumer demographics.

CHAPTER 2
IMPACT OF MARKETPLACE LENDING ON CONSUMERS' FUTURE BORROWING CAPACITIES AND BORROWING OUTCOMES

2.1 Introduction

Consumer lending, at $3.6 trillion in 2017, constitutes a significant share of the U.S. economy. Banks are the primary providers of credit to most consumers. However, over the last decade, several FinTech disruptors, including *marketplace lending* (MPL) platforms such as Lending Club and Prosper, have entered the consumer credit market and have grown rapidly. MPL platforms allow individual investors and institutions to compete with traditional financial intermediaries such as banks. Moreover, MPL platforms rely heavily on data-driven technology for origination and, in contrast to traditional banking models, they rely on direct due diligence by MPL investors ([49, 50]). Thus, greater credit market competition and the collective information production by MPLs and their investors could challenge the traditional banking model and affect how borrowers access credit.

In this paper, we examine how obtaining MPL credit affects a borrower's future borrowing capacities (i.e., credit scores, credit card utilization, and credit card limits) and their borrowing outcomes (i.e., debt accumulation and defaults). We interpret these effects within the context of the screening technology of the MPL platform and its investors. For instance, screening by MPL lenders implies that MPL borrowers should have fewer defaults relative to observably similar borrowers who are denied the credit. Moreover, if screening by MPL lenders signals better expected outcomes, then other lenders should be more willing to extend credit to MPL borrowers, thereby improving MPL borrowers' future borrowing capacities ([51]).

Using the same framework, we also examine how the screening technology of MPL lenders compares to that of traditional banks, in order to shed light on MPLs' ability to challenge the traditional banking model. However, it is challenging to identify screening, as lenders typically screen on private information that is not always observable to the econometrician. Our comprehensive account-level data for all U.S. consumers mitigates this challenge. First, we can account for detailed borrower characteristics before loan origination, which are also observable to lenders while they make their credit decisions. Next, we also observe the borrower's credit usage and credit outcomes ex post, which are correlated with the lender's private information. Thus, given observably similar MPL and bank borrowers, if MPL lenders have better private information and screen better than banks, then MPL borrowers' ex post credit outcomes and their ability to access credit should be better than bank borrowers.

We identify approximately one million borrowers on one of the largest U.S. MPL platforms using anonymized data from Equifax. These detailed data allow us to compare MPL borrowers to non-MPL borrowers and track their credit characteristics and credit outcomes at a monthly frequency. At loan origination, the average MPL borrower's credit score is 19 points lower than the average U.S. borrower. The average MPL borrower also has six more debt-related accounts than the average U.S. borrower. The most salient difference is that MPL borrowers have about twice as many credit cards, twice the amount of credit card balances, and twice the credit card utilization ratio as the average U.S. borrower. Overall, MPL borrowers appear to have lower observable credit quality and a higher reliance on credit card debt.

In order to construct observably similar benchmark borrowers to MPL borrowers, we focus on borrowers that reside in the same 5-digit ZIP code as an MPL borrower and who also applied for an unsecured installment loan from a bank in the same year–month as the MPL borrower. As a result, we obtain two cohorts of bank applicants: (a) those that successfully obtained credit from banks (i.e, *bank borrowers*) and (b) those that were denied credit from banks (i.e, *unmet borrowers*).[1] We further match these MPL and non-MPL borrowers on the various credit characteristics on which

[1] Our dataset does not contain information on borrowers who were denied credit by the MPL platform. Thus, we cannot identify and track these borrowers in our analysis.

MPL and non-MPL borrowers tend to differ.[2] Together, these observably similar benchmark borrowers allow us to compare the effect of obtaining MPL credit with that of obtaining or being denied comparable bank credit.

We begin our analysis by examining the effect of MPL loan take-up on a borrower's future borrowing capacities: the borrower's credit scores, credit limits, and credit utilization. We find that the borrowing capacities of MPL borrowers improve shortly after the MPL loan take-up. Within the first two months after obtaining the MPL loan, MPL borrowers experience a sharp 38-point increase in credit scores relative to unmet borrowers and a 13-point increase relative to bank borrowers. As a result, we find that 39.7% of the MPL borrowers in the unmet cohort jump from subprime to near-prime or near-prime to prime shortly after obtaining the MPL loan. We also find that lenders are more willing to supply credit to MPL borrowers after their loan take-up. However, in contrast to the credit score trends, the increase in credit limits is not immediate but rather grows steadily over time. Two years after the MPL loan take-up, MPL borrowers have 15% ($2,740) and 6% ($1,247) higher credit limits relative to the average pre-loan credit limits of $18,266 for unmet borrowers and $20,784 for bank borrowers, respectively. Finally, MPL borrowers also enjoy immediate higher borrowing capacities on their credit cards because MPL borrowers tend to consolidate their credit card debt with the MPL loan, which sharply reduces their existing credit card utilization. This response is consistent with the MPL borrowers' most frequently stated loan purpose of debt consolidation during their MPL loan application.[3]

Next, we analyze the effect of obtaining an MPL loan on a borrower's future borrowing outcomes as measured by debt accumulation and defaults. First, it is important to note that higher borrowing capacities after obtaining an MPL loan need not result in more immediate or future bor-

[2]We focus on unsecured personal installment loans from banks, as it is a comparable credit product to the MPL loan. We match the MPL and these non-MPL borrowers on the levels and trends of credit utilization ratios and credit scores. We also match them on the number of open accounts, revolving accounts, mortgage balance, total non-mortgage balance, credit card limits, monthly income, and debt-to-income ratio. Our matching exercise yields 347,172 unmet cohorts and 118,148 bank cohorts.

[3]Using publicly available data from Lending Club and Prosper, we find that approximately 81.82% of MPL loan applications state "debt consolidation" as the primary reason for accessing MPL credit.

rowing if, for instance, the *permanent income* hypothesis holds. However, if borrowers are credit constrained either currently or in the future, then greater borrowing capacities can lead to more borrowing. Similarly, if borrowing allows an individual to manage temporary liquidity shocks in the short term without having to default, then additional borrowing can lower defaults in the short run. In contrast, additional borrowing can also be associated with adverse selection if borrowers with a higher likelihood of default borrow more and default.

We document that MPL borrowers increase their credit card borrowing at a higher rate than the benchmark cohorts of bank and unmet borrowers. MPL borrowers were able to pay down their credit card debt by about 64% relative to the unmet borrowers immediately after obtaining the MPL loan; however, two years later, MPL borrowers have 14% ($1,448) more credit card debt than unmet borrowers. Similarly, MPL borrowers have 9% ($1,010) more credit card debt than bank borrowers. Further, two years after obtaining an MPL loan, MPL borrowers have higher aggregate debt and have higher required monthly debt repayments relative to their monthly income. Thus, our results suggest that MPL borrowers, despite the immediate debt consolidation, have a greater debt burden and appear relatively more liquidity constrained than their benchmark cohorts two years after the MPL loan take-up.

Next, we examine whether the additional borrowing by MPL borrowers affects their defaults. We find that two years after obtaining an MPL loan, MPL borrowers default across *all* types of debt more than the ex ante observably similar benchmark unmet and bank borrowers. Two years post loan origination, MPL borrowers default 1.13 percentage points (pp) (24% relative to the mean default rate) more than their benchmark unmet borrowers and 0.72 pp (20%) more than bank borrowers.[4] Consistent with the deterioration in long-term borrowing outcomes, MPL borrowers' credit scores are 2 points and 7 points lower than their benchmark unmet and bank borrowers, respectively, two years after the MPL loan take-up. In contrast, while the credit scores of the bank borrowers also jump after their loan take-up, their credit scores are relatively more persistent over time compared

[4]The 2-year mean default rate for the unmet cohort is 4.75%, and the bank cohort is 3.53%.

to MPL borrowers.

Thus, in stark contrast to the results on borrowing capacities, the worse borrowing outcomes of MPL borrowers suggest that MPL lenders screen less effectively than banks (i.e., adverse selection). We argue that our results are unlikely to be driven by MPL and bank borrowers facing different loan terms (i.e., the moral hazard effect). First, our results are robust to controlling for loan terms.[5] Next, MPL borrowers default at higher rates across all types of debt, which is arguably a mix of various loan terms. MPL borrowers also default more than bank borrowers on credit card debt, which generally has standardized terms across borrowers ([54]). Thus, the worse borrowing outcomes of MPL borrowers seem to be more indicative of their poorer credit quality rather than the loan terms available to them. Finally, we also show that our results are not sensitive to our matching procedure.

Next, we try to shed light on why borrowing from MPLs temporarily improves the borrowing capacities of borrowers, but MPL borrowers subsequently default at a higher rate than similar cohorts of bank borrowers and borrowers with unmet credit demand. Our evidence suggests that the initial credit score jump is somewhat "mechanical" in nature and is unlikely to be driven by an information spillover from the MPL loan. First, we observe that the credit scores of MPL borrowers jump even before MPL loan origination data are reflected in the credit bureau data.[6] Second, we find that this credit score jump is almost entirely explained by the sharp decline in the credit utilization of borrowers who use the MPL loan to immediately pay down their credit card debt. These results indicate that credit scores after obtaining an MPL loan jump because credit scoring models, which factor in credit utilization, mechanically assign a higher credit score after debt consolidation.[7]

Similarly, our analysis suggests that the MPL borrowers' higher credit limits relative to their benchmark borrowers are markedly explained by their mechanical credit score jump within the first

[5]Lenders can also use non-price loan terms (e.g., maturity, loan amount) to screen borrowers (see [52, 53]). Thus, controlling for loan terms could control for both the moral hazard effect and the screening effect associated with them.

[6]For instance, most MPL loans are reported to credit bureaus about three months after they are originated. However, credit scores increase within the first two months after loan origination.

[7]However, in practice, one fundamental reason why credit utilization factors heavily in credit scoring models (which are inverse relative default probabilities) is because consumers choose to default on their unsecured credit card debt before other secured debt (e.g., auto loans, mortgages).

two months after obtaining an MPL loan. Moreover, we find that the increase in credit limits is driven primarily by the opening of new credit card accounts as opposed to increased limits on existing accounts. As new lenders arguably rely more on credit scores for their lending decisions than existing relationship lenders, this evidence further highlights the role of the credit score jump in explaining the higher future credit limits of MPL borrowers.

Next, we attempt to understand why MPL borrowers subsequently default at a higher rate compared to similar cohorts of bank borrowers and borrowers with unmet credit demand. We provide suggestive evidence for the adverse selection of MPL borrowers on two dimensions. First, we find that MPL borrowers default more than bank borrowers because MPL borrowers are more likely to be liquidity constrained. That is, we find that contemporaneous changes in the required monthly debt repayments as a fraction of monthly income, a proxy for changes in liquidity constraints, can explain the differential default rates between MPL borrowers and the benchmark bank and unmet borrowers.

Second, we find that among MPL borrowers, those who use a greater fraction of their loan proceeds to consolidate debt have lower future default rates and higher future credit scores. Debt consolidation can be a positive signal because it suggests the borrower's intention to refinance expensive debt, pay off debt faster with fixed regular payments, or simplify and consolidate payments spread across multiple debt accounts. However, while debt consolidation is the primary stated loan purpose of the MPL borrowers, MPL lenders cannot ensure that their borrowers will use their loan proceeds for their stated loan purpose, thereby increasing the likelihood of adverse selection issues.[8] Further, in contrast to the MPL borrowers, we find this adverse selection on debt consolidation to be significantly smaller among bank borrowers.

Our results on adverse selection suggest that even though both MPL and bank borrowers obtain loans, the lower ability of MPL lenders to screen borrowers can pool more high–default risk

[8]For instance, despite stating debt consolidation as the loan purpose, borrowers with a higher default probability might be more likely to use their loan proceeds for consumption, thus ending up with more debt.

MPL borrowers with low–default risk MPL borrowers. However, over time, these borrowers' types are revealed through their private actions (e.g., debt accumulation, defaults). Consistent with these arguments, we find that the average borrowing capacities and outcomes of MPL borrowers deteriorate more steeply over time than bank borrowers. Overall, our results indicate that the screening technology of MPL platforms is weaker than banks.

Finally, we analyze why banks are able to screen better than MPL platforms. In our sample of first-time bank installment loan borrowers and MPL loan borrowers, we find that the better performance of bank borrowers is mainly driven by those who have a previous relationship with their originating bank as opposed to non-relationship bank borrowers. These results are consistent with banks having more information to screen their relationship borrowers, such as other account-level and transaction-level data, which are not easily available to the MPL platforms. We also find that after November 2014, when the MPL platform in our study reduced the information provided on loan applicants to their investors (see [50]), the default rates of MPL borrowers relative to bank borrowers worsened. These results suggest that investors are an important contributor to the MPL screening technology.

We contribute to the burgeoning literature on investor screening on MPL platforms. For example, [49] find that peer lenders display greater accuracy in predicting loan defaults than borrowers' credit scores. [55] suggest that lender screening improves over time owing to repeated participation on MPL platforms. Exploiting a natural experiment on the Lending Club platform, [53] show that loan terms (e.g., maturity) can be used to screen borrowers. [50] show superior screening by sophisticated investors on MPL platforms.[9] Broadly, this area of research has examined screening within MPL platforms. We contribute to this literature by benchmarking the screening technology of MPL platforms relative to traditional banks.[10]

[9] Other studies on MPL platforms have focused on how various borrower characteristics affect loan approval and loan pricing. See, for instance, the effect of race ([56]), the appearance of trustworthiness ([57]), attractiveness ([58]), social networks ([59]), and the unverifiable reasons stated on MPL loan applications ([60]).

[10] Another strand of literature studies whether MPL credit serves as a complement or substitute to bank credit (see [61, 62, 63]). However, this is not our study's focus.

Our results also highlight the superior information available to traditional financial intermediaries to screen borrowers ([64, 65]). Data-driven information processing can either substitute or complement traditional hard information such as credit scores ([66]), but our results show the limitation of using hard data to infer relationship-based information that is available to traditional financial intermediaries.

In a related paper, [51] finds that obtaining MPL credit has positive information spillover effects for borrowers. However, [51] focuses on repeat applicants. Like most peer-to-peer markets, the MPL platform typically relies on feedback and reputation systems to facilitate transactions by publishing information on past borrowing outcomes. Thus, it is likely that repeat borrowers are of higher quality because the platform screens out the low-quality borrowers over time. Alternatively, borrowers may exert more effort toward loan repayment in order to maintain continued access to the MPL platform.

To our knowledge, we are the first to show that MPL borrower outcomes improve temporarily after their loan take-up, but they subsequently default at a higher rate. [67] also document that FinTech borrowers are more likely to default and exhibit higher indebtedness than borrowers from traditional financial institutions. While they analyze the behavioral biases of FinTech borrowers, our study focuses on explaining these trends more generally by comparing the screening technologies of MPL platforms to traditional lenders and by exploring the economic factors that drive their distinctive screening technologies.

Our results contribute to the literature on the association between borrower quality and the mechanisms through which borrowers are funded on MPL platforms. The MPL loans in our study are funded using the price assignment mechanism, wherein a menu of take-it-or-leave-it loan contracts are offered to borrowers based on the platform's credit scoring algorithm.[11] Our results are consistent with [68] and [69], who show that while the price assignment mechanism improves the probability of funding loans, it comes at the cost of increasing adverse selection among borrowers.

[11]For instance, borrowers can leave the MPL platform without applying for a loan after they view the menu of loan contracts available to them.

Finally, our results also document the dependence of the credit market on credit scores, in which even non-fundamental changes in credit scores can have real effects on credit supply ([70, 20]). Overall, these results highlight a trade-off associated with automated underwriting using credit scores: While credit scoring can improve the overall screening ability of credit markets ([71]), it can also crowd out the production of soft information ([72, 73]).

2.2 Empirical methodology and identification

The main objective of this paper is to examine the effects of obtaining MPL credit on borrowers' future borrowing capacities and future borrowing outcomes. To this end, we evaluate the effects of obtaining MPL credit against two benchmarks. The first benchmark is an observably similar non-MPL borrower whose credit demand was unmet. In other words, such a non-MPL borrower was denied a loan after applying for it or the borrower refused to take up an approved loan. The second benchmark is an observably similar non-MPL borrower who obtains credit from a traditional financial intermediary such as a bank. For both benchmark non-MPL borrowers, we consider credit products that are comparable to the MPL loan, namely, unsecured bank personal installment loans.

By comparing the future borrowing capacities and outcomes of MPL borrowers to these benchmark non-MPL borrowers, we seek to compare the screening technology of MPL lenders with the screening technology of traditional bank lenders. However, a primary challenge in identifying screening is that screening typically occurs on information that is privately observed by the lender, but not the econometrician. We mitigate this challenge by using detailed micro credit bureau data on borrowers. First, these detailed data allow us to control for the borrower characteristics that lenders utilize for their credit decisions. Second, we also observe a borrower's behavior and outcomes ex post, which should be correlated with their lender's private information that was used to screen borrowers and make the credit decision. Thus, if MPL lenders screen better than banks, then MPL borrowers' expected credit market outcomes should be better than observably similar bank borrowers.

To compare the effect of obtaining an MPL loan on a borrower's future borrowing capacities and outcomes, we estimate the following cross-sectional regression model for *every* event-month $t \in [-12, +24]$ around the MPL loan take-up:

$$Y_{ic}^{t} - Y_{ic}^{-1} = \Delta Y_{ic}^{\{t,-1\}} = \alpha + \beta^{t} MPL_i + f(\mathbf{X}_i^{\{-1\}}) + \lambda_c + \varepsilon_{ic}, \tag{2.1}$$

where subscript i indexes individuals and subscript c indexes cohorts. A *cohort* is a pair of observably similar MPL and non-MPL borrowers who live in the same 5-digit ZIP code and apply for credit in the same year–month. We construct cohorts by implementing a k–nearest neigbhors (KNN) algorithm to match MPL and non-MPL borrowers on both the levels and the trends of several credit-related variables (e.g., credit score, credit balance, income, debt-to-income ratio) as described in detail in Section 2.3.3.

$\Delta Y_{ic}^{\{t,-1\}}$ represents the change in borrowing capacity or borrowing outcome of borrower i (who belongs to cohort c) from event-month -1 to event-month t. Event-month 0 represents the MPL (or equivalently bank) loan origination month. MPL_i equals 1 if individual i obtains an MPL loan, and equals 0 otherwise (i.e., the individual obtains a bank loan or the individual's credit demand is unmet). Thus, β^t captures the effect of obtaining an MPL loan on future credit outcomes and borrowing capacities t months after the MPL loan take-up relative to the non-MPL benchmark. $f(\mathbf{X}_i^{\{-1\}})$ is a vector of control variables that includes a large set of credit-related variables and loan terms and is measured in the month prior to the MPL loan origination. λ_c represents cohort fixed effects, and ε_{ic} is the error term. We double-cluster standard errors at the ZIP code and origination year–month level.

The model in Equation (2.1) is akin to the following fully saturated model in *levels*:

$$Y_{ict} = \alpha_i + \sum^{t} \beta^{t} MPL_i \times Month_t + \sum^{t} f(\mathbf{X}_i^{\{-1\}}) \times Month_t + \lambda_{ct} + \varepsilon_{ict},$$

with *Cohort*$\times$*Year-month* (λ_{ct}), *Individual* (α_i) fixed effects, an indicator for each month ($Month_t$), and allowing for the effect of the differences in ex ante characteristics to vary flexibly over time ($f(\mathbf{X}_i^{\{-1\}}) \times Month_t$).[12] However, the differenced model in Equation (2.1) is parsimonious and thus easier to implement compared to the fully saturated model in levels. We verify that both Equation (2.1) and the levels specification above provide the same results.

The differenced outcome variable $\Delta Y_{ic}^{\{t,-1\}}$ differences out the time-invariant borrower-specific heterogeneity α_i. Cohort fixed effects (λ_c) ensure that β^t, which is the coefficient associated with MPL_i, is identified *within*-cohort by comparing the MPL borrower to the matched non-MPL borrower.[13] The vector of control variables, $f(\mathbf{X}_i^{\{-1\}})$ also includes the matching variables used for constructing our cohorts prior to loan take-up to account for any imperfect matches between MPL and non-MPL borrowers.

Controlling for the observable information that is common to both the MPL and the bank's lending decision is important for our analysis. For instance, if both banks and MPL lenders use a borrower characteristic that is unobservable by the econometrician, then β^t can be biased if bank and MPL lenders intentionally obtain a different (priced) risk exposure on this omitted borrower characteristic. Typically, one cannot completely eliminate this possibility because the full set of borrower characteristics that lenders use for their credit decisions is generally unknown. However, this concern is mitigated in our setting because MPL lenders are required by the Securities and Exchange Commission to publicly disclose the information they use to evaluate the risk of loans. This information is primarily provided by credit bureaus, which is similar to our data, and consists of information such as the number of debt accounts and the usage and payment history on those accounts. Arguably, this information is also available to banks.

A further potential concern is that even if the common information set used by the lenders is

[12] We do not control for contemporaneous variables $f(\mathbf{X}_{i,t})$ to avoid the "bad" controls problem because some of the credit-related variables in $f(\mathbf{X}_{i,t})$ can themselves be affected due to the MPL loan take-up (see [74]).

[13] A cohort c is calendar-time and borrower-pair specific. This is because the MPL borrower and the counterfactual borrower are matched on the calendar year–month of the loan take-up (i.e., the calendar year–month and event year–month are perfectly collinear). Thus, cohort fixed effects perfectly absorb calendar-time and event-time fixed effects.

known, their proprietary credit model is unknown, which can again bias β^t if we assume a linear regression model. This concern is mitigated by our KNN matching analysis because matching MPL and non-MPL borrowers can account for any non-linear dependence of the outcome variable on the matching variables, thereby avoiding functional form restrictions imposed by a linear regression model. Most of our baseline analysis relies on the matched cohorts, but we also show that our results are similar if we conduct this analysis using the unmatched sample by controlling for all the matching variables used to construct our cohorts.

In additional robustness tests involving comparisons of MPL borrowers and bank borrowers, we also include loan terms as controls to account for the possibility that the differences in future credit outcomes and borrowing capacities are driven by the differences in loan terms between the MPL loans and the bank installment loans. For instance, taking up a larger loan, or a higher interest rate-loan today might reduce the borrower's future borrowing capacity.

2.3 Data and summary statistics

2.3.1 Data

We obtain our data from Equifax. All the data are used purely for academic purposes, and they contain completely anonymized information. The credit bureau's trade line-level data provide comprehensive, anonymized records of the various lines of credit opened by every U.S. consumer. Our dataset spans the time period between 2011 and 2016. We use this dataset to identify all individuals who originated unsecured installment loans from one of the largest MPLs in the consumer credit space in the U.S. We identify 1,283,429 MPL borrowers in our primary dataset.[14] Among these borrowers, we include only loans with nonmissing loan origination dates and strictly positive balances (91.84% of sample). Next, we exclude repeat borrowers and focus only on one-time MPL borrowers

[14]We use publicly available data from the MPL platform in our study, and we find that the credit bureau's sample covers approximately 98.65% of all loans self-reported by the MPL platform on its website.

on this platform (81.39% of sample).[15] After applying these filters, our data consist of 1,043,370 one-time MPL borrowers who originated an MPL loan between 2011 and 2016.

Next, we merge our MPL origination data with the borrower's credit data (e.g., credit score, credit balance, utilization ratio, defaults) and demographic data (e.g., monthly income, occupation, education).[16] These credit data consist of monthly snapshots at the individual level. We gather the credit data for a borrower from 12 months before the origination of the MPL loan (the *pre-period*) to 24 months after origination (the *post-period*). As the demographic data are available only since June 2013, we further restrict our sample to MPL loan originations between June 2013 and December 2016. Thus, our final sample consists of 763,986 borrowers who obtained an MPL loan between June 2013 and December 2016.

2.3.2 Descriptive statistics: MPL borrowers versus the average U.S. borrower

In Table 2.1, Panel A, we compare the characteristics of the MPL borrowers in the month before the MPL loan origination to a 5% random sample of the total U.S. population. The results show that the average MPL borrower's credit score is 19 points lower than the average U.S. borrower's credit score. The average MPL borrower also has about six more debt accounts than the average U.S. borrower. The average MPL borrower most saliently differs from the average U.S. borrower on credit card–related debt accounts. For instance, the average MPL borrower has about twice the number of credit cards, twice the amount of credit card balances, and twice the credit utilization ratio as the average U.S. borrower.

[15]This filter reduces concerns about strategic borrower behavior, which can contaminate the analysis of the effect of MPL loan take-up on future borrowing capacities and outcomes.

[16]We have access to Vantage 3.0 credit scores as opposed to FICO scores. The MPL we study uses FICO scores for its lending decisions. For our empirical analysis, it is important to note that the correlation between the Vantage 3.0 score and the FICO score is greater than 0.9. Source: *http://files.consumerfinance.gov/f/201209_Analysis_Differences_Consumer_Credit.pdf*.

Table 2.1: Descriptive statistics

This table presents descriptive statistics of the credit and income characteristics of MPL borrowers. Panel A compares the characteristics of MPL borrowers in the month before MPL loan origination to a 5% random sample of the total U.S. population. Panel B compares MPL borrowers to all non-MPL borrowers who unsuccessfully apply for a bank installment loan (i.e., unmet borrowers) and to non-MPL borrowers who apply for and receive a bank installment loan (i.e., bank borrowers). Both sets of non-MPL borrowers are chosen such that they reside in the same 5-digit zip-code as the MPL borrower and apply for the installment loan in the same month as the MPL borrower. Panel C compares MPL borrowers to matched counterfactuals of observably similar bank borrowers and unmet borrowers using data from Panel B.

Panel A: Comparing MPL Borrowers to Average American Consumer

	MPL Platform Borrowers	National Average
	(I)	(II)
Credit card variables		
Credit Score	656.44	675.47
Credit card utilization	69.42%	30.89%
Credit card balance	$9,821	$4,197
Credit card accounts (#)	3.84	1.97
Other credit variables		
Debt accounts (#)	10.49	4.68
Total Debt	$232,463	$208,195
Monthly Income	$3,602	$3,437
Debt-to-Income	41.03%	27.82%
Other debt		
Auto accounts (#)	1.02	0.66
Mortgage accounts (#)	0.86	0.79
Student loan accounts (#)	2.23	1.66
Auto debt	$20,659	$17,038
Mortgage debt	$189,597	$186,237
Student debt	$24,425	$19,122
Other characteristics		
% College Graduates	26.85%	32.30%
% Sophisticated Job	19.64%	19.52%

Panel B: Pre-matching summary statistics

	MPL	Unmet	Bank
Observations	734,742	17,369,467	2,916,178
Credit card variables			
Credit score	656	634	700
Credit card utilization	70	48.9	43.8
Credit card balance	9,943	6,470	8,368
Credit card accounts (#)	3.84	2.28	3.01
Other credit variables			
Debt accounts (#)	10.7	6.02	8.31
Total Debt	229,539	259,163	212,614
Monthly income	3,584	3,419	3,902
DTI ratio	39.7	37.1	36.9
Originated loan terms			
Loan amount	13,744	—	12,893
Loan maturity (months)	42	—	42.7
Loan interest rate (%)	16.2	—	10.2
Monthly loan payment	423	—	318

Panel C: Post-matching summary statistics

	Unmet Cohort		Bank Cohort	
	MPL	Unmet	MPL	Bank
Observations	347,172	347,172	118,148	118,148
Credit card variables				
Credit score	654	655	669	672
Credit card utilization	72.3	71.7	67.7	67.5
Credit card balance	10,935	11,220	11,559	12,160
Credit card accounts (#)	4.07	3.48	4.18	3.56
Other credit variables				
Debt accounts (#)	11.1	9.67	11.21	9.95
Total debt	243,119	235,453	238,749	226,424
Monthly income	3,654	3,719	3,797	3,849
DTI ratio	41.6	40.5	41.9	41.3
Originated loan terms				
Loan amount	14,207	—	15,078	13,588
Loan maturity (months)	42.2	—	42.8	49.3
Loan interest rate (%)	16.1	—	14.3	11.8
Monthly loan payment	434	—	451	345

While the monthly income of the average MPL borrower is similar to that of the average U.S. borrower, the average MPL borrower is more indebted. The total debt-to-income (DTI) ratio for the average MPL borrower is 41.03%, compared to 27.82% for the average U.S. borrower. Relative to the average U.S. borrower, the average MPL borrower is equally likely to have a sophisticated job, although MPL borrowers are less likely to have a college degree.[17] Overall, one of the main takeaways from Table 2.1, Panel A, is that the average MPL borrower differs significantly from the average U.S. consumer in their credit card use.

2.3.3 Cohort construction and summary statistics

This section describes the construction of cohorts for our empirical analysis. Each cohort consists of a pair of observably similar MPL and a non-MPL borrower who live in the same 5-digit ZIP code and apply for credit in the same year–month. We construct two types of cohorts—namely, the *bank cohort* and the *unmet cohort* . These two types of cohorts correspond to the two benchmarks against which we compare an MPL borrower's future credit capacities and outcomes. The bank cohort consists of observably similar non-MPL borrowers who obtain unsecured installment loans from banks . The unmet cohort consists of observably similar non-MPL borrowers whose demand for unsecured bank installment loans is unmet.

We start with 763,986 MPL borrowers who obtained an MPL loan between June 2013 and December 2016 (see Section 2.3.1). For every MPL borrower, we identify non-MPL borrowers who reside in the same 5-digit ZIP code as the MPL borrower and who also applied for a bank installment loan in the same month as the MPL borrower. This procedure yields about 19 million unmet borrowers and 3 million bank borrowers. Next, we omit borrowers who have multiple credit applications in the same month. This filter retains 734,742 MPL borrowers (96% of the total MPL borrowers), 17.37 million unmet borrowers (92%), and 2.92 million bank borrowers (96%). Table 2.1, Panel B,

[17] We classify jobs in the physician/dentist, lawyers/judges, professional/technical, management, business owner industries as sophisticated jobs.

compares these MPL, unmet, and bank borrowers.

Table 2.1, Panel B, shows that MPL and non-MPL borrowers differ significantly even after controlling for location and loan application period. On average, MPL borrowers' credit scores are 22 points higher than that of unmet borrowers, but they are 44 points lower than bank borrowers. The MPL borrowers have significantly higher credit card utilization ratios, higher credit balances, and more debt-related accounts than both unmet and bank borrowers. Despite their lower credit scores and higher debt-to-income ratios, MPL borrowers receive a 6.6% higher loan amount than bank borrowers. However, MPL borrowers pay a 6 pp higher interest rate than the bank borrowers for a loan with similar maturity. Overall, MPL borrowers pay $105 (or 33%) higher monthly payments towards loan repayment than the bank borrowers.

To control for the observable differences between MPL and non-MPL borrowers we employ a tight matching algorithm. We match MPL and non-MPL borrowers to have similar monthly trends and levels in their credit scores, credit card balances, and credit card utilization ratios over the one year before their respective loan applications. This is because Panels A and B of Table 2.1 show that the average MPL and non-MPL borrowers differ most significantly on credit card–related variables. Next, we use a k-nearest neighbor (KNN) matching algorithm to match the MPL and the non-MPL borrowers on their number of debt accounts, number of revolving debt accounts, mortgage debt, non-mortgage debt, monthly income, and their monthly debt-to-income ratios in the month before their loan applications.

Out of 734,742 MPL borrowers, 590,201 of them (80%) have complete credit history information for conducting the above matching procedure. Using the above matching criteria and borrowers with non-missing credit data, we can match 347,172 MPL borrowers (or 59%) to unmet borrowers to create our unmet cohort, and we can match 118,148 MPL borrowers (or 20%) to bank borrowers to create our bank cohort. These match rates suggest that MPL borrowers tend to be observably more similar to the unmet borrowers than the bank borrowers. Overall, our unmet cohort and bank cohorts consist of MPL borrowers and their counterfactual non-MPL borrowers who (a) reside in the same 5-digit ZIP code, (b) applied for their installment loans in the same month, (c) exhibit identical credit card–related trends, and (d) are similar on other credit-related variables.

Table 2.1, Panel C shows the summary statistics for the unmet cohort and the bank cohort in the month before MPL loan origination. The MPL and non-MPL borrowers within both cohorts seem balanced on credit scores, credit card utilization, and credit card balances. The balancing based on other credit characteristics (e.g., monthly income, total debt) for the MPL and non-MPL borrowers within both cohorts has also improved compared to the pre-matched sample in Table 2.1, Panel B. Despite having similar credit characteristics, the average MPL borrower in the bank cohort can obtain a loan amount that is 11% higher than the bank borrower. The maturity of the MPL borrower's loan is 7 months shorter, and the interest rate is 2.5 pp higher than the bank borrower's loan. Finally, the average MPL borrower within the bank cohort pays $106 (or 31%) higher monthly payments than the bank borrower towards loan repayment.

2.4 How does MPL Borrowing Impact Consumers' Future Borrowing Capacities and Borrowing Outcomes?

2.4.1 Graphical analysis

We begin by plotting the monthly trends for borrowing capacities and outcomes in Figure 2.1 and Figure 2.2, respectively, to visually illustrate the effect of MPL loan take-up. The dashed vertical lines in all the plots indicate the loan origination month. These figures also illustrate the pre-trends in borrowing capacities and outcomes before the loan take-up.

Figure 2.1 shows an immediate increase in borrowing capacities for both MPL and bank borrowers through a sharp reduction in credit utilization ratios and a jump in credit scores within the first two months after their loan take-up. The credit card balance trends in Figure 2.2, Panel A, show that the sharp reduction in credit utilization ratios is driven by the paying down of credit card debt immediately after obtaining the loan. This pattern is consistent with debt consolidation, which is the main stated purpose of MPL loans.[18]

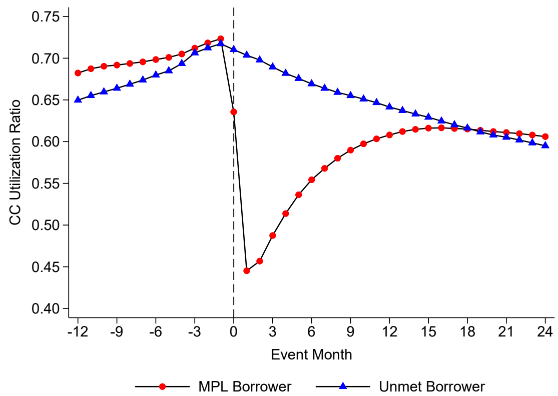
Panels A and B of Figure 2.1 also show a striking pattern: The credit score trends closely mirror the credit utilization trends. For instance, the credit scores of both MPL and bank borrowers rise sharply in the first two months after their loan originations. These are also the months during which the MPL and bank borrowers consolidate their credit card debt. The credit scores of both MPL and bank borrowers fall after the second month following loan origination. However, relative to the bank borrowers, the credit scores of the MPL borrowers fall more steeply and then revert to their pre-loan take-up levels within two years. Over the same period, the credit utilization ratios of the MPL and bank borrowers increase steadily, but with a much steeper increase for the MPL borrowers. This suggests that the MPL borrowers borrowed against their borrowing capacities at a faster rate.

The credit card balance trends in Figure 2.2, Panel A, indicate that credit consolidation is short lived. Within two years of obtaining loans and consolidating their debt, the credit balances of both the MPL and bank borrowers steadily increase and eventually surpass their pre-loan take-up levels. In contrast, the unmet borrowers, whose bank loan applications were unsuccessful, could not consolidate their debt. Consequently, their credit balances remain steady over time. Figure 2.2, Panel A, also suggests that the MPL, unmet, and bank borrowers experienced an increase in their credit limits over time. This is because their credit utilization ratios two years after their loan originations are lower than their pre-loan take-up values despite having similar, or higher, credit balances.

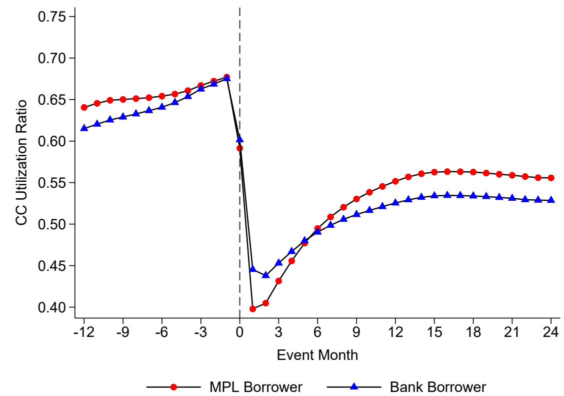
Panels B and C of Figure 2.2 plot the default trends for the unmet and bank cohorts. A borrower is considered to be in default in month t if the borrower is 90+ days past due on a required payment in month t. Thus, this default rate definition allows for self-cures and considers self-cures in month t as non-delinquent borrowers in month t. For each type of debt (e.g., credit card debt), we compute the average default rate as the number of delinquent borrowers as a fraction of the total number of borrowers.

[18] The publicly available data for the MPL lender in our study show that 81.8% of all approved MPL borrowers during our sample period state *debt consolidation* as the loan purpose on their loan applications.

Panel A: Credit Card Utilization

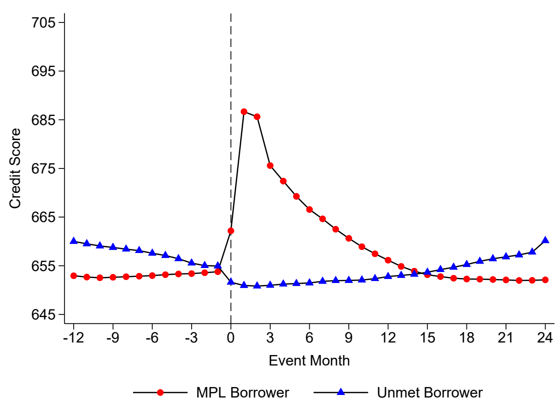


Unmet Cohort

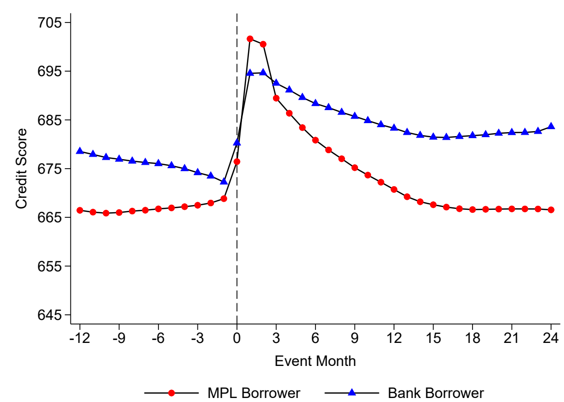


Bank Cohort

Panel B: Credit Scores



Unmet Cohort

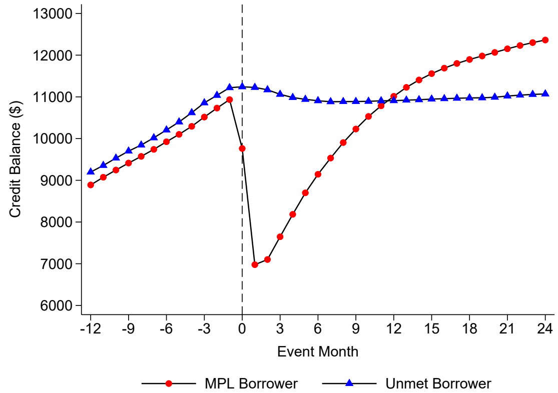


Bank Cohort

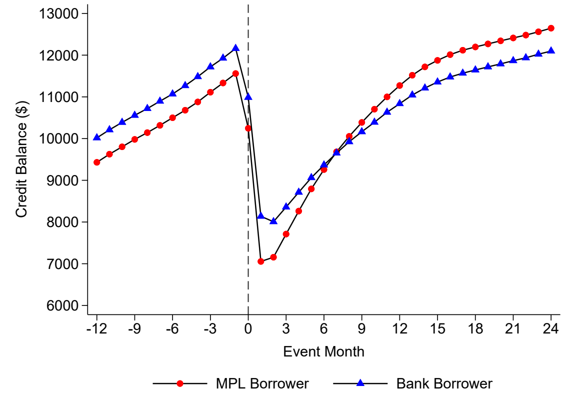
Figure 2.1: Impact of MPL loans on borrowing capacities

This figure presents average trends in the borrowing capacities of MPL borrowers in the months around MPL loan origination for the unmet and bank cohorts. Panel A documents trends in credit card utilization ratios, Panel B documents trends in credit scores. In both panels, the *x*-axis displays event time relative to the month of loan origination and the *y*-axis represents borrowing capacities.

Panel A: Credit Card Balance

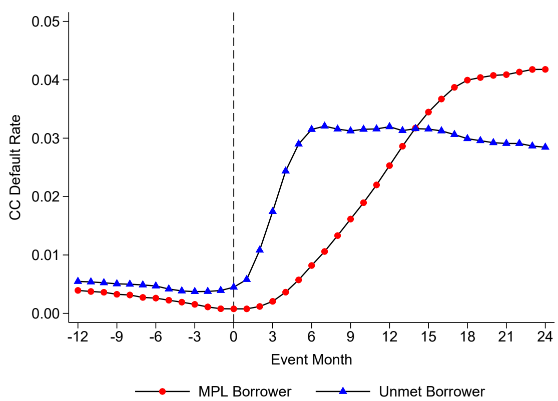


Unmet Cohort

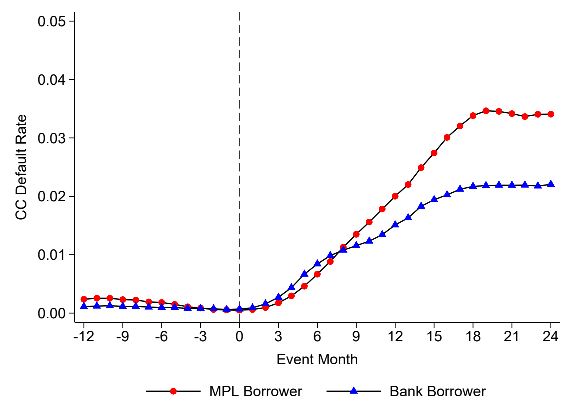


Bank Cohort

Panel B: Credit Card Defaults

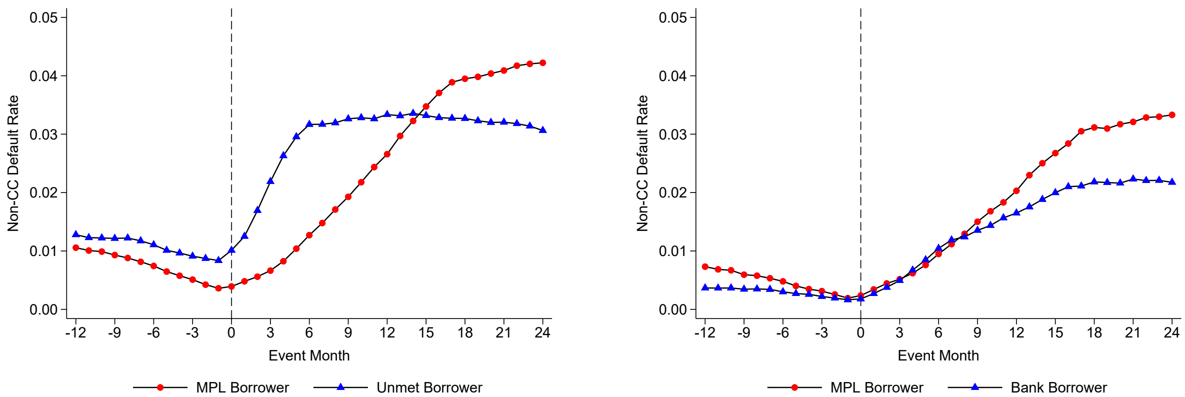


Unmet Cohort



Bank Cohort

Panel C: Non-Credit Card Defaults



Unmet Cohort

Bank Cohort

Figure 2.2: Impact of MPL loans on borrowing outcomes

This figure presents average trends in the borrowing outcomes of MPL borrowers in the months around MPL loan origination for the unmet and bank cohorts. Panel A documents credit card balance trends, Panel B documents credit card default trends, and Panel C documents non-credit card default trends. In all panels, the *x*-axis displays event time relative to the month of loan origination and the *y*-axis represents borrowing outcomes.

The default rates of the unmet borrowers for both credit card debt and non–credit card debt increase sharply within the three months after their unsuccessful loan applications. This pattern contrasts with the default rates of the MPL and bank borrowers, which remain relatively low in the first three months after their loan take-up but then trend upwards thereafter. Two years after the loan take-up, MPL borrowers default at a higher rate across all types of debt relative to the benchmark unmet and bank borrowers.

2.4.2 Univariate Analysis

We supplement our graphical analysis with summary statistics for the effect of MPL loan take-up in Table 2.2 for the unmet cohort, the bank cohort, and the full unmatched sample of MPL borrowers. Panel A of Table 2.2 presents the immediate short-run response of MPL loan take-up on future borrowing capacities and outcomes. We find that the total debt of both MPL and bank borrowers increases in the immediate term, which suggests that both sets of borrowers use only a portion of their loan proceeds to consolidate debt. This is because, if the entire loan proceeds are used to consolidate debt, then the borrower's total debt should remain unchanged.[19] The average MPL borrower in the full unmatched sample consolidates less debt than the MPL borrower in the bank and unmet cohorts. The change in the DTI ratio for the MPL borrowers is also greater than both the unmet and bank borrowers immediately after loan take-up. This suggests that the MPL borrowers must set aside a larger fraction of their monthly income for debt repayment.

The credit score of the unmet borrower drops by about 4 points, while the credit scores of the MPL and the bank borrowers increase by 33 and 22 points, respectively, shortly after their loan take-up. The unmet borrowers default more than the MPL and bank borrowers shortly after not obtaining the credit. Overall, Table 2.2, Panel A, shows an improvement in borrowing capacities and borrowing outcomes for the MPL and the bank borrowers relative to the unmet borrowers immediately after loan take-up.

Table 2.2, Panel B, presents the long-run response of MPL loan take-up on future borrowing capacities and outcomes. Panel B shows that two years after the loan take-up, the total debt of the MPL borrowers is still relatively higher than the unmet and the bank borrowers. Comparing the long-term debt response to the short-term debt response suggests that all the three types of borrowers do not accumulate much debt beyond the first month after loan origination. However, over the same period, the monthly DTI ratio of the MPL borrower increases significantly relative to both the bank

[19]The increase in total debt immediately after loan origination ($\Delta TotalDebt$) equals the loan amount (L) minus the consolidation amount (C) (i.e., $\Delta TotalDebt = L - C$).

Table 2.2: Summary Statistics

This table presents descriptive statistics that document changes in the credit profile characteristics of MPL borrowers after the MPL loan origination. Panel A reports the immediate response of MPL borrowers' credit characteristics one month after MPL loan take-up. Panel B reports the long-run response of MPL borrowers' credit characteristics 2 years after MPL loan origination. Both panels report changes in the credit profile characteristics of the full sample of MPL borrowers after MPL loan origination. Both panels also compare changes in the credit profile characteristics of MPL borrowers to observably-similar matched bank borrowers and borrowers with unmet credit demand.

Panel A: Short-run $(-1, +1)$ response to MPL take-up

	Full Sample	*Unmet Cohort*		*Bank Cohort*	
	MPL	MPL	Unmet	MPL	Bank
ΔLog(Total Debt) (log-pts)	21.719	19.462	1.386	18.271	14.462
ΔDTI Ratio (pp)	8.190	7.987	1.547	7.788	5.198
Borrowing capacities					
ΔCredit Utilization (pp)	-25.905	-27.791	-1.352	-27.886	-22.926
ΔCredit Score (pts)	30.295	32.88	-4.004	32.802	22.303
ΔLog(Credit Limit) (log-pts)	4.340	3.312	2.869	2.493	1.696
Borrowing outcomes					
ΔLog(Credit Balance) (log-pts)	-98.439	-103.157	-5.43	-113.726	-112.959
ΔCredit Card Defaults (pp)	-0.008	-0.002	0.201	0.007	0.029
ΔNon-Credit Card Defaults (pp)	0.125	0.122	0.422	0.149	0.107

Panel B: Long-run $(-1, +24)$ response to MPL take-up

	Full Sample	*Unmet Cohort*		*Bank Cohort*	
	MPL	MPL	Unmet	MPL	Bank
ΔLog(Total Debt) (log-pts)	25.429	20.686	-3.155	19.693	11.469
ΔDTI Ratio (pp)	18.222	17.567	4.043	15.467	7.854
Borrowing capacities					
ΔCredit Utilization (pp)	-8.545	-11.276	-11.284	-11.716	-14.094
ΔCredit Score (pts)	-5.602	-1.735	4.951	-2.295	11.243
ΔLog(Credit Limit) (log-pts)	40.320	36.476	20.476	30.602	21.781
Borrowing outcomes					
ΔLog(Credit Balance) (log-pts)	0.708	-9.747	-31.823	-22.714	-45.523
ΔCredit Card Defaults (pp)	4.279	4.101	2.523	3.353	2.147
ΔNon-Credit Card Defaults (pp)	4.023	3.867	2.256	3.14	2.019

borrower and the unmet borrower. This can occur if MPL borrowers substitute their existing debt with more expensive debt (i.e., debt with higher interest rates) or more short-term debt (i.e., debt with shorter maturity), both of which will result higher monthly payments. Alternatively, this can also occur if the MPL borrowers' incomes reduce more than the bank borrowers' and unmet borrowers' incomes.

The credit limits of the MPL borrowers are higher than both counterfactual benchmarks two years after loan take-up. Finally, Table 2.2 , Panel B, shows that, relative to the benchmark borrowers, the MPL borrowers default at a higher rate in the long run across all types of debt, and they have lower credit scores.

Overall, the univariate analysis shows that the borrowing capacities (e.g., credit scores) and outcomes (e.g., credit card defaults) of the MPL borrowers improve shortly after the MPL loan take-up relative to their benchmark borrowers. Thus, the short-run response to the MPL loan take-up, which indicates an improvement in the borrowing capacities of MPL borrowers, suggests that MPL lenders screen their borrowers more effectively than banks. However, this improvement in the MPL borrower's borrowing capacities and outcome is temporary, since they both deteriorate faster than the benchmark borrowers. Two years after the MPL loan take-up, MPL borrowers have, on average, lower borrowing capacities and more adverse credit outcomes than their benchmark bank and unmet borrowers. Thus, in stark contrast to the short-run response, the long-run response suggests that MPL lenders' screening technology is weaker than banks.

2.4.3 Regression analysis of the effect of MPL loan take-up

In this section, we examine the effect of obtaining an MPL loan on a borrower's future borrowing capacities and borrowing outcomes by estimating our baseline specification in Equation (2.1) over different time horizons relative to the month of MPL loan take-up. Our regression results are largely similar to the univariate analysis shown in Table 2.2. However, by including cohort fixed effects and the matching variables in our regression model, we are better able to compare the MPL borrower with the non-MPL borrower within a cohort while also controlling for confounding effects due to any inexact matching.

We do not include loan terms as controls in our baseline analysis in order to facilitate a comparison between the unmet cohort and bank cohort. This is because loan terms are not available for unmet borrowers as they were unsuccessful in obtaining a loan. However, in robustness checks for the bank cohort, we also control for loan terms and show that our results are qualitatively similar.

How does MPL Borrowing Impact Consumers' Borrowing Capacities?

Credit Utilization

Columns (1) and (2) of Table 2.3 report the regression results for the change in credit utilization for MPL borrowers relative to the unmet and bank borrowers, respectively. We estimate the baseline specification in Equation (2.1) with the change in credit utilization ratio as our dependent variable. All columns display β, which is the coefficient associated with MPL in Equation (2.1). As we include cohort fixed effects, β is identified using *within*-cohort variation by comparing MPL borrowers to their benchmark borrowers.

We find that the credit utilization ratios of MPL borrowers decline in the first few months after the loan take-up relative to the benchmark unmet and bank borrowers but then steadily increases thereafter. This is consistent with credit card debt consolidation occurring shortly after loan take-up, as seen in Figure 2.1. Columns (1) and (2) also show that the economic significance of the pre-trends is small, which demonstrates the efficacy of our matching procedure to construct cohorts. Column (1) shows that two years after the loan take-up, MPL borrowers' credit utilization has steadily increased and is 1.21 pp lower than that of the unmet borrowers, who could not obtain a loan and consolidate their debt. Column (2) shows that, relative to bank borrowers, MPL borrowers have a slightly greater utilization ratio (0.38 pp) two years after their loan take-up.

Table 2.3: Impact of MPL loans on borrowing capacities

This table presents the evolution of MPL borrowers' borrowing capacities in the months surrounding MPL loan origination. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) present the evolution of credit card utilization ratios, credit scores, and credit card limits, respectively. Columns (1), (3), and (5) (Columns (2), (4), and (6)) report the regression results for the change in borrowing capacities for the MPL borrowers relative to the matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	CC Utilization		Credit Scores		Log(CC Limits)	
Monthly DID	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period						
-12	0.049	-1.157***	-1.597***	-3.434***	-4.110***	-3.646***
	(0.084)	(0.099)	(0.286)	(0.261)	(0.157)	(0.155)
-6	0.043	-0.638***	-0.943***	-2.632***	-1.887***	-1.534***
	(0.059)	(0.053)	(0.126)	(0.169)	(0.070)	(0.088)
-3	-0.286***	-0.159***	-0.172	-1.720***	-0.948***	-0.498***
	(0.030)	(0.045)	(0.124)	(0.127)	(0.153)	(0.070)
-2	-0.143***	-0.059*	0.215***	-1.084***	-0.443***	-0.236***
	(0.024)	(0.031)	(0.075)	(0.110)	(0.085)	(0.041)
Post-period						
$+0$	-8.251***	-1.632***	12.493***	1.025**	0.247	0.122*
	(0.518)	(0.359)	(0.656)	(0.499)	(0.164)	(0.068)
$+1$	-26.940***	-6.103***	38.090***	13.029***	0.255	0.448***
	(0.565)	(0.425)	(0.878)	(1.062)	(0.672)	(0.111)
$+2$	-25.375***	-5.037***	37.365***	12.354***	2.649***	1.460***
	(0.468)	(0.441)	(0.994)	(0.977)	(0.848)	(0.147)
$+3$	-21.586***	-4.001***	27.472***	3.770***	5.560***	2.582***
	(0.466)	(0.428)	(0.716)	(0.745)	(0.778)	(0.187)
$+6$	-13.118***	-1.686***	18.284***	0.131	11.211***	4.630***
	(0.462)	(0.408)	(0.724)	(0.727)	(0.799)	(0.267)
$+12$	-5.435***	0.450*	7.143***	-4.050***	15.790***	6.094***
	(0.296)	(0.255)	(0.820)	(0.745)	(0.816)	(0.351)
$+18$	-2.330***	0.514**	1.798**	-5.527***	16.110***	6.028***
	(0.216)	(0.230)	(0.870)	(0.713)	(0.791)	(0.453)
$+24$	-1.208***	0.382*	-2.153***	-6.538***	15.337***	5.848***
	(0.173)	(0.217)	(0.725)	(0.629)	(0.878)	(0.573)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
# Cohorts	347,172	118,148	347,172	118,148	347,172	118,148
Avg. Adj. R^2	0.25	0.243	0.171	0.175	0.231	0.201

Credit Scores

Next, in Table 2.3, Columns (3) and (4), we analyze the effect of obtaining an MPL loan on the borrower's credit scores. We estimate the baseline specification in Equation (2.1) with the change in credit score as the dependent variable, and report the coefficient associated with MPL. We find that, relative to the unmet borrowers who did not obtain a loan, the credit scores of the MPL borrowers sharply increased by 38 points after obtaining an MPL loan. To put the magnitude of this credit score increase in context, we find that 39.7% of the MPL borrowers jump from a lower credit category (e.g., subprime, near-prime) to a higher credit category (e.g., prime) shortly after obtaining their MPL loan.[20] Similarly, the 13-point credit score increase of the MPL borrowers relative to the bank borrowers allowed 8.9% ($36.2 - 27.3$) more MPL borrowers to jump from a lower credit category to a higher credit category.

However, the credit scores of MPL borrowers steadily decrease over time relative to their benchmark borrowers. Two years after loan origination, the MPL borrowers' credit scores are about 2 points and 7 points lower than the benchmark unmet and bank borrowers, respectively.

Credit limits

Columns (5) and (6) of Table 2.3 show that the credit limits of the MPL borrowers increase after MPL loan take-up relative to the benchmark unmet and bank borrowers. We estimate the baseline specification in Equation (2.1) with the log change in total credit card limits as the dependent variable, and report the coefficients associated with MPL. For each month, the total credit limit for an individual is computed by aggregating the credit limits across all the credit cards of an individual. Thus, this definition captures credit limit changes on both the intensive and the extensive margin.

Column (5) of Table 2.3 shows that the credit limits of MPL borrowers increase monotonically

[20]Subprime, near-prime, and prime consumers have credit scores in the range 300–619, 620–679, and 680–850, respectively. Among the MPL borrowers in the unmet cohort, 25.8% jump from the near-prime to the prime category, 9.3% jump from the subprime to the near-prime category, and 4.6% jump from the subprime to the prime category.

over time relative to the unmet borrower within the first 18 months after obtaining the MPL loan. Two years after the MPL loan take-up, MPL borrowers have 15.3% (or $2,740) higher total credit limits than their benchmark unmet borrowers. Column (6) presents similar evidence for the bank cohort. Credit limits for the MPL borrowers increase monotonically relative to the bank borrowers over the first year after obtaining the MPL loan, and decrease thereafter. Two years after MPL loan take-up, the MPL borrowers have 5.8% (or $1,247) higher total credit limits than their benchmark bank borrowers.

Overall, the results in Table 2.3 show that after obtaining an MPL loan, the borrowing capacities of the MPL borrowers increase.

How does MPL Borrowing Impact Consumers' Borrowing Outcomes?

In this subsection, we examine the effect of MPL loan take-up on the borrower's future borrowing outcomes using our baseline regression framework. It is important to note that greater borrowing capacities need not necessarily result in greater borrowing. For instance, if the increase in borrowing capacity (e.g., credit limits) is supply driven (e.g., automatic credit limit increases by banks), then under the permanent income hypothesis, it should not lead to more borrowing. Similarly, borrowers with credit score improvements may apply for higher credit limits without intending to borrow on them immediately, but instead to build greater buffers to deal with future liquidity shocks (e.g., the buffer stock hypothesis of [75]).

On the other hand, if borrowers are credit constrained, then greater borrowing capacities can lead to more immediate borrowing ([48]). Further, if borrowing allows consumers to manage temporary liquidity shocks without having to default, then having greater borrowing capacities should reduce defaults. However, if consumers with a higher default likelihood are more likely to build borrowing capacities and then borrow more, then greater borrowing capacities should lead to more defaults.

Credit card debt

Table 2.4, Columns (1) and (2), show how credit card balances change over time for the MPL borrowers relative to the unmet and bank borrowers, respectively. We estimate the baseline specification in Equation (2.1) with the log change in total credit card balances as the dependent variable, and report the coefficients associated with MPL.

After consolidating their credit card debt in the first two months after the MPL loan take-up, the MPL borrowers subsequently accumulate 13.8% and 8.6% higher credit card balances over the next two years relative to their benchmark unmet and bank borrowers, respectively.[21] More importantly, as the credit card balance trends in Panel A of Figure 2.2 show, the MPL borrowers accumulate these credit balances at a faster rate than the bank borrowers. This trend suggests that the MPL borrowers have a greater propensity to consume out of credit card liquidity than bank borrowers.

Total debt

Table 2.4, Columns (3) and (4), present the baseline regression results for the total debt balances of the unmet cohort and bank cohort, respectively. Total debt balances are computed by aggregating a consumer's debt across all her debt accounts. In the post period (i.e., the period after loan origination), this aggregation also includes the new installment loan issued to the MPL borrower and the bank borrower.[22]

First, Columns (3) and (4) show a sharp increase in total debt immediately after loan origination. This suggests that both the MPL borrowers and bank borrowers use only a part of their loan proceeds to consolidate their debt. This trend can also be seen in the Figure B.1, which plots the monthly trends for the total debt balance. On average, MPL borrowers and bank borrowers use

[21] Strictly speaking, the coefficient estimates should be interpreted in log point changes. Thus, a -100 log point change will translate to a -63.2% ($e^{-1} - 1 = 0.632$) change. However, for ease of stating the results, we interpret log point changes as percentage changes.

[22] In general, we find that both the bank and MPL installment loans are reported to the credit bureau with a delay of 1–3 months. We correct for this delay before computing the total debt balances to ensure that any given month reflects the true total debt balances in that month.

Table 2.4: Impact of MPL loans on future borrowing

This table presents the evolution of MPL borrowers' overall debt levels in the months surrounding MPL loan origination. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) present the evolution of credit card balances, total balances, and debt-to-income ratios, respectively. Columns (1), (3), and (5) (Columns (2), (4), and (6)) report the regression results for the change in the borrowing outcomes for the MPL borrowers relative to the matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Log(CC Balance)		Log(Total Debt)		DTI Ratio	
Monthly DID	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period						
-12	1.333***	4.092***	-2.647***	-2.135***	0.049	-0.328***
	(0.251)	(0.504)	(0.222)	(0.241)	(0.076)	(0.080)
-6	0.293*	1.765***	-1.250***	-1.300***	0.040	-0.307***
	(0.159)	(0.344)	(0.184)	(0.190)	(0.059)	(0.067)
-3	-0.472*	0.176	-0.518**	-0.718***	-0.046	-0.138***
	(0.251)	(0.140)	(0.228)	(0.232)	(0.042)	(0.042)
-2	0.017	0.296**	-0.223	-0.369***	-0.085**	-0.106***
	(0.145)	(0.130)	(0.162)	(0.137)	(0.033)	(0.035)
Post-period						
$+0$	-25.652***	-3.392**	22.949***	3.532***	10.483***	1.023***
	(1.702)	(1.486)	(0.213)	(0.205)	(0.107)	(0.164)
$+1$	-102.500***	-14.100***	17.763***	3.954***	6.371***	2.531***
	(1.986)	(2.387)	(0.428)	(0.215)	(0.107)	(0.099)
$+2$	-89.371***	-7.326***	18.596***	4.938***	5.925***	3.300***
	(2.313)	(2.182)	(0.663)	(0.223)	(0.122)	(0.117)
$+3$	-65.679***	-1.883	19.665***	5.650***	5.740***	3.228***
	(2.203)	(2.044)	(0.660)	(0.272)	(0.148)	(0.129)
$+6$	-25.104***	7.243***	20.960***	5.511***	6.336***	3.617***
	(1.647)	(1.533)	(0.657)	(0.264)	(0.219)	(0.117)
$+12$	3.126***	11.670***	22.173***	6.231***	9.546***	4.947***
	(1.128)	(0.994)	(0.612)	(0.370)	(0.282)	(0.199)
$+18$	11.800***	10.433***	21.419***	5.943***	11.454***	5.931***
	(1.102)	(0.924)	(0.663)	(0.411)	(0.299)	(0.233)
$+24$	13.842***	8.649***	20.279***	5.868***	11.944***	5.808***
	(1.030)	(1.058)	(0.791)	(0.516)	(0.217)	(0.203)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
# Cohorts	347,172	118,148	347,172	118,148	347,172	118,148
Avg. Adj. R^2	0.171	0.149	0.246	0.277	0.185	0.196

43.6% and 48.3% of their loan proceeds to consolidate their debt, respectively. This implies that MPL borrowers use most of their loan proceeds for consumption purposes rather than for debt consolidation, even though the MPL borrowers' primary loan purpose stated in their loan application is debt consolidation. The results in Columns (3) and (4) show that two years after the loan origination, MPL borrowers have 20.3% and 5.9% greater debt than their benchmark unmet and bank borrowers, respectively.

Further, Columns (3) and (4) show that the total debt of the MPL borrowers relative to their benchmark borrowers remains relatively flat in the post–loan origination period. This contrasts with the total credit card debt trends shown in Columns (1) and (2), which drop to their lowest point in month $+1$ and increase steeply thereafter until month $+24$. For instance, the dollar change in total debt for the MPL borrower relative to the unmet borrower is \$5,667 between month $+1$ and month $+24$. In comparison, the dollar change in total credit card debt for the MPL borrower relative to the unmet borrower over the same period is \$5,314, which is greater. These numbers suggest that the MPL borrowers substituted, on average, about \$353 ($5667 - 5314$) of their other outstanding debt with credit card debt over time by borrowing out of credit card liquidity.

Debt-to-income ratio

Next, we present regression results for the changes in monthly DTI ratio over time in Table 2.4, Columns (5) and (6). The DTI ratio is the ratio of the total monthly required debt payment relative to the borrower's monthly income. The total required monthly debt payments are the aggregate monthly payments that a borrower is required to make across all her debt accounts to remain non-delinquent. Borrowers with larger DTI ratios are arguably more liquidity constrained because they have to set aside a larger fraction of their monthly income for debt repayment.[23] Thus, changes in DTI ratio can indicate how the liquidity constraints of borrowers change over time.

[23]For instance, a common cutoff for the DTI ratio in the lending market is 43%, beyond which credit is denied more often. See https://www.consumerfinance.gov/ask-cfpb/what-is-a-debt-to-income-ratio-why-is-the-43-debt-to-income-ratio-important-en-1791/.

The results in Columns (5) and (6) show that the DTI ratio of the MPL borrower relative to the matched unmet and bank borrowers steadily increases over time. Two years after loan origination, the MPL borrower has a 11.9% and 5.8% higher DTI ratio relative to the benchmark unmet and bank borrower, respectively. We find similar results if we regress the change in total monthly debt payment (i.e., the numerator in the DTI ratio) instead of the change in DTI ratio (see Table B.1). This mitigates the concern that income growth might be driving our results. We find that two years after the MPL loan origination, the MPL borrower has a 30.1% and a 12.7% higher monthly debt payment relative to the benchmark unmet and bank borrowers, respectively.

Overall, the future borrowing trends of MPL borrowers suggests that they are more likely to borrow out of credit card liquidity, more likely substitute other debt with credit card debt, and more likely to be more liquidity constrained. This use of credit card debt by the MPL borrowers is consistent with prior studies, which suggest that liquidity-constrained consumers are more willing to take on high interest rate debt (e.g., credit card debt), if the debt has a long maturity ([76]).

Defaults

Columns (1) and (2) of Table 2.5 show the regression results of defaults across all kinds of debt using our baseline specification in Equation (2.1) for the unmet and bank cohorts. We find that, two years after the MPL loan origination, MPL borrowers default 1.13 pp and 0.72 pp more than their benchmark unmet and bank borrowers, respectively. These point estimates suggest that MPL borrowers default 24% and 20% more than their benchmark unmet and bank borrowers, respectively, when compared to the average 2-year default rate of 4.75% for the unmet cohort and 3.53% for the bank cohort. We find similar results for credit card defaults (Columns (3) and (4)) and non–credit card defaults (Columns (5) and (6)). Overall, the evidence on defaults in Table 2.5 suggests that MPL borrowers default at a higher rate than their benchmark counterfactual borrowers across *all* kinds of debt.

Table 2.5: Impact of MPL loans on default rates

This table presents the evolution of MPL borrowers' default rates in the months surrounding MPL loan origination. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) present the evolution of defaults on any kind of debt, defaults on credit cards, and defaults on non–credit card products, respectively. Columns (1), (3), and (5) (Columns (2), (4), and (6)) report the regression results for the change in the default rates for the MPL borrowers relative to the matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	All Defaults		CC Defaults		Non-CC Defaults	
Monthly DID	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period						
-12	0.188***	0.185***	0.013	0.030	0.182***	0.188***
	(0.034)	(0.042)	(0.021)	(0.022)	(0.033)	(0.041)
-6	0.092***	0.084	0.042**	0.039**	0.088***	0.086*
	(0.027)	(0.052)	(0.020)	(0.020)	(0.027)	(0.051)
-3	-0.026	0.049**	-0.033*	0.031**	0.049**	0.035
	(0.026)	(0.023)	(0.018)	(0.016)	(0.023)	(0.022)
-2	-0.038**	0.008	-0.024**	0.004	0.011	0.016
	(0.017)	(0.020)	(0.012)	(0.013)	(0.018)	(0.018)
Post-period						
$+0$	-0.249***	0.003	-0.104***	0.003	-0.141***	0.005
	(0.030)	(0.020)	(0.012)	(0.009)	(0.027)	(0.020)
$+1$	-0.502***	-0.010	-0.253***	0.011	-0.276***	-0.016
	(0.051)	(0.034)	(0.016)	(0.016)	(0.042)	(0.030)
$+2$	-1.128***	-0.032	-0.669***	-0.041*	-0.605***	0.012
	(0.069)	(0.033)	(0.038)	(0.022)	(0.048)	(0.030)
$+3$	-1.785***	-0.096***	-1.157***	-0.067**	-0.989***	-0.046*
	(0.095)	(0.030)	(0.064)	(0.030)	(0.060)	(0.025)
$+6$	-2.395***	-0.347***	-1.918***	-0.225***	-1.317***	-0.231***
	(0.127)	(0.090)	(0.107)	(0.052)	(0.073)	(0.072)
$+12$	-0.806***	0.229	-0.646***	0.230*	-0.351***	0.081
	(0.177)	(0.144)	(0.123)	(0.124)	(0.117)	(0.086)
$+18$	0.832***	0.763***	0.650***	0.755***	0.549***	0.399***
	(0.176)	(0.135)	(0.138)	(0.114)	(0.118)	(0.088)
$+24$	1.127***	0.724***	0.791***	0.547***	0.812***	0.473***
	(0.132)	(0.131)	(0.097)	(0.090)	(0.093)	(0.100)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
# Cohorts	347,172	118,148	347,172	118,148	347,172	118,148
Avg. Adj. R^2	0.105	0.097	0.085	0.082	0.029	0.042

Robustness checks

In this section, we test the robustness of our baseline findings. In Table B.2 and Table B.3, we show that our baseline results are not dependent on our matching procedure. In these tables, we consider a 5% random sample of all U.S. borrowers and we re-run our baseline analysis without matching MPL and non-MPL borrowers. We use this random sample to estimate our baseline Equation (2.1) with the same controls. However, we replace cohort fixed effects with fixed effects at the 5-digit ZIP code level, the 5-point credit score bin level, and the loan origination year-month level.[24] Our modified baseline specification effectively compares MPL borrowers with non-MPL borrowers within the same 5-digit ZIP code, with similar credit scores, and who apply for their loans in the same year-month.

Our baseline point estimates for both borrowing capacities and outcomes are largely unchanged (in both sign and economic magnitude) in the unmatched sample analysis. For example, our baseline matched cohort analysis estimates suggest that MPL borrowers' credit scores are 2–7 points lower than their benchmark non-MPL borrowers two years after MPL loan take-up. In comparison, the corresponding estimates from our unmatched sample analysis are 3–9 points. Despite the similarity in estimates provided by the matched cohort analysis and the unmatched sample analysis, we prefer the former in principle because it accounts for any nonlinear dependence of the outcome variable on the matching variables. This is important given that we do not observe the functional form of a lender's credit model.

Our baseline results are also robust to controlling for loan terms and are presented in Table B.4–Table B.6. Recall that we can control for loan terms only in our bank cohort analysis because the unmet borrowers do not obtain loans. We distinguish between non-price loan terms (e.g., loan amount, maturity) and price loan terms (e.g., interest rates) in our analysis for the reasons stated below and include them as controls sequentially. The odd-numbered columns in Table B.4–Table B.6

[24]The 5% random sample contains 33,482 MPL borrowers, 106,974 bank borrowers, and 401,923 unmet borrowers with non-missing credit data for regression analysis.

control for non-price loan terms, while the even-numbered columns control for both non-price and price loan terms. The sample size for these tests is lower by 29% due to missing loan terms.

We first add non-price loan terms such as loan amount and maturity to our baseline regression specification. These controls broadly serve two purposes. First, they control for the effect of non-price loan terms on future borrowing capacities and outcomes. For instance, a higher loan amount today can reduce a borrower's future borrowing capacities and also increase future defaults. Second, lenders can also use non-price loan terms to screen borrowers. For unsecured debt, such as the MPL and bank installment loans in our sample, lenders can screen borrowers on only two non-price loan terms – loan amount ([77, 52]) and loan maturity ([78, 53]). We find that our results are stronger compared to the baseline results after controlling for non-price loan terms.

The loan interest rate can also affect future borrowing capacities and outcomes. Additionally, the interest rate is also related to adverse selection ([79]), which is precisely the effect our analysis attempts to capture. That is, conditional on observable borrower characteristics and non-price loan terms, higher interest rates will attract higher default risk borrowers in the absence of adequate screening by lenders. Consistent with this notion, the results after controlling for interest rates are weaker compared to our baseline results.

2.5 Economic channel and interpretation of the results

The previous sections document two main results. First, the borrowing capacities and outcomes of MPL borrowers improve shortly after obtaining the MPL loan relative to their counterfactual benchmark borrowers. Second, after this initial improvement, the borrowing capacities and outcomes of MPL borrowers deteriorate over time and become worse in the long run when compared to their benchmark borrowers.

The short-run and long-run responses to obtaining an MPL loan appear to be in discord. For instance, the immediate increase in credit scores and the willingness of lenders to provide more credit to MPL borrowers indicate that obtaining an MPL loan signals that the borrower is of a higher credit quality than what is suggested by the borrower's observable characteristics before the MPL loan. This evidence is consistent with MPL lenders screening their borrowers before loan approval. Further, as their borrowing capacities improve relative to the benchmark bank borrowers, it suggests that MPL lenders screen their borrowers better than banks.

On the other hand, the ex post worse repayment outcomes compared to bank borrowers indicate that MPL lenders screen their borrowers poorly compared to banks. In this section we investigate the underlying economic mechanisms that can help rationalize these discordant effects of obtaining MPL credit.

2.5.1 Information frictions and default patterns

We begin by analyzing the results that document the ex post higher default rates of the MPL borrowers relative to their benchmarks. Specifically, we ask whether greater information frictions between lenders and borrowers, such as adverse selection or moral hazard, can explain the higher default rates of MPL borrowers relative to bank borrowers.

In general, the potential for both adverse selection and moral hazard in the MPL markets can be high. Borrowers in the MPL markets are dispersed, anonymous, and distant from their lenders. These factors make it challenging to screen and monitor borrowers ([80]). However, MPL platforms and investors can use sophisticated credit models and rely on a feedback or reputation system to mitigate the effects of adverse selection and moral hazard ([81, 50, 82]).

It is also important to distinguish between adverse selection and moral hazard effects. Adverse selection suggests that borrowers with a higher default risk are also more likely to obtain MPL loans than bank loans because MPL lenders screen their borrowers poorly compared to banks. In contrast, moral hazard suggests that even MPL borrowers who are ex ante identical to bank borrowers will default at a higher rate. This higher default rate can occur either because MPL lenders monitor their borrowers less intensively than banks, or because MPL borrowers face different contract terms than bank borrowers. For instance, Table 2.1, Panel C, shows that MPL loans are, on average, larger than bank loans even for observably similar MPL and bank borrowers. This difference can lead to more defaults on MPL loans due to the moral hazard associated with the repayment of more debt.

Typically, it is difficult to separate adverse selection from moral hazard because the adverse selection of borrowers occurs based on information that is privately observable to the borrowers. However, we believe that our default results are more consistent with the adverse selection effect, which suggests that MPL lenders screen poorly compared to banks. First, the default rates of MPL borrowers are greater than bank borrowers even after controlling for the loan contract terms in our analysis. Next, the default rates of MPL borrowers are higher across all debt accounts, which is arguably a mix of various loan terms (e.g., loan size, interest rate, maturity, collateralization, lender monitoring intensity). Thus, the relatively higher default rates of MPL borrowers across all their debts seem to be more indicative of their generally poorer credit quality (i.e., adverse selection) rather than the kind of loan terms available to them (i.e., moral hazard).

Further, we also find that MPL borrowers' default rate on credit card debt is significantly higher than that of bank borrowers. Credit cards typically have standardized terms across lenders – they are unsecured, have sticky and similar interest rates, and require similar minimum monthly payments ([83, 54]). Thus, given the similarity of credit card terms, the higher credit card default rates of MPL borrowers more likely indicate their poorer credit quality. We also find that lower credit score borrowers are more likely to default than higher credit score borrowers two years after the MPL loan take-up (see Table B.10). This evidence is again consistent with adverse selection as adverse selection problems, and hence the benefits of screening, are likely greater for lower credit score borrowers than for higher credit score borrowers.

In the subsequent subsections we investigate the economic nature of the private information based on which the adverse selection of MPL borrowers occurs. The evidence in Section 2.4.3 suggests that MPL borrowers and their counterfactual benchmarks differ on at least two dimensions. First, the MPL borrowers have a greater propensity to borrow out of liquidity. Second, the MPL borrowers consolidate less of their credit card debt using their loan proceeds. In the subsequent subsections, we investigate whether potential adverse selection related to these factors can account for the default patterns observed between the MPL borrowers and their counterfactual benchmarks.

Adverse selection on liquidity constraints

We begin by analyzing the potential adverse selection of MPL borrowers associated with their greater propensity to borrow out of liquidity and then default at higher rates than their benchmarks. This adverse selection effect can occur if some MPL borrowers strategically increase their borrowing because they anticipate defaulting in the future. Alternatively, it could also occur because some MPL borrowers always tend to be more credit or liquidity constrained, and they default because they cannot maintain their debt repayments. We can ascertain which of the above two factors drives MPL borrowers' defaults by observing these borrowers' future behavior and outcomes.

To examine whether higher total debt levels or liquidity constraints explain our default patterns, Table 2.6 re-estimates the default regressions for the unmet and bank cohorts after controlling for contemporaneous changes in liquidity constraints and total debt obligations over time. We proxy for borrower liquidity constraints using the borrower's monthly DTI ratio, which captures the borrower's monthly debt payment relative to her monthly income. We compute a borrower's total debt by aggregating across all the borrower's debt accounts.

Table 2.6, Column (1) controls for contemporaneous changes in total debt obligations, and Table 2.6, Column (2) controls for contemporaneous changes in liquidity constraints for the default regressions. A comparison of the regression estimates in Columns (1) and (2) of Table 2.6 with the estimates shown in Table 2.5, Column (1) (i.e., without the debt controls) suggests that changes in liquidity constraints, as opposed to changes in total debt, are better able to explain the diverging default trends between the MPL and the benchmark unmet borrowers. Similarly, Table 2.6, Columns (3) and (4) show that liquidity constraints are also better able to explain the default trends between the MPL borrowers and the benchmark bank borrowers. We find similar results when we analyze

Table 2.6: Adverse selection on liquidity constraints

This table presents results that examine the role of different measures of borrower indebtedness in explaining trends in credit card defaults. Columns (1)–(2) and Columns (3)–(4) present the results for the unmet cohort and the bank cohort, respectively. Columns (1) and (3) (Columns (2) and (4)) report the regression results that control for contemporaneous change in total debt (contemporaneous change in debt-to-income ratio). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

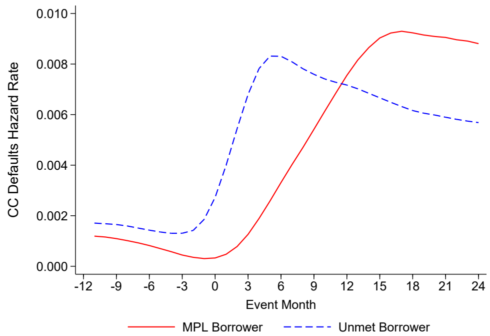
	Dependent Variable: All Defaults			
Monthly DID	Unmet Cohort		Bank Cohort	
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)
Pre-period				
-12	0.172***	0.188***	0.175***	0.185***
	(0.034)	(0.034)	(0.042)	(0.042)
-6	0.085***	0.092***	0.081	0.084
	(0.027)	(0.027)	(0.051)	(0.052)
-3	-0.026	-0.022	0.049**	0.051**
	(0.026)	(0.026)	(0.023)	(0.023)
-2	-0.039**	-0.035**	0.008	0.008
	(0.017)	(0.017)	(0.021)	(0.021)
Post-period				
$+0$	-0.263***	-0.944***	-0.001	0.0004
	(0.033)	(0.058)	(0.020)	(0.020)
$+1$	-0.452***	-1.179***	-0.013	-0.055
	(0.052)	(0.077)	(0.034)	(0.035)
$+2$	-1.031***	-2.210***	-0.037	-0.191***
	(0.068)	(0.114)	(0.034)	(0.039)
$+3$	-1.648***	-3.255***	-0.095***	-0.385***
	(0.095)	(0.141)	(0.032)	(0.034)
$+6$	-2.081***	-4.565***	-0.323***	-1.130***
	(0.117)	(0.158)	(0.090)	(0.085)
$+12$	-0.526***	-4.021***	0.286*	-1.203***
	(0.188)	(0.084)	(0.146)	(0.083)
$+18$	1.123***	-2.901***	0.823***	-1.210***
	(0.182)	(0.080)	(0.136)	(0.091)
$+24$	1.344***	-2.568***	0.781***	-1.040***
	(0.133)	(0.092)	(0.131)	(0.122)
Matching Controls	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
ΔTotal Debt $(-1, t)$	✓		✓	
ΔDTI Ratio $(-1, t)$		✓		✓
# Cohorts	347,172	347,172	118,148	118,148
Avg. Adj. R^2	0.106	0.196	0.097	0.184

defaults on credit cards and non–credit card products separately, and report our findings in Table B.7. Overall, the results in Table 2.6 are consistent with the adverse selection of MPL borrowers on the dimension of liquidity constraints relative to their benchmark borrowers.

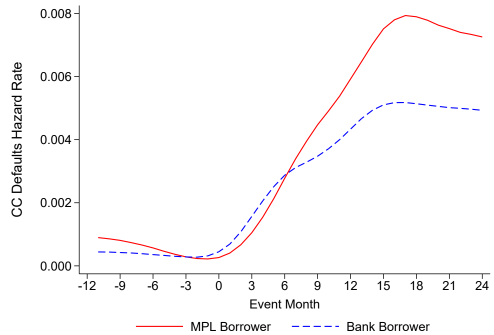
Further, Figure 2.3 plots the hazard rates of defaults to shed more light on the timing and rate of defaults.[25] The default hazard rate for unmet borrowers peaks shortly after they are unable to obtain credit. Interestingly, the MPL borrower's hazard rate also peaks around the same level as that of the unmet borrower, but about 12 months later – i.e., between month 15 and 18 after obtaining the MPL loan. In comparison, the hazard rates for the bank borrowers always remain lower than both unmet and MPL borrowers. Overall, these patterns indicate that providing an MPL loan only temporarily relaxes the MPL borrower's liquidity constraints and only delays her eventual default.

[25]The hazard rate at month t is defined as the number of loans that enter delinquency in month t as a fraction of the number of non-delinquent loans in month $t-1$.

Panel A: Credit Cards

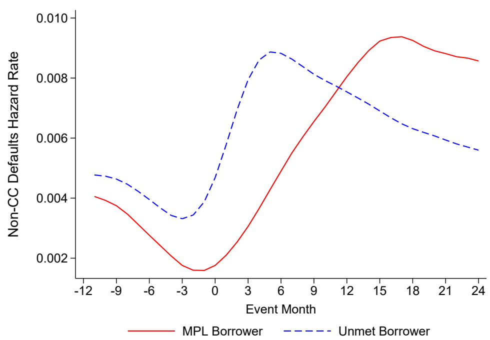


Unmet Cohort

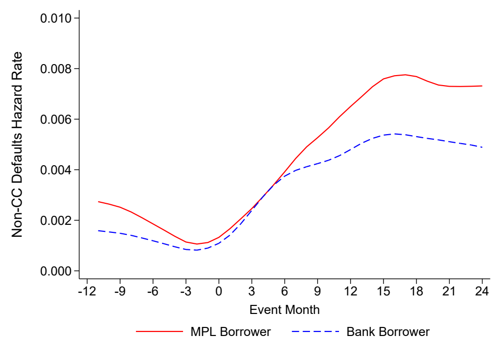


Bank Cohort

Panel B: Non-Credit Cards



Unmet Cohort



Bank Cohort

Figure 2.3: Default hazard rates

This figure presents the default hazard rates for unmet and bank cohorts. The default hazard rate at month t is defined as the number of loans that enter delinquency in month t as a fraction of the number of non-delinquent loans in month $t-1$. Panel A and Panel B show default hazard rates for credit card debt, and non-credit card debt, respectively. In all panels, the x-axis displays event time relative to the month of loan origination and the y-axis represents default hazard rate.

Adverse selection on debt consolidation

While debt consolidation is one of the primary stated loan purposes of MPL borrowers, the MPL lenders cannot ensure that borrowers will use their MPL loans for their stated purpose. For instance, borrowers who use their loan proceeds for consumption of goods rather than for debt consolidation can end up with higher debt levels, which can lead to higher defaults. Such information asymmetries between the MPL lenders and borrowers can potentially lead to adverse selection problems. Therefore, we analyze the potential adverse selection of the MPL borrowers on this second dimension since our results show that MPL borrowers consolidate less debt than bank borrowers.

A commitment to consolidate debt can signal positively about a borrower's credit quality during the loan application phase. Debt consolidation can simplify multiple payments that are spread across several debt accounts into a single payment for the consolidated debt account. Thus, debt consolidation can signal a borrower's commitment to keep track of their debt obligations and avoid mistakes such as missed payments. In the specific case of credit card debt consolidation, borrowers can also signal a commitment to pay off their debt more quickly because personal installment loans typically have a fixed and shorter maturity (e.g., three or five years) compared to credit cards.[26]

Consequently, we examine whether a borrower's degree of debt consolidation shortly after their loan take-up is predictive of their future creditworthiness. Further, if banks screen their borrowers better than MPL lenders, then debt consolidation should be less predictive of future creditworthiness for the bank borrowers than the MPL borrowers.

We measure a borrower's degree of debt consolidation by her consolidation ratio, which is the amount of loan proceeds the borrower uses for debt consolidation as a fraction of her loan amount. We measure the debt consolidation amount by subtracting the total change in debt in the monthly

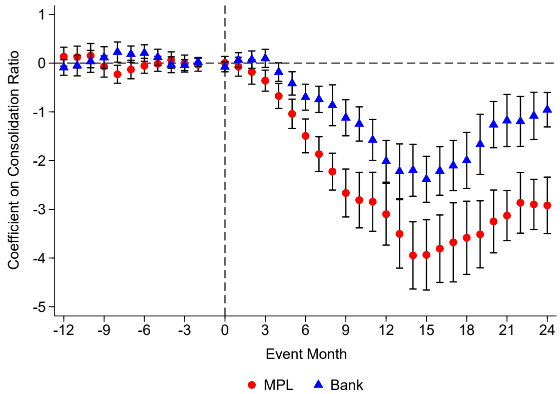
[26]Perhaps this is why LendingClub, a large MPL lender, introduced a new feature in June 2019 that allows LendingClub to pay off their borrowers' credit cards directly without disbursing the loan proceeds to the borrower. Moreover, consistent with being able screen borrowers and mitigate adverse selection using this new feature, LendingClub offers lower interest rates for borrowers who opt for this feature. Source: `https://www.americanbanker.com/news/new-lendingclub-feature-lets-customers-pay-off-card-debt-directly`

interval $(-1,+1)$ from the loan amount, where month 0 is the loan origination month.[27] We re-estimate the default regressions in Table 2.5, Column (1) and Column (2), after including the consolidation ratio and its interaction with the *MPL* indicator variable. Then we plot the coefficients associated with consolidation ratio for MPL and bank borrowers in Figure 2.4, Panel A.

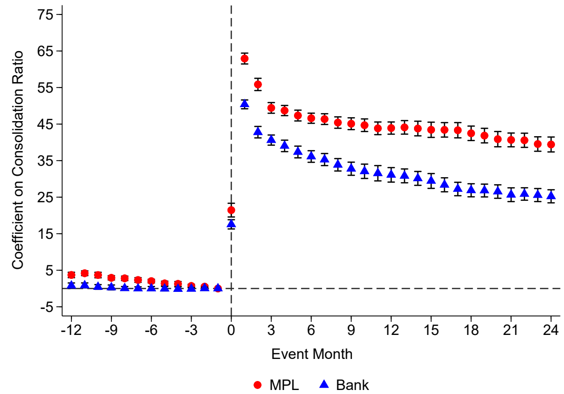
First, the negative and significant coefficients in Figure 2.4, Panel A, show that a higher consolidation ratio immediately after obtaining a loan predicts lower future defaults. Moreover, the consolidation ratio is more predictive of defaults for MPL borrowers than bank borrowers. The figure indicates that a 10 pp increase in the consolidation ratio is associated with a 0.1 pp reduction in the defaults of bank borrowers two years after loan origination (equivalent to a 4.7% reduction relative to the mean default rate of the benchmark bank borrowers). In contrast, a 10 pp increase in the consolidation ratio suggests a 0.3 pp reduction in the defaults of the MPL borrowers two years after the loan origination (equivalent to a 9.1% reduction relative to the mean default rate of the MPL borrowers). Figure 2.4, Panel C shows that the difference between the MPL and bank consolidation ratio coefficients is statistically significant and increasing over time post loan origination.

We find similar results in Figure 2.4, Panel B, when we use the change in future credit scores instead of default rates as a proxy for borrower quality. Two years after loan origination, a borrower who uses her entire loan proceeds to consolidate her debt (i.e., 100 pp increase in consolidation ratio) has a 25 (40) point increase in credit scores if she is a bank borrower (MPL borrower), relative to a borrower who uses none of the loan proceeds to consolidate debt.

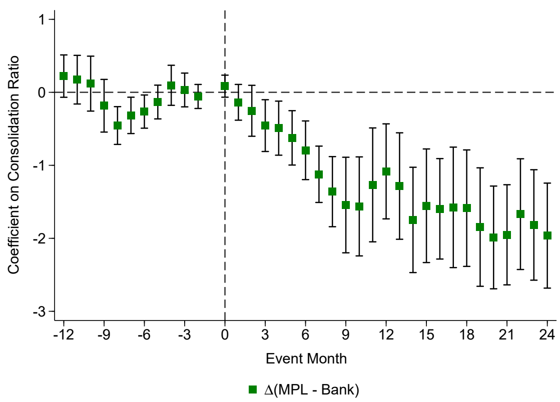
[27] That is, the consolidation ratio is defined as $1 - \frac{\Delta TotalDebt(-1,+1)}{LoanAmount}$



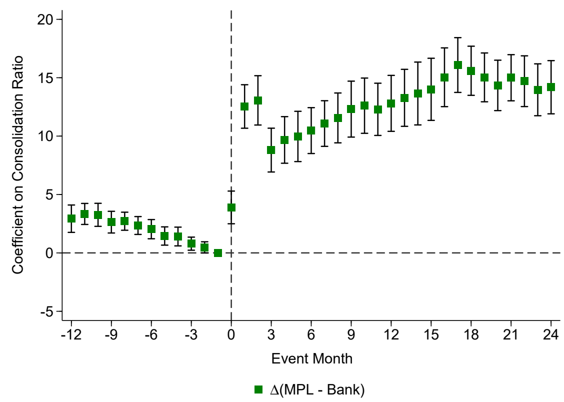
(a) Default Regressions



(b) Credit Score Regressions



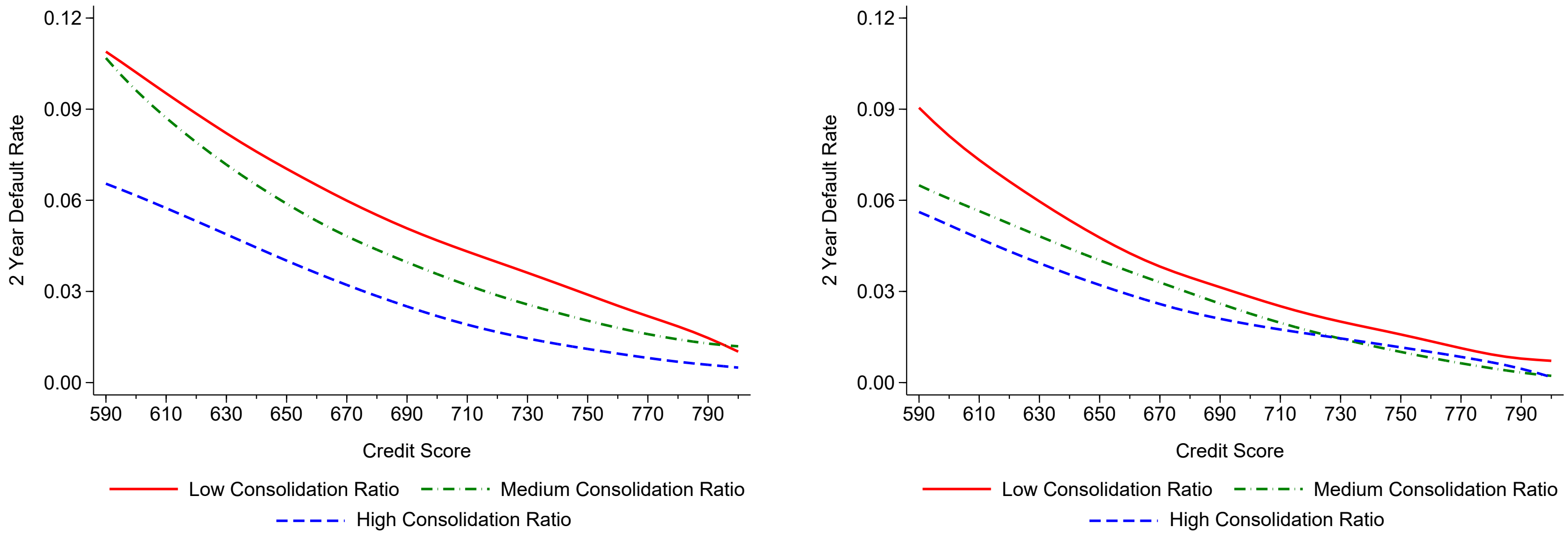
(c) Default Regressions



(d) Credit Score Regressions

Figure 2.4: Adverse selection on debt consolidation

The figure presents the coefficient associated with consolidation ratio for the MPL and bank borrowers. Consolidation ratio is the proportion of the loan amount used by a borrower for debt consolidation within the first two months after loan origination. These coefficients are estimated by including consolidation ratio and its interaction term with the MPL indicator to our baseline specification for loan default rate (Panel A) and future credit scores (Panel B). In all panels, the x-axis displays event time relative to the month of loan origination and the y-axis represents the magnitude of the coefficient estimate. The x-axis in Panel A starts at month $+3$ because a borrower is considered to be in default only if the borrower is 90+ days delinquent.



(a) MPL Default Rate

(b) Bank Default Rate

Figure 2.5: Adverse selection on debt consolidation by credit score

The figure presents the two-year default rate at each credit score by consolidation ratio terciles. Consolidation ratio is the proportion of the loan amount used by a borrower for debt consolidation within the first two months after loan origination. Panel A and Panel B present the results for the MPL and bank borrowers, respectively. The default rate trends are lowess smoothed with a bandwidth of 0.5.

We provide further evidence that the consolidation ratio is related to adverse selection. At each credit score before loan origination, we sort borrowers into terciles based on their consolidation ratio. Figure 2.5 plots the average 2-year default rates of the MPL borrowers (Panel A) and the bank borrowers (Panel B) for each tercile across credit scores.[28] Figure 2.5 shows that the dispersion of the default rates across the consolidation ratio terciles is higher for borrowers with lower credit scores. This evidence is further consistent with adverse selection, because adverse selection problems (and hence the benefits of screening) are arguably greater for borrowers with lower credit scores than for borrowers with higher credit scores. Moreover, at each credit score level, the dispersion of default rates across the consolidation ratio terciles is higher for the MPL borrowers than for the bank borrowers. This evidence is again consistent with the weaker screening of the MPL borrowers relative to the bank borrowers.

Overall, the results in Figure 2.4 and Figure 2.5 are consistent with adverse selection related to debt consolidation. Moreover, these results also show that the consolidation ratio has greater explanatory power for the future defaults and credit scores for the MPL borrowers than bank borrowers, even though both are observably similar before the loan take-up. Thus, these results are consistent with MPL lenders screening poorly relative to banks on the borrowers' likelihood of debt consolidation after loan take-up.

2.5.2 What explains the immediate improvement in borrowing capacities and outcomes after MPL loan take-up?

Thus far, we have provided evidence suggesting that the adverse selection of MPL borrowers can explain their ex post default outcomes relative to bank borrowers. However, this adverse selection suggests that the immediate change in borrowing capacities of the MPL borrower should be worse than the bank borrowers, which is contrary to our empirical findings. In this section we investigate

[28] As defaults are rare, to reduce noise, we average the default rates within credit score bin sizes of 5 (e.g., 620–624, 625–629), as opposed to for every unique credit score value.

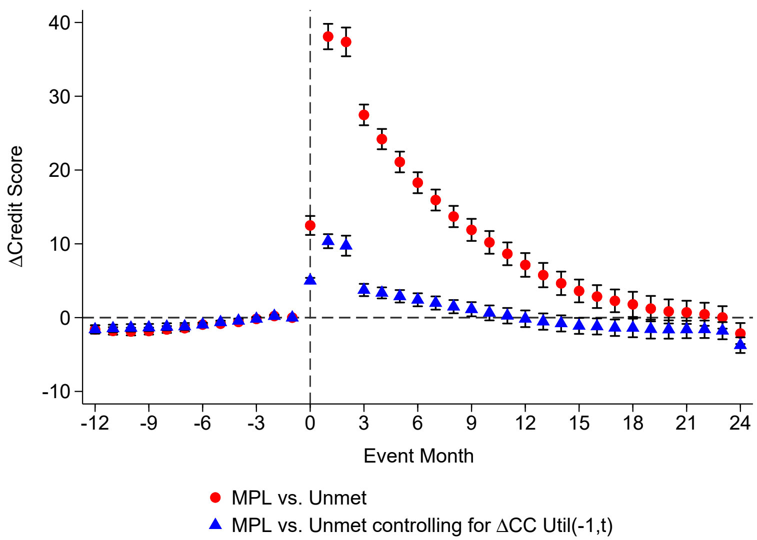
the explanation for the immediate effects of obtaining an MPL loan.

Immediate credit score jump

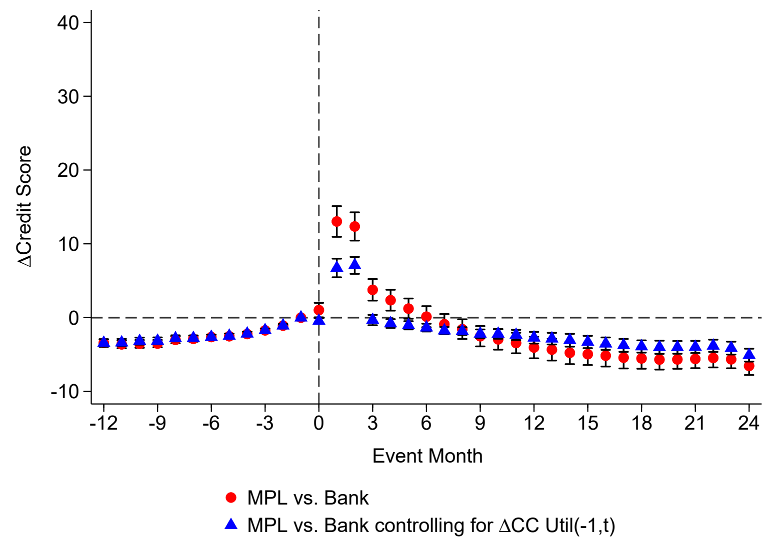
Recall that both bank installment loans and MPL loans are reported to credit bureaus after a delay. On average, bank installment loans are reported one month after origination, while MPL loans are reported three months after origination.[29] However, the credit scores of the MPL borrowers increase during the first two months after loan take-up (see Figure 2.1). Thus, the credit score jump for MPL borrowers is unlikely to be information driven, because during the period of the credit score jump, the information on loan origination is unavailable to the credit bureau or to other parties who rely on the credit bureau for such information.

Instead, we find that the immediate credit score jump in the first two months after the MPL loan take-up is somewhat “mechanically” related to the sharp decline in credit card utilization that occurs due to credit card debt consolidation. This can be gleaned from the raw trends of credit card utilization and credit scores shown in Figure 2.1, which are more or less mirror images of one another. We also formally test this mechanical relationship by adding the contemporaneous change in credit utilization as a control to our baseline credit score regression specifications in Table 2.3, Columns (3) and (4). We plot these regression estimates in Figure 2.6, and we report the coefficients in Table B.8.

[29] Despite this delay in reporting, the loans are recorded with their actual loan origination dates.



(A) Unmet Cohort



(B) Bank Cohort

Figure 2.6: Impact of credit card debt consolidation on credit scores

The figure presents regression plots that document the impact of credit card debt consolidation on consumer credit scores in the months around MPL loan origination. Panel A and Panel B plot regression estimates and their 95% confidence intervals for the unmet and bank cohorts, respectively. These point estimates are presented in Table B.8. In each panel, the regression estimates that do not account (do account) for concurrent changes in credit card utilization are presented in the form of red circles (blue triangles). In both panels, the *x*-axis displays event time relative to the month of MPL loan origination and the *y*-axis represents credit scores.

Figure 2.6 visually displays the timing and the size of the effect of MPL loan take-up on credit scores (relative to their benchmarks) with and without controlling for the contemporaneous credit utilization change. The figure shows that the credit score increase for the MPL borrowers is largely explained by their contemporaneous sharp decline in credit utilization. We characterize this credit score response as "mechanical" in the sense that it can be computed by inserting the credit utilization change in the credit scoring formula. Credit utilization is the second most important component in credit scoring models and it generally explains 30% of the variation in credit scores.[30] This is because, conditional on defaulting, consumers are more likely to default on their unsecured debt first (e.g., credit card debt) before defaulting on secured debt (e.g., mortgages and auto loans).

Higher credit limits

In this section we show that MPL borrowers enjoy higher credit limits than their benchmark borrowers because of their credit score jump immediately after loan take-up. Since credit scores are used ubiquitously by lenders for their lending decisions, an improvement in a borrower's credit score should directly lead to increased borrowing capacity (see [84, 20, 70]). To test whether MPL borrowers' higher credit limits are driven primarily by their immediate credit score jump, we add the credit score jump as a control to our baseline credit limits regression specifications in Table 2.3, Columns (5) and (6). The coefficients for these specifications are reported in Table 2.7, Columns (1) and (3).

A comparison of Table 2.3, Column (5) with Table 2.7, Column (1) shows that the credit score jump shortly after the MPL loan take-up explains 68% of the credit limit increase of MPL borrowers relative to unmet borrowers two years after the MPL loan take-up. Similarly, Table 2.7, Columns (3) suggests that the credit score jump explains 32% of the credit limit increase of MPL borrowers relative to the bank borrowers two years after the MPL loan take-up. Moreover, the explanatory

[30]In comparison, the most important factor in credit scoring models – i.e., payment histories – explains 35% of the variation in credit scores. See `https://www.experian.com/blogs/ask-experian/credit-education/score-basics/my-credit-score/`

Table 2.7: What explains the ex post increase in credit card limits?

This table presents results examining the factors that drive the increase in MPL borrowers' credit card limits after MPL loan take-up. Columns (1) and (2) (Columns (3) and (4)) report the regression results for the change in credit card limits for the MPL borrowers relative to the matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Log(Credit Card Limits)			
Monthly DID	Unmet Cohort		Bank Cohort	
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)
Pre-period				
-12	-5.323***	-5.323***	-3.905***	-3.905***
	(0.186)	(0.186)	(0.143)	(0.143)
-6	-2.429***	-2.429***	-1.642***	-1.642***
	(0.096)	(0.096)	(0.086)	(0.086)
-3	-1.313***	-1.110***	-0.575***	-0.620***
	(0.178)	(0.071)	(0.071)	(0.064)
-2	-0.639***	-0.562***	-0.278***	-0.328***
	(0.103)	(0.054)	(0.041)	(0.045)
Post-period				
$+0$	0.644***	-0.136*	0.280***	-0.228***
	(0.188)	(0.071)	(0.074)	(0.057)
$+1$	0.813	-0.352***	0.845***	-0.262***
	(0.758)	(0.124)	(0.116)	(0.077)
$+2$	2.162**	0.155	1.705***	0.014
	(0.921)	(0.134)	(0.148)	(0.104)
$+3$	3.662***	0.718***	2.565***	0.308**
	(0.837)	(0.123)	(0.189)	(0.143)
$+6$	6.310***	1.278***	4.052***	0.520***
	(0.826)	(0.216)	(0.275)	(0.195)
$+12$	7.863***	0.954***	4.752***	-0.092
	(0.853)	(0.266)	(0.339)	(0.211)
$+18$	6.504***	-0.255	4.333***	-0.956***
	(0.838)	(0.313)	(0.428)	(0.245)
$+24$	4.918***	-1.532***	3.977***	-1.906***
	(0.912)	(0.369)	(0.535)	(0.293)
Cohort FE	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
ΔCredit Score $(-1,+1)$	✓	✓	✓	✓
ΔLog(# CC Accounts $(-1,t)$)		✓		✓
# Cohorts	347,172	347,172	118,148	118,148
Avg. Adj. R^2	0.241	0.582	0.206	0.569

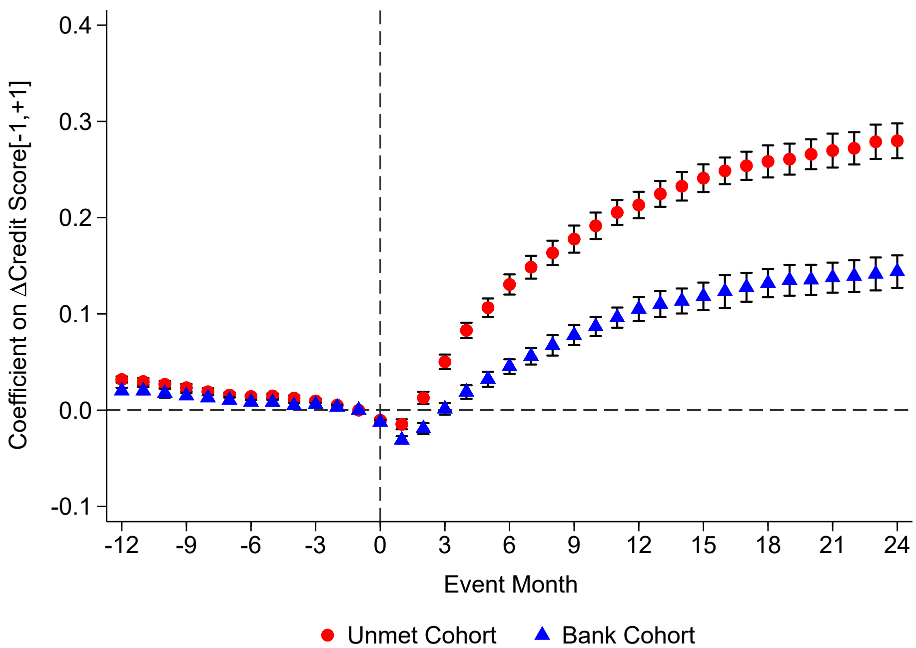


Figure 2.7: Impact of post-loan take-up credit score jump on credit limits

The figure presents the effect of the immediate credit score jump after the MPL or bank loan take-up on credit limits. The figure plots the coefficients and their 95% confidence intervals associated with the post-loan take-up credit score jump from the regressions in Table 2.7, Columns (1) and (3). In all panels, the *x*-axis displays event time relative to the month of MPL loan origination for MPL borrowers.

power of this immediate credit score jump strengthens over time, as shown in Figure 2.7, which plots the coefficient on the credit score jump over time from the estimated regressions in Table 2.7, Columns (1) and (3).

Next, we distinguish between two sources of credit limit growth: (i) new account openings (i.e., extensive margin), and (ii) increased limits on existing credit cards (i.e., intensive margin). The credit limit growth on the extensive margin is usually demand-driven because consumers need to apply to open new credit card accounts. In contrast, the credit limit growth on the intensive margin can be supply-driven because lenders can choose to increase credit limits even when borrowers do not request it.

Table 2.7, Columns (2) and (4) control for the credit limit growth on the extensive margin by including the log-change in the number of credit card accounts. The results show that the credit limit growth of the MPL borrowers primarily occurs on the extensive margin. This indicates that the credit limit increase for MPL borrowers is more likely to be demand-driven than supply-driven. Moreover, these results also suggest that the credit limit growth most likely occurs at new lenders, who arguably have to rely on hard information such as credit scores for their lending decisions. This is because existing lenders can increase credit limits on existing credit cards if consumers demand more credit. Additionally, consistent with the demand-driven increase in credit limits, Table B.9 shows that MPL borrowers are more likely to apply and open more new credit card accounts conditional on the credit score jump immediately after their loan take-up. Overall, the evidence in Table 2.7 and Table B.9 indicate that MPL borrowers enjoy higher credit limits due to their sharp improvement in credit scores after the MPL loan take-up.

2.5.3 Why do MPL borrowers' capacities and outcomes improve only temporarily?

The results in Section 2.5.1 suggest that, compared to bank lenders, MPL lenders are less able to screen high–default risk borrowers from the low–default risk borrowers. This inability to screen borrowers can pool the high–default risk borrowers along with low–default risk borrowers. Thus, relative to the bank borrowers, the weaker screening of the MPL borrowers ex ante is expected to result in lower average credit quality (e.g., lower future credit scores, higher future defaults) of the approved MPL borrowers ex post. Further, this pooling does not unravel immediately after obtaining the loan because the credit score jump shortly after the loan take-up is non-fundamental, as argued in Section 2.5.2.

Subsequently, the average credit market outcomes of the MPL borrowers deteriorate over time because the low–default risk and high–default risk borrowers, who are pooled at loan origination, separate over time as their types are revealed through their private actions (e.g., debt accumulation, delinquencies). Further, as the screening of MPL borrowers is weaker than bank borrowers, the credit market outcomes of the MPL borrowers deteriorate faster than bank borrowers as documented in our results.

2.5.4 Why do MPL lenders screen poorly compared to banks?

In this section, we provide evidence on two possible factors that explain why MPL lenders might screen poorly compared to banks. First, we find that compared to MPL lenders, banks have superior information on their borrowers due to their various relationships with the borrower. This superior information could be hard in nature, such as a borrower's checking account data or transaction history ([64]). Alternatively, the superior information could also be soft in nature, which is more easily collected and stored by a loan officer as opposed to a machine or an algorithm ([65]). However, unlike banks, such information may not be available to the MPL lenders because borrowers do not use the MPL platform for payments or transactions and do not interact with the MPL lenders in person.

Second, [50] show that MPL investors play an important role in screening borrowers over and above the screening by MPL platforms. However, the screening ability of MPL investors reduced over time as the MPL platform in our study reduced their information disclosure on loan applicants over time. We find that this reduction in the screening ability of MPL investors over time has further lowered the screening of MPL borrowers relative to bank borrowers.

We provide evidence on the two aforementioned factors in Table 2.8. In Panel A, we compare the performance of the MPL borrowers with two kinds of bank borrowers – namely, *new bank* borrowers and *relationship bank* borrowers. New bank borrowers are those who have no prior relationship with the bank that originates their bank installment loan. Relationship borrowers are those who have previously borrowed *other* credit products (e.g., credit cards, mortgages, auto loans) from the bank that originates their bank installment loan. Importantly, we drop borrowers with previous bank installment loans from our sample of relationship borrowers to ensure that our comparison is still within borrowers who are first-time personal installment loan applicants at both banks and MPL platforms.

Table 2.8, Panel A displays the results for the two-year cumulative default rates after the MPL/bank loan origination. $Relationship$ is an indicator variable that equals 1 for a cohort if the matched bank borrower in the cohort is a relationship borrower, and equals 0 if the matched bank borrower is a new borrower. The coefficient associated with *MPL×Relationship* in all columns in Panel A indicates that MPL borrowers perform relatively more poorly compared to relationship bank borrowers than new bank borrowers.

In Table 2.8, Panel B we exploit an unanticipated event in November, 2014 which drastically reduced the information on MPL loan applicant characteristics made available to investors, and thus diminished their screening ability (see [50] for more information regarding this event). We compare the performance of the MPL borrowers to bank borrowers for loans originated before and after this event in November 2014. Panel B displays the results for the two-year cumulative default rates across all kinds of debt after the MPL/bank loan origination. $Post$ is an indicator variable that equals 1 for all loans that were originated after November, 2014, and equals 0 for loans originated before November 2014. The positive and statistically significant coefficient on $MPL \times Post$ in all columns of Panel B indicates that MPL screening became relatively weaker than bank screening after the November 2014 event.

We also confirm this result in Figure 2.8, which plots the average two year cumulative default rates around the November 2014 event for the MPL and bank borrowers. Figure 2.8 shows that the performance of MPL and bank borrowers trend parallelly before November 2014. However, the performance of the MPL borrowers deteriorates shortly after November 2014, while the performance of the bank borrowers remains relatively unchanged.

While our results indicate that the screening of MPL borrowers can improve by providing MPL investors with more information, it comes with a caveat. [50] show that providing more information to MPL investors comes with the cost of creating greater adverse selection *among* investors when some investors are more sophisticated than the others. Given that MPL platforms seek to maximize

Table 2.8: Why do MPLs screen poorly compared to banks?

This table reports results that examine the factors that potentially explain the relatively poor screening ability of MPLs compared to banks. Panel A explores whether banks have superior information on their borrowers owing to their various relationships with the borrower. We classify bank borrowers on the basis of their past relationship with the bank issuing them their installment loans. $Relationship$ is an indicator variable that equals 1 for bank cohorts who have previously borrowed from the bank that originates their bank installment loan, and 0 for bank borrowers are those who have no prior lending relationship with the bank that originates their bank installment loan. Panel B explores how MPL screening quality compares to bank screening quality over time by exploiting a natural experiment in the MPL industry. In November 2014, the MPL platform drastically reduced the information on MPL loan applicant characteristics made available to investors, thus diminishing the screening ability of sophisticated investors on the platform. $Post$ is an indicator variable that equals 1 for loans originated after November 2014, and 0 otherwise. In Column (1) of both panels, the dependent variable is an indicator that equals 1 if the borrower defaults on any kind of debt in the 24-month window after the origination of the installment loan. In Columns (2) and (3), the dependent variable identifies credit card defaults and non–credit card defaults, respectively. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Relationship Bank Borrowers

Dependent Variable:	24M All Defaults	24M CC Defaults	24M Non-CC Defaults
	(1)	(2)	(3)
MPL	1.046***	0.999***	0.676**
	(0.393)	(0.269)	(0.333)
MPL × Relationship	1.586***	0.740***	1.165***
	(0.328)	(0.275)	(0.270)
Cohort FE	✓	✓	✓
Matching Controls	✓	✓	✓
Other Controls	✓	✓	✓
# Cohorts	109,298	109,298	109,298
Avg. Adj. R^2	0.075	0.064	0.077

Panel B: November 2014 Event

Dependent Variable:	24M All Defaults	24M CC Defaults	24M Non-CC Defaults
	(1)	(2)	(3)
MPL	0.580**	-0.219	0.526*
	(0.230)	(0.164)	(0.281)
MPL × Post	2.214***	2.374***	1.366***
	(0.470)	(0.355)	(0.401)
Cohort FE	✓	✓	✓
Matching Controls	✓	✓	✓
Other Controls	✓	✓	✓
# Cohorts	109,298	109,298	109,298
Avg. Adj. R^2	0.076	0.064	0.078

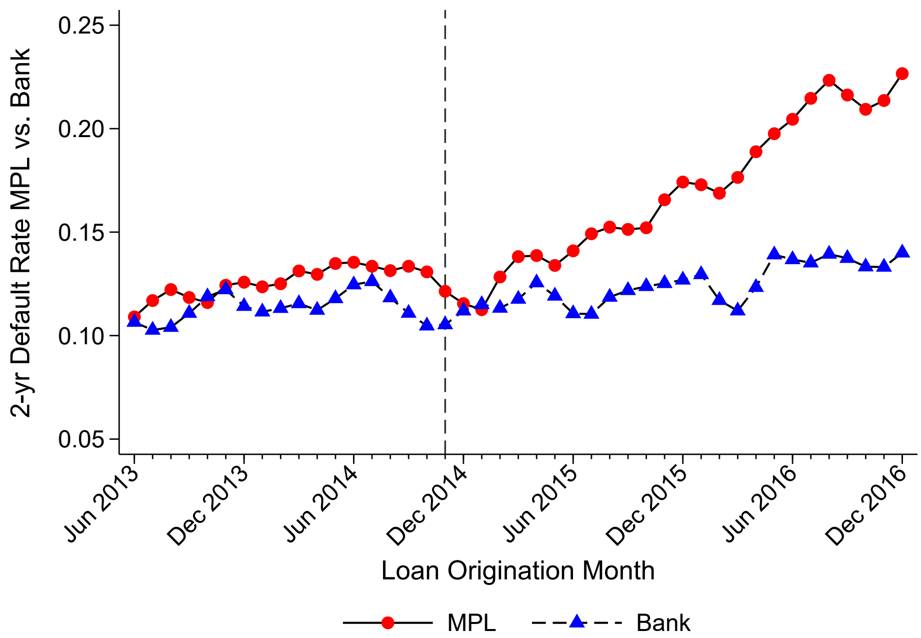


Figure 2.8: Impact of investor screening on MPL loan quality

The figure presents the average two year cumulative default rates at each month around November-2014 for the MPL and bank borrowers. November-2014 is the month in which the MPL platform in our study reduced the information provided to their MPL investors. The default rate for a borrower is computed across all the debt accounts held by the borrower. The *x*-axis represents the origination month of the MPL (bank) loan for the MPL (bank) borrower.

loan volume, greater adverse selection among investors can discourage the participation of less sophisticated investors thereby reducing the loan volume on MPL platforms.

2.6 Conclusion

In this paper, we examine how loans from MPL platforms impact the borrowing capacities and borrowing outcomes of borrowers. We benchmark the borrowing capacities and outcomes of MPL borrowers with two types of observably similar borrowers: (i) those who obtained a bank loan, and (ii) those whose credit demand was unmet or denied. By studying these effects of MPL loans relative to the two types of borrowers, we seek to compare the quality of MPL lenders' screening technology vis-à-vis the screening technology of banks.

We find that MPL borrowers' borrowing capacities – as proxied by their credit scores, credit utilization ratios, and credit limits – improve shortly after obtaining the MPL loan relative to their benchmark borrowers. However, in the long-run, MPL borrowers perform poorly and have higher default rates than their counterfactual benchmark borrowers. We document evidence that the relatively worse long-run borrowing outcomes of MPL borrowers is consistent with MPL lenders screening less effectively than banks, which leads to greater adverse selection of MPL borrowers. We provide evidence for two factors on which the adverse selection of MPL borrowers occurs. First, we find that MPL borrowers are more likely to be liquidity constrained than bank borrowers. And second, we find that MPL borrowers are less likely to use their loans for their stated loan purpose.

Finally, we also explore the factors which allow banks to screen their borrowers better than MPL platforms. Our results suggest that banks have an information advantage compared to MPL platforms because they engage in relationship lending, which allows banks access to unique borrower information such as their transactions history or soft information. We also find that greater information provision to the investors on the MPL platform can reduce the screening gap between banks and MPL lenders.

While our results indicate that MPL lenders screen less effectively than banks, we do not take a stance on the consumer welfare effects of MPL loans, and leave this an avenue for future research.

CHAPTER 3

SHOCKED BY BANK FUNDING SHOCKS: EVIDENCE FROM 500 MILLION CONSUMER CREDIT CARDS

3.1 Introduction

Uninsured short-term wholesale liabilities such as repos and commercial paper are an important source of funding for many banks. However, a reliance on short-term wholesale funding can expose banks to significant roll-over risks and runs ([85, 86, 87]). These funding markets dried up suddenly in 2008, causing negative shocks to bank liquidity ([88, 89, 90]). In this paper, we show a new channel—namely, credit card limits, through which banks transmitted their wholesale funding liquidity shocks to their consumers and affected their consumption. Importantly, we show that banks transmitted their liquidity shocks unequally across consumers, which sheds light on both *who* bears the real costs of fragile bank funding structures and *how persistent* they are.

Wholesale funding is a significant source of funding for many of the banks in our sample (which account for 75% of the credit card issuance in the U.S.). However, if these banks can easily substitute their wholesale funding with another funding source on similar terms, then they may not need to pass on their funding shocks to their consumers. Similarly, while credit cards are an important source of marginal financing for many households in the U.S.[1], if consumers can switch costlessly to other credit cards, or if they have sufficient unused credit[2], then the transmission of bank liquidity shocks through credit cards should not have an effect on their credit card spending. Thus, frictions

[1]Of the 40% of U.S. households that cannot cover an unexpected emergency expense of $400, 43% said they use credit cards to cover these unexpected expenses and will pay it off over time. Source: `https://www.federalreserve.gov/publications/files/2017-report-economic-well-being-us-households-201805.pdf`.

[2]According to the 2007 Survey of Consumer Finance, 59.8% of households held two or more credit cards, and these households utilize less than half of their available credit limit, on average.

that constrain both banks and their consumers in the credit market are necessary for the wholesale funding shock to have a persistent real impact through credit cards ([79]).

Using 500 million credit cards from a major credit bureau, which include *all* the credit cards for any given individual, and a *within*-consumer empirical design, we show that banks that faced a sudden decline in wholesale funding reduced the credit limits on their consumer credit cards. However, there is significant heterogeneity in how banks pass on their funding shocks across their consumers. Affected banks reduced credit limits more sharply for consumers with lower credit scores and higher utilization ratios. In response, the consumers of the affected banks reduced their total credit card consumption suggesting frictions in the credit card market. Further, we also show that the negative effect of the bank wholesale funding liquidity shock on consumption through credit cards was long lasting. Thus, our evidence suggests that the borrowing constraints on credit cards induced by the funding shock were likely an important contributor to the decline in aggregate consumption during the Great Recession and its sluggish recovery thereafter ([91, 92]).

The short-term wholesale funding market for banks collapsed in September 2008. We refer to the time period before (after) September 2008 as the *pre-shock* (*post-shock*) period. We measure a bank's exposure to the funding liquidity shock by the bank's dependence on short-term wholesale funding in the pre-shock period. The intensity of the wholesale funding liquidity shock varies across banks as there is variation in the banks' dependence on wholesale funding in the pre-shock period. Moreover, the collapse of the short-term wholesale funding markets seems largely unanticipated.

The primary identification challenge is to isolate the changes in credit supply from the changes in credit demand. For example, individual-specific demand factors such as income changes can affect a bank's credit extension to an individual. So, we first focus on individuals with credit cards from multiple banks and use a fixed-effects methodology similar to [17]. We essentially compare how the credit limits on credit cards issued to the *same* individual change as a function of the issuing bank's exposure to the liquidity shock. We find that a bank with a one standard deviation greater dependence on wholesale funding in the pre-shock period cuts its credit limits by 4.75%, or equivalently by $434, based on the average credit limit across consumers.

Another potential endogeneity concern is individual-bank-specific demand driven by a consumer's preference to use some credit cards more than their other credit cards. To mitigate this concern, we construct a "leave-out-mean" credit limit change for each card, which by construction excludes the credit limit change due to individual-bank-specific demand, but captures the bank's average change in credit supply. Our results remain unchanged even after accounting for potential individual-bank-specific demand. Our findings are also not driven by the household balance sheet channel, or any particular bank, or bank type, such as large banks, risky banks, or risk-averse banks. Further, on the extensive margin, we find that banks that were more affected by the short-term wholesale funding shock were also less likely to issue new credit cards and more likely to close existing credit cards.

We also validate our exposure measure and our baseline result using publicly available data from banks' regulatory filings. First, banks that had a greater dependence on short-term wholesale funding indeed experienced a greater decline in short-term wholesale funding in the post-shock period. Moreover, this decline in short-term wholesale funding was not offset by changes in the other sources of bank funding such as deposits and equity. Second, at the bank level, a bank's greater dependence on wholesale funding in the pre-shock period is associated with a reduction in credit card loans on its balance sheet in the post-shock period.

Next, using the near universe of approximately 500 million credit cards issued to 134 million consumers, we document that the liquidity crunch induced by the wholesale funding market had real consequences for aggregate credit card borrowing. By including all credit cards for each consumer, we can examine whether consumers can hedge themselves by substituting to other credit cards.[3] We show that individuals who had a greater exposure to the funding shock through their banks experienced a greater reduction in their total credit limits. On average, a 1% short-term wholesale

[3]We also aggregate limits and balances across newly opened credit cards to capture the substitution effect more precisely.

funding shock induced reduction in an individual's total credit limit reduced the individual's total credit card balance by 0.32%. Equivalently, in terms of marginal propensity to consume (MPC), a $1 reduction in total credit limit led to $0.07 reduction in total credit balance. These MPC estimates through the card card channel are similar to the household balance sheet channel documented in [91] ($0.05-$0.07 MPC for $1 decline in housing values). While it is possible that individual-level demand factors confound our individual-level analysis, it seems less likely based on our credit card-level analysis, which suggests that demand factors are mostly uncorrelated with the exposure measure.[4]

We find heterogeneous effects of the short-term wholesale funding shock on consumers. First, at the credit card level, we find that banks do not transmit shocks equally across consumers. Conditional on the same liquidity shock, banks reduced credit limits by more for consumers with lower credit scores and higher credit card utilization. We find that consumers who had a 90% or more credit card utilization experienced a $970 reduction in their credit limits, on average. While, consumers with less than 50% utilization experienced a credit limit reduction of $370. These results are consistent with the increased cost of lending to such borrowers due to information frictions when a bank's cost of funding increases ([79]). At the individual level, conditional on the same magnitude of the funding shock transmitted by banks, we find that consumers who had higher aggregate credit card utilization and lower credit scores cut back more on their credit card balances. These results are consistent with credit-constrained consumers being unable to hedge themselves from the transmitted bank shocks. Overall, our results at the credit card level and the consumer level show that consumers who face more credit constraints bear greater costs of bank fragility.

We find that in the long run, while the total credit extended by credit card issuers returns to pre-shock levels at the bank level, the effect of the wholesale funding shock was persistent for some individuals. Specifically, among consumers with low credit scores, even in the long run, the

[4]For the credit card-level analysis, the coefficient estimates for the exposure measure are similar with or without individual fixed effects. Also, the estimates are similar when we use the "leave-out-mean" approach. Together both results suggest that individual-level demand factors are unlikely to be correlated with the exposure measure.

consumers who were more exposed to their banks' wholesale funding shock had lower total credit limits and as a result, were more limited in their ability to borrow on their credit cards, than the low-exposure consumers. In contrast, the effect of the wholesale funding shock dissipated relatively quickly over time for the consumers with higher credit scores. These results suggest that either (i) financing frictions for lower-quality borrowers were binding for a very long time or (ii) the funding shock itself weakened borrowers' fundamentals, thereby limiting their access to credit in the future. Regardless of the underlying channel, our results underscore the long-term real consequences of a bank's fragile funding structure across different types of consumers.

Finally, we also compare the importance of the credit card channel relative to alternate revolving credit lines through which banks can transmit their funding shock. Specifically, we compare the transmission of the bank funding shock between credit cards and the home equity line of credit (HELOC), which is an alternate revolving line of credit that homeowners can tap to smooth their consumption. However, unlike the unsecured credit card debt, HELOCs are secured by the consumer's home. We find that banks transmitted the short-term wholesale funding shock primarily through credit cards as opposed to HELOCs even though both revolving credit accounts were available to banks for transmitting the funding shock.

Our paper contributes to the literature on the transmission of bank shocks to firms ([17, 93, 94, 42, 95]) and households ([96, 97, 98, 99]). Using detailed microdata on credit card limits and a within-consumer empirical design, we identify the transmission of banks' liquidity shocks to consumers through the credit card lending channel.

Importantly, our paper contributes to the post-crisis regulatory reform that has focused on addressing the vulnerabilities of a bank's funding structure that is especially reliant on wholesale funding ([89]). For instance, the Federal Reserve has proposed to tie the risk-based capital surcharges for the systemically important U.S. banks to their reliance on wholesale funding.[5] Our results en-

[5]See `https://www.federalreserve.gov/newsevents/speech/tarullo20141120a%20.htm#fn10`

rich this debate by providing elasticities of aggregate credit limits and credit balances to wholesale funding and also the heterogeneity of these elasticities across different types of consumers. In doing so, we shed light on who bears the cost of bank fragility and for how long.

Our paper also adds to the literature on the sharp decline in household consumption during the recent Great Recession. [100, 91, 101] attribute the consumption decline to the poor state of household balance sheets, which were impaired by the sharp decline in house prices. We complement their study by showing how the impaired balance sheets of financial intermediaries affected the credit supply to the economy and the consumption of goods. Instead of helping households smooth consumption during the crisis, banks can pass on their own shocks to households and may also amplify them. Further, while consumption recovered slowly in the post-crisis period, this recovery was puzzlingly slow for non-durable goods and services relative to durable goods, despite the recovery in households' net worth ([92]). Our evidence suggests that borrowing constraints on credit cards, which are used to consume non-durable goods and services also played a significant role in the sluggish recovery in consumption in the post-crisis period.

Our paper is also related to [48], [14], and [102] who document that households borrow more immediately after credit card limit increases. In contrast, we focus on a large negative liquidity shock that varies across banks and negatively affects their credit decisions. Using the near universe of credit cards in the U.S., we identify how the pass through varies across consumers. In addition to examining balance changes at the credit card level, we also study changes in total credit card balance and total debt balance at the individual level. Our results suggest that some consumers couldn't hedge the negative funding shock that their banks transmitted by borrowing from other sources. Most importantly, we document that although banks' total credit supply has recovered, the impact of the funding shock is still persistent for some borrowers even a decade after the shock.

The rest of the paper proceeds as follows. Section 3.2 discusses identification challenges and empirical methodology. Section 3.3 describes the data. Section 3.4 presents the main results. Section 3.5 studies the heterogeneous response to the funding shock. Section 3.6 presents the long-run effects of the funding shock. We conclude in Section 3.7.

3.2 Identification challenges and empirical methodology

This section discusses the identification challenges and the empirical specification used to identify the transmission of the short-term wholesale funding shock to consumers through credit cards. The aggregate trends of credit card limits and balances indicate that such a transmission channel through credit cards may exist. Time-series patterns shown in Figure 3.1 reveal that credit card limits declined by approximately 25% between January 2008 and January 2010. Similarly, during the same time period, aggregate credit card balances declined by approximately 16.7%. This figure appears to suggest that the drop in aggregate credit card limits precedes the drop in credit card balances, which is consistent with households being unable to smooth their consumption through credit cards due to a reduction in their credit limits. However, such aggregate trends can be confounded by various credit demand factors.

Consequently, the main identification challenge is to isolate the changes in credit supply from the changes in credit demand. Specifically, our empirical exercise could be subject to potential endogeneity concerns if credit card issuers change credit limits in anticipation of changes in credit demand. For instance, credit card issuers can reduce credit limits in anticipation of lower consumer demand (e.g., an increase in unemployment in the aftermath of the 2008–2009 Great Recession) if maintaining unused credit lines is costly for credit card issuers.[6]

Our identification strategy allows us to mitigate such endogeneity concerns. We use an unanticipated funding shock to banks resulting from the dry-up of the short-term wholesale funding market at the end of 2008. Banks varied substantially in their dependence on short-term wholesale funding. Thus, banks that depended more on short-term wholesale funding experienced greater unanticipated funding liquidity shocks. Next, we use the granular credit card account–level data to estimate the impact of the funding shock on the credit limits extended by the banks to their credit card borrow-

[6]For example, banks are required to hold capital even on unused credit card commitments.

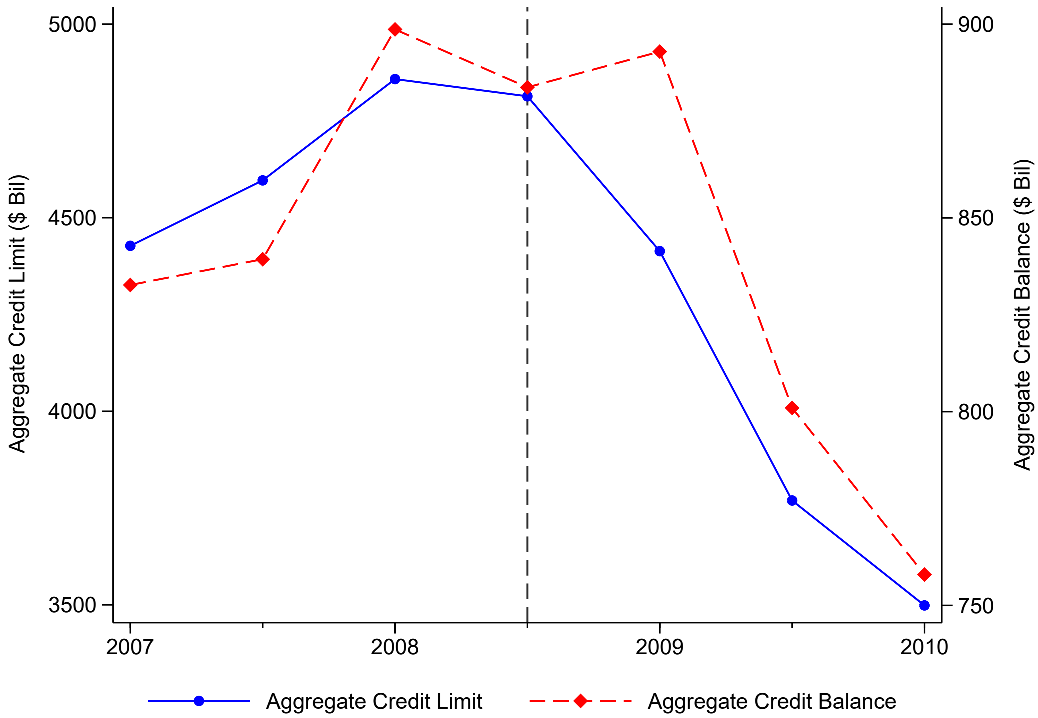


Figure 3.1: Aggregate changes in credit limits and credit balances

The figure presents the aggregate trend in credit limits and credit balances over time. Our data are available at a semiannual frequency from 2007–2010 from one of the three major credit bureaus in the U.S. The solid blue line represents the trend in aggregate credit limits over time. The dashed red line represents the trend in aggregate credit balances over time. The dashed vertical black line represents July 2008, which demarcates the pre- and post-shock period in our analysis. While both aggregate credit limits and balances decline in the post-shock period, the changes in aggregate credit balances seem to follow changes in credit limits.

ers. For identification, we construct our tests similar to [17] (see also [42, 43]), in which we isolate changes in credit limits at the credit card account level in the presence of $Individual$ fixed effects. We estimate the following specification:

$$\Delta CreditLimit_{i,c,b} = \alpha + \beta Exposure_b + f(\mathbf{X}_{i,c,b}) + \eta_i + \varepsilon_{i,c,b} \tag{3.1}$$

where i, c, and b index individuals, credit cards, and banks, respectively. $\Delta CreditLimit_{i,c,b}$ is the log-change in credit limits for individual i's credit card c with bank b from the pre-shock to the post-shock period.

The log-change in credit limits for each credit card is computed by first collapsing the time-series credit card–level data by averaging across time to obtain a single credit card–level cross-section separately in the pre-shock and post-shock period. Pre-shock and post-shock periods are symmetric, with each consisting of three semiannual archives. Each archive is a snapshot of the credit limit and the balance on each credit card in our sample. The pre-shock period includes semiannual archives for January 2007, July 2007, and January 2008. The post-shock period includes semiannual archives for January 2009, July 2009, and January 2010.

The variable $Exposure_b$ measures the exposure of a bank to the wholesale funding shock. It is constructed as the pre-shock value of the short-term wholesale funding-to-deposit ratio, thus it captures the proportion of a bank's runnable funding to its stable funding. The main coefficient of interest is β, which measures the impact of a bank's wholesale funding shock on the credit limits of its borrowers. η_i is the individual fixed-effects, which controls for confounding individual-specific demand factors (e.g., income changes) that could bias our results. However, individual-bank-specific demand factors can still confound our analysis, especially if consumers prefer to use some of their credit cards more than their other credit cards.[7] In such cases, the true measure of individual demand will be better reflected through those frequently used credit cards. We address this plausible concern in Section 3.4.1 by constructing a "leave-out-mean" credit limit for each bank–consumer pair. This measure captures the average credit limits extended by the bank across all credit cards after excluding the bank's credit limit extended to that consumer.

Finally, $f(\mathbf{X}_{i,c,b})$ is a vector of control variables at the bank level, the individual–bank level, and the credit card level, as measured in the *pre-shock* period. The bank-level variables control for differences in characteristics across credit card issuers that can confound our analysis, such as size,

[7]For instance, consumers may be rewarded with cash-back or points that are redeemable for cash on certain credit cards.

performance, and lending quality. Moreover, our specification allows for these bank-level characteristics to have differential effects on the post-period relative to the pre-period. Effectively, this allows for a horse-race between our proposed mechanism based on the funding shock versus the other bank-level characteristics along the lines of [103, 104]. For instance, the credit extended by large banks and small banks might differ in the post-period relative to the pre-period because large banks and small banks were subject to different intensities of regulatory oversight post-2008. As further robustness, we also control for bank characteristics non-parametrically using indicator variables (e.g., above/below median) and find that our results are unchanged. We also include individual–bank variables (e.g., the number of credit-related accounts with the bank) and credit card–level variables (e.g., the age of the account) to control for individual-bank-specific demand and supply factors that can affect credit limits. The error term is $\varepsilon_{i,c,b}$.

To estimate the effect of the banks' wholesale funding shock on credit card balances, we estimate a two-stage least squares regression model. The first stage is estimated using Equation (3.1), which captures the change in credit limits (i.e., the bank's credit supply) that resulted from the wholesale funding shock. In the second stage, we estimate the effect of credit limit changes on credit balance changes by instrumenting the credit limit changes with the bank's exposure to the wholesale funding shock. We estimate the following second-stage regression:

$$\Delta CreditBalance_{i,c,b} = \alpha' + \beta' \Delta \widehat{CreditLimit}_{i,c,b} + f(\mathbf{X}_{i,c,b}) + \eta'_i + \varepsilon'_{i,c,b} \tag{3.2}$$

where $\Delta \widehat{CreditLimit}_{i,c,b}$ are the fitted values from Equation (3.1). For β' to be consistent, the instrument $Exposure_b$ must satisfy the exclusion restriction. That is, a bank's wholesale funding exposure should affect only the changes in their borrowers' credit balances through credit limit changes. This seems plausible, since most borrowers are unlikely to be familiar with their banks' funding structure for it to directly affect their credit card consumption.[8] An important caveat in our

[8]However, it is possible that a reduction in bank funding can reduce a bank's investment in its service quality, which can in turn affect the usage of their issued credit card. We test this possibility by regressing the change in a bank's

analysis of credit card balances is that we observe the snapshot of balances in each archive, which can differ from the true borrowing on those credit cards. However, since the credit card spending of unconstrained borrowers should not be systematically affected by credit limit changes, β' should capture the changes in credit card borrowing from the pre- to the post-shock period due to the short-term wholesale funding shock.

An alternate method to conduct our analysis is by using the time-series panel data and including $Individual \times Archive$ fixed effects. However, this is more challenging, as it entails a larger number of fixed effects. Further, collapsing the time-series and estimating cross-sectional regressions mitigates the econometric issues related to the underestimation of standard errors in the panel data that have short time dimensions ([44, 45]). Thus, our procedure provides us with conservative standard errors. Similarly, one could also carry out the analysis at the individual–bank level (as opposed to the individual–bank–card level) by aggregating across the multiple credit cards an individual has with a bank. However, we prefer the individual–bank–card level analysis as we can control for the credit card-level variables related to demand more flexibly (e.g., credit card utilization ratio, credit card age). However, as robustness, we also conduct our baseline analysis at the individual–bank level and find that our results largely unchanged.

3.2.1 The short-term wholesale funding shock

A bank's funding sources can be broadly divided into *deposits* and *wholesale funding*. Wholesale funding consists of non-deposit financing such as repos and commercial paper. These funding are provided mainly by institutional investors such as money market funds (MMFs) and other banks. Deposits are insured by the Federal Deposit Insurance Corporation (FDIC) and are thus a cheaper source of funding than wholesale funding. By the virtue of being insured, deposits are also less

noninterest expense-to-assets ratio from the pre- to the post-shock period on the *Exposure* measure. The noninterest expense includes advertising, promotional, public relations, business development expenses, and salaries. We do not find any evidence that banks with a greater dependence on wholesale funding reduced their noninterest expense per unit of assets.

sensitive to a bank's financial health than other uninsured sources of bank funding. As a result, deposits are more stable and less prone to runs ([105]). However, banks generally find it costly to raise deposits quickly to cover any funding gap because the supply of deposits is highly inelastic with respect to the deposit rates offered ([106]). Therefore, banks turn to non-deposit or wholesale funding as an alternative to deposits when they need to quickly cover any funding gap.

Figure C.1 shows the change in bank funding measures over time as a fraction of total assets. Consistent with wholesale funding and deposits being substitutes, Subfigures C.1a, C.1b, and C.1c show their negative correlation over time. Subfigure C.1d gives credence to the substitution argument by showing that the total liabilities are either relatively stable (before 2008) or decreasing (after 2008) across time.

The total wholesale funding consists of all funding sources of banks other than deposits and equity. This funding can be broadly divided into short-term and long-term wholesale funding. Short-term wholesale funding consists of funding liabilities that have a maturity of less than one year, such as repos, commercial paper, and interbank borrowing. However, long-term wholesale funding consists of other funding liabilities that have a maturity of greater than one year. Furthermore, among the total wholesale funding, the short-term wholesale funding is more sensitive to a bank's financial condition. This is because the shorter the maturity of the wholesale funding, the more exposed it is to rollover risks. Therefore, a decline in a bank's financial health can quickly make short-term wholesale funding prone to runs and liquidity shocks if the lenders who provide the short-term funding choose not to rollover their funding ([88, 107]).[9] Figure C.2 shows the change in the components of wholesale funding over time as a fraction of total assets. The rise and fall of wholesale funding—especially the short-term funding—is apparent.

We measure a bank's exposure to the short-term wholesale funding shock as the short-term wholesale funding-to-deposits ratio. Recall that this ratio is a measure of a bank's runnable funds

[9]For instance, the maturity of certain short-term wholesale liabilities (e.g., repos, commercial paper) can be as short as a day or a week.

(i.e., short-term wholesale funding) as a proportion of its non-runnable funds (i.e., deposits). A bank with a higher value of this measure should be more prone to the short-term wholesale funding shock. In Figure 3.2, we plot our main bank exposure variableover time. Figure 3.2 shows that short-term wholesale funding increased with respect to deposits from 2004 to 2008, then fell dramatically after 2008. This is consistent with the sudden dry-up of the wholesale funding market which was documented in previous studies ([90]).

Figure 3.2: Exposure measure over time

The figure presents the time trend of our main independent variable of interest, *Exposure*, which is defined as the ratio of a bank's short-term wholesale funding to its total deposits. The figure is plotted by averaging the *Exposure* variable across the banks in our sample in each quarter. The data for the plot below are gathered from the quarterly Y-9C filings of U.S. BHCs.

3.3 Data and summary statistics

3.3.1 Data

We use data from Equifax, which is one of the three major credit bureaus in the U.S. All the data described below are used purely for academic purposes, and they contain anonymized information. The credit bureau's data provide comprehensive records of the various credit accounts opened by every U.S. resident. These credit accounts span credit cards, mortgage, auto, student loans, and personal/business loans.

We identify the 25 bank holding companies (BHCs) from the credit bureau's data files that contain credit card issuers and also have a non-zero dependence on wholesale funding. These 25 BHCs account for 75% of the market in terms of open credit cards. From this set of 25 credit card issuers, we omit six issuers that differ from the rest of the sample of banks, and we omit one issuer because of insufficient data coverage during our sample period. Of the six issuers we omit, four are foreign credit card issuers, one issuer specializes in retail store credit cards, and one issuer targets a particular segment of the U.S. population. By omitting these six issuers, we mitigate potential credit card–specific demand factors that can confound our analysis. For instance, if consumers use retail store credit cards exclusively to borrow and purchase expensive luxury goods, then reductions in the credit limits and balances of the retail store credit cards could reflect the lower demand for luxury goods in the post-2008 period. Our final dataset consists of 18 credit card issuers that cover seven of the top 10 credit card issuers and account for 65% of the market share.

Next, from the entire U.S. population, we identify all individuals who have active credit card accounts issued by at least one of the 18 credit card issuers in our sample, as of January 2008. We then obtain their credit card limit and balance information. As mentioned previously, this information on credit card limits and balances is available at a semiannual frequency. For the credit-card level analysis, we omit credit cards that are closed in the post-shock period. This filter ensures that we do not pick up changes in credit card limits and balances due to credit card cancellations or personal bankruptcies. As a result, the credit-card level analysis focuses on tracking changes in credit cards along the intensive margin. However, for robustness, we also show that our results are similar on the extensive margin. We also limit our analysis to credit cards that have at least one nonmissing pre-shock and one nonmissing post-shock limit and balance observation. We winsorize both measures at the 1% and 99% levels. Finally, we average the credit limits on credit cards separately in the pre- and post-shock periods to capture the average credit supply on individual credit cards before and after the shock. We do the same for credit card balances. Our baseline credit card–level analysis with individual fixed effects relies on comparing changes in credit card limits and balances *within* individuals. Thus, our baseline credit card–level analysis includes only individuals who have two or more credit cards issued by banks that are exposed to the short-term wholesale funding shock, in which the banks differ in their exposure to the funding shock. After this final filter, we are left with 158 million credit cards issued to 54 million individuals.

Importantly however, for our individual-lavel analysis, we use our entire dataset of 500 million credit cards issued to 134 million individuals.[10] By doing this, we allow credit card closures, more importantly, credit card originations, in the post-shock period. This analysis aggregates credit limits and balances across all credit cards for each consumer and allows us to test the overall effect of the short-term wholesale funding shock at the individual level. For instance, we can account for the possibility that consumers can hedge themselves by opening new credit cards in the post-shock period.

[10]Table C.1 reports the summary statistics at the consumer level for the full sample (134 individuals) and the fixed effect model (FE) sample (54 million individuals). This table shows that individuals in the FE sample have similar credit score, monthly income, debt-to-income ratio, credit card utilization, and mortgage balance as individuals in the full sample. Individuals in the FE sample have more credit card accounts, more debt related accounts, and higher credit card balances. However, the difference is mechanically driven by the fact that we require individuals in the FE sample have at least two credit cards.

3.3.2 Bank-level summary statistics

We use a bank's dependence on wholesale funding as a measure of the bank's exposure to the unexpected liquidity shock. Therefore, we use the cross-sectional variation in the dependence on short-term wholesale funding across banks as a measure of the variation in a bank's exposure to the unexpected liquidity shock. Table 3.1 shows how the exposure to liquidity shocks varies cross-sectionally across the banks in our sample. To show this, we first collect data from the quarterly BHC Y-9C regulatory filings for each of our credit card–issuing banks at the bank holding company level from 2006–2010.[11] We define the pre-shock period from 2006Q1 to 2007Q4, and we define the post-shock period from 2009Q1 to 2010Q4. Then, we collapse the quarterly bank-level data to obtain a single bank-level cross-section separately in the pre-shock and post-shock periods by averaging across time.

Table 3.1, Panel A presents summary statistics for the bank-level characteristics after splitting the pre-shock cross-section into banks that had a high exposure (above median) and banks that had a low exposure (below median) to the wholesale funding liquidity shock based on the bank's short-term wholesale funding-to-deposits ratio. We report means and medians for the bank-level characteristics where the medians are reported in square brackets. Column (3) reports the difference between the means for the high– and low–shock exposure banks, and also tests for its statistical significance.

[11] We can collect data at the holding company level for 17 out of the 18 credit card–issuing banks. We supplement our data with the quarterly call report data for the remaining credit card–issuing bank. Our results are robust even after excluding this bank from our analysis. However, we choose to retain it for our analysis because it is an economically important bank that ranks among the top five credit card–issuing banks in terms of market share.

Table 3.1: Balancing of covariates

This table presents the summary statistics at the credit card–issuing bank level. Panel A uses quarterly BHC Y-9C and Call report regulatory filings data in the pre-shock period. The pre-shock period for regulatory filing data ranges from 2006Q1 to 2007Q4. Panel B uses credit bureau data in the pre-shock period. The pre-shock period for credit bureau data consists of three semiannual archives namely, January 2007, July 2007, and January 2008. The data are first collapsed to obtain a single bank-level cross-section in the pre-shock period by averaging across time. Next, the statistics reported in the table are obtained by dividing the cross-section based on high (above median) and low (below median) dependence on short-term wholesale funding in the pre-shock period. The dependence on short-term wholesale funding is measured as the ratio of short-term wholesale funding to total deposits. The variables in Panel A are reported as a fraction of total assets unless otherwise specified. The table reports means and medians. Medians are reported in square brackets. Column (3) reports the difference in means and medians. The significance for the statistical test that tests the equality of means are reported using *, **, and ***, which indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Quarterly BHC Y-9C and Call report regulatory filing data

	Dependence on short-term wholesale funding (exposure to shock)		
	High-exposure (1)	Low-exposure (2)	Diff (3)
Wholesale funding			
Short-term wholesale funding/Deposits	0.300	0.101	0.200***
	[0.217]	[0.095]	[0.122]
Wholesale funding	0.242	0.099	0.143***
	[0.218]	[0.098]	[0.120]
Fed funds purchased	0.014	0.016	-0.002
	[0.009]	[0.008]	[0.001]
Repo	0.074	0.026	0.048*
	[0.042]	[0.020]	[0.023]
Other liabilities maturity $<$ 1 yr	0.060	0.024	0.035***
	[0.059]	[0.018]	[0.041]
Other liabilities maturity $<$ 1 yr	0.094	0.033	0.061***
	[0.098]	[0.020]	[0.078]
Other			
Assets (log)	19.599	16.377	3.222***
	[19.213]	[16.555]	[2.659]
Deposits	0.539	0.662	-0.123*
	[0.555]	[0.713]	[-0.158]
Equity capital	0.096	0.177	-0.081
	[0.092]	[0.098]	[-0.006]
Liquid assets	0.223	0.226	-0.003
	[0.215]	[0.220]	[-0.005]
Business mix			
CC loans	0.041	0.070	-0.029
	[0.038]	[0.029]	[0.010]
Mortgage loans	0.298	0.379	-0.081
	[0.304]	[0.442]	[-0.138]
C&I loans	0.115	0.100	0.015
	[0.100]	[0.095]	[0.004]
Performance			
ROE	0.088	0.105	-0.018
	[0.087]	[0.080]	[0.007]
Non-perf loans	0.007	0.005	0.002
	[0.006]	[0.004]	[0.003]
Non-perf CC loans/Total CC loans	0.019	0.023	-0.004
	[0.021]	[0.011]	[0.010]
Risk-based capital ratio	12.140	23.215	-11.075*
	[11.605]	[14.221]	[-2.617]

Panel B: Credit bureau data

	Dependence on short-term wholesale funding (exposure to shock)		
	High-exposure (1)	Low-exposure (2)	Diff (3)
Borrower fundamentals			
Credit score	733.47	729.21	4.26
	[741.75]	[750.89]	[-9.14]
Monthly income ($)	3,897.39	3,868.95	28.44
	[3,985.62]	[3,994.93]	[-9.31]
Debt-to-income ratio (DTI)	33.49	32.55	.94
	[34.65]	[31.95]	[2.7]
Subprime (%)	10.08	13.71	-3.63
	[8.39]	[5.28]	[3.11]
Credit card debt			
Credit card accounts	4.49	4.17	.32
	[4.54]	[3.98]	[0.56]
Credit card balance	6,936	5,743.50	1,192.50**
	[6,634.90]	[6,013.95]	[620.95]
Credit card utilization	26.61	28.96	-2.36
	[23.90]	[21.74]	[2.16]
Credit card delinquency	0.75	0.86	-0.11
	[0.54]	[0.42]	[0.12]
Other debt			
Total-debt related accounts	10.58	9.87	0.71
	[10.55]	[9.97]	[0.58]
Mortgage balance ($)	191,283.39	170,178.43	21,104.96*
	[198,673.86]	[163,028.70]	[35,645.16]
Auto balance ($)	16,732.41	17,077.06	-344.65
	[16,806.57]	[16,840.96]	[-34.39]

Table 3.1, Panel A, shows that the mean exposure (i.e., the dependence on short-term wholesale funding) for the banks in the high-exposure group is three times greater than the mean exposure for banks in the low-exposure group. The total wholesale funding as a fraction of assets is also about 2.5 times greater for the high-exposure group than the low-exposure group. In terms of individual components of wholesale funding, except for the federal funds purchased, the high-exposure banks have a significantly greater dependence (by about 2.5–3 times) on all components of the wholesale funding compared to the low-exposure banks.

High-exposure banks are also significantly larger than low-exposure banks, and they have a smaller deposit base as a fraction of their assets. This is consistent with previous literature that argues that a bank can substitute deposit funding with wholesale funding either (a) because of the bank's inability to raise deposits quickly when it must expand lending, or (b) in response to an outflow of deposits (see [108]). Moreover, [108] also suggest that, when required, large banks (as opposed to small banks) are better able to substitute deposits with wholesale funding because they face lower financial frictions. As a result, they obtain wholesale funding at a lower cost. Our results are consistent with the observed size differential between high- and low-exposure banks in our sample.

Further, Table 3.1, Panel A, shows that the high- and low-exposure banks are not statistically different in terms of their equity capital, which is an alternate source of bank funding. The high- and low-exposure banks are also similar in terms of their business mix, which measures the extent to which the banks engage in credit card, mortgage, and commercial and industrial (C&I) lending. The covariate balance along each component of the business-mix dimension also mitigates the concern that a shock to a particular industry in which banks operate drives the drop in credit card lending.

Finally, Table 3.1, Panel A, shows that the high- and low-exposure banks have similar performance-based measures. Importantly, given that credit cards are the focus of our study, we find no statistical difference in the performance of credit card loans between the high- and low- exposure banks. If anything, the point estimates suggest that the credit card loans of the high-exposure banks performed better than the low-exposure banks in the pre-shock period. The high-exposure banks had 0.4 percentage points fewer non-performing credit card loans as a fraction of their total credit card loans (or 17.4% lower than the low-exposure banks).

3.3.3 Credit card–level summary statistics

Table 3.1, Panel B, presents summary statistics for the quality of borrowers to whom banks issue credit cards, after splitting banks into high- and low-exposure banks. We follow the same procedure as shown in Table 3.1, Panel A. We first collapse the data to obtain a single bank-level cross-section in the pre-shock period by averaging across the semiannual archives. Next, we obtain the statistics reported in the table by dividing the cross-section into the same high- and low-exposure banks as defined in Table 3.1, Panel A.

Table 3.1, Panel B, shows that the high-exposure banks lend to borrowers who have better credit quality and stronger fundamentals. The borrowers of the high-exposure banks have higher credit scores and a higher monthly income. The percentage of subprime credit card borrowers (i.e., borrower credit scores < 620) for the high-exposure banks is lower than that of the low-exposure banks. While borrowers of the high- and low-exposure banks have a similar number of credit card accounts, the borrowers of the high-exposure banks have a significantly higher credit card balance (by \$1,192.50) and a lower utilization ratio (by 2.36 percentage points) than the low-exposure banks. This implies that borrowers of the high-exposure banks have, on average, significantly higher credit card limits.

Further, Table 3.1, Panel B, shows that, despite higher credit card balances and similar debt-to-income ratios, borrowers of the high-exposure banks have a lower delinquency rate on their credit cards relative to the borrowers of the low-exposure banks. In terms of other debt, the borrowers of the high-exposure banks have about one more debt-related account, a higher mortgage balance, and a lower auto balance than the borrowers of the low-exposure banks. Despite the differences in the composition of debt, it is important to note that the overall debt-to-income ratios are similar for the borrowers in both groups.

Overall, the summary statistics in Table 3.1, Panel B, are consistent with the statistics in Table 3.1, Panel A and show that the high-exposure banks had a better lending quality than the low-exposure banks. To the extent that better-quality borrowers can handle adverse shocks that are correlated with the negative shock to their bank's wholesale funding, the summary statistics imply that the wholesale funding supply shock transmitted from the banks to their borrowers is negatively correlated with the borrowers' demand shocks. In other words, any confounding borrower-related demand shocks that can bias our results should work against finding the proposed relationship between a bank's short-term wholesale funding shock exposure and the cut in credit limits.

3.3.4 Preliminary bank-level results

Figure 3.3 provides evidence for our baseline result at the bank level using the BHC Y-9C regulatory filings data. We partial out (orthogonalize) our exposure measure, and the log-change in credit card loans with respect to bank size by regressing them against the log of total assets and obtaining the residuals. This procedure adjusts for the fact that banks that have a greater dependence on wholesale funding are significantly larger, as reported in the summary statistics in Table 3.1. Figure 3.3 shows that banks that have a greater dependence on short-term wholesale funding experienced a greater reduction in credit card loans on their balance sheets from the pre- to the post-shock period. However, aggregated bank-level data cannot distinguish between credit supply and credit demand effects. Thus, we rely on the credit card–level data to infer the credit supply–driven effects of the short-term wholesale funding liquidity shock.

Figure 3.4 provides evidence for the necessary conditions for our empirical tests. First, Subfigure 3.4a provides supporting evidence for our main assertion that banks with a greater dependence on short-term wholesale funding experienced a greater reduction in short-term wholesale funding in the post-shock period. Subfigures 3.4b, 3.4c, and 3.4d plot changes in the total wholesale funding, total liabilities, and equity capital. These figures indicate that banks could not make up for the funding gap using other sources of funding after the loss of short-term wholesale funding.[12]

[12] Table C.2 shows the statistical significance of the relationships plotted in Figure 3.3 and 3.4.

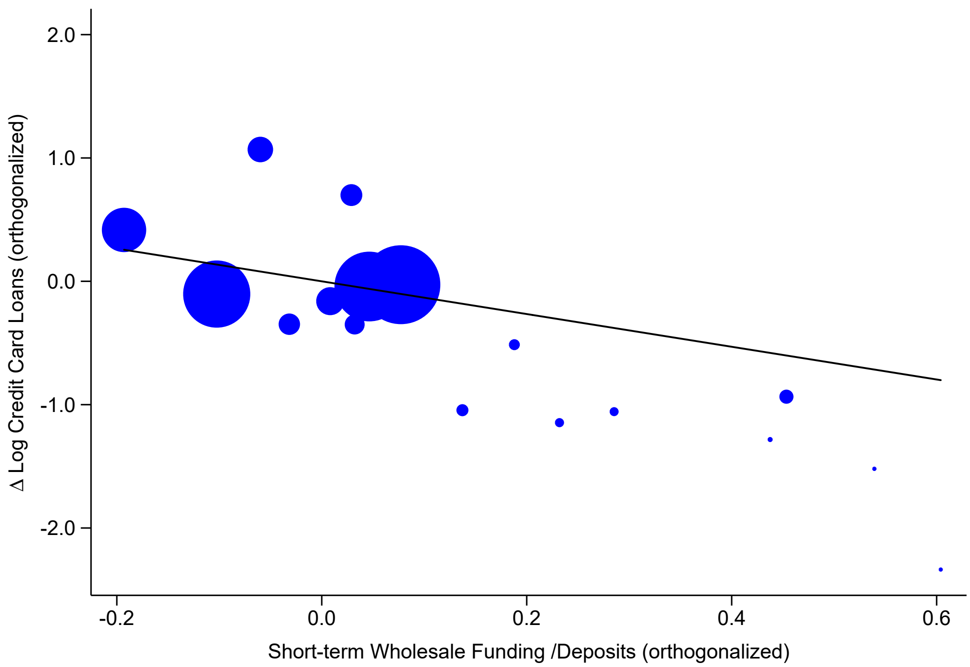
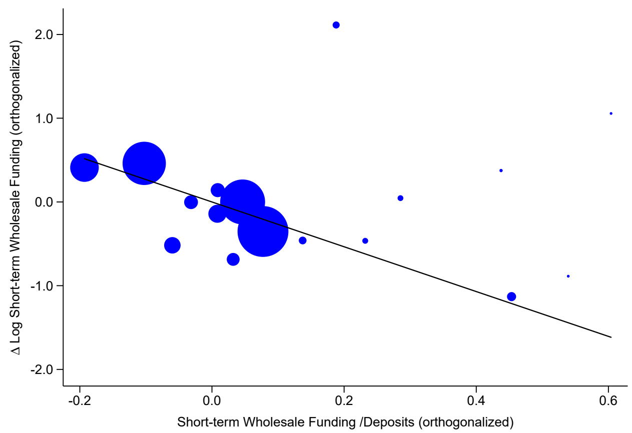
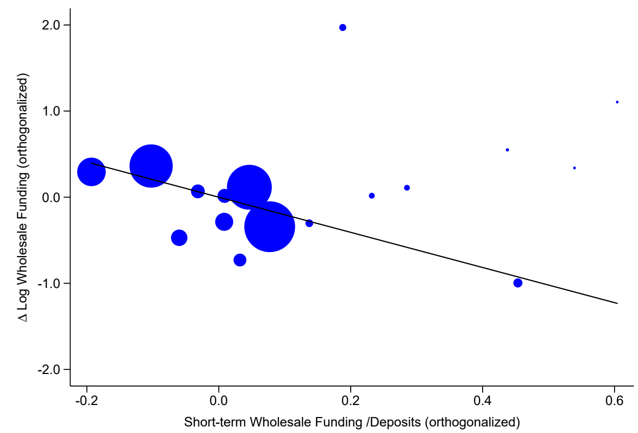


Figure 3.3: Bank exposure and change in credit card loans

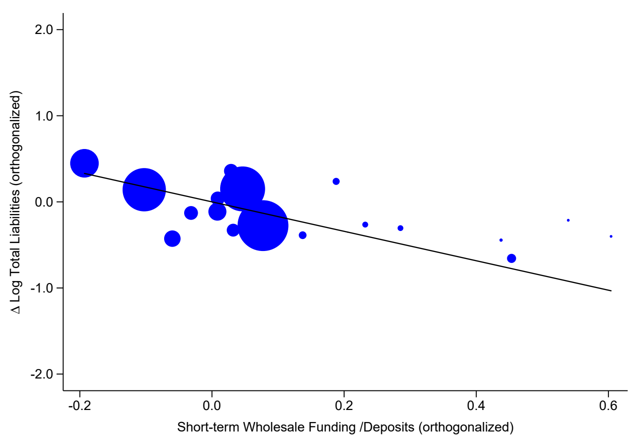
The figure plots the change in credit card loans from the pre-shock to the post-shock period as a function of our main independent variable of interest, *Exposure*, which is defined as the ratio of a bank's short-term wholesale funding to its total deposits. However, before plotting, we partial out (orthogonalize) *Exposure* and the log-change in credit card loans with respect to the log of total bank assets to account for the fact that larger banks, in general, have a greater dependence on wholesale funding (see Table 3.1). The pre-shock period ranges from 2006Q1 to 2007Q4, and the post-shock period ranges from 2009Q1 to 2010Q4. The points on the graph are plotted proportional to bank size as measured by the average total assets in the pre-shock period. The data for the plot below are gathered from the quarterly Y-9C filings of U.S. BHCs.



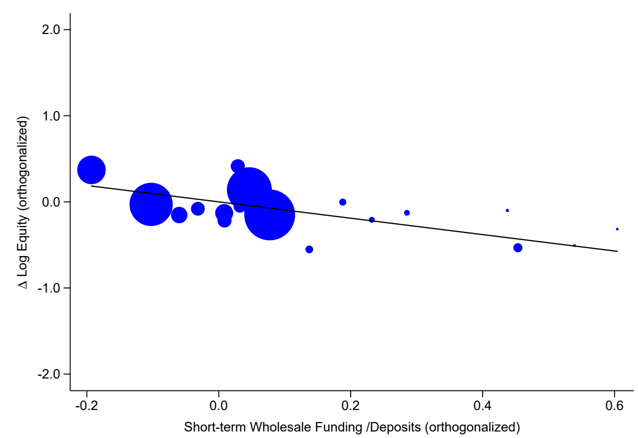
(a) Δ*Short-term wholesale funding*



(b) Δ*Wholesale funding*



(c) Δ*Total Liabilities*



(d) Δ*Equity capital*

Figure 3.4: Bank exposure and change in bank funding

The figure plots the change in bank funding measures from the pre-shock to the post-shock period as a function of our main independent variable of interest, *Exposure*, which is defined as the ratio of a bank's short-term wholesale funding to its total deposits. However, before plotting, we partial out (orthogonalize) *Exposure* and bank funding measures with respect to bank assets to account for the fact that larger banks, in general, have a greater dependence on wholesale funding (see Table 3.1). The pre-shock period ranges from 2006Q1 to 2007Q4, and the post-shock period ranges from 2009Q1 to 2010Q4. The points on the graph are plotted proportional to bank size as measured by the average total assets in the pre-shock period. The data for the plot below are gathered from the quarterly Y-9C filings of U.S. BHCs.

3.4 Results

A funding shock increases a bank's marginal cost of extending credit because it increases the bank's funding costs. Banks can pass on their higher funding costs to consumers either through price (e.g., increasing interest rates on credit card borrowing) or through quantity (e.g., reducing credit limits). However, interest rates on credit cards tend to be relatively inelastic ([83, 54]). Moreover, an increase in interest rates can lead to adverse selection issues since only the riskier borrowers are willing to borrow ([83, 109]). Thus, credit limits, as opposed to interest rates, are more likely to be the primary margin of adjustment for banks as they transmit their funding shocks to consumers through credit cards. We first estimate the average effect of the wholesale funding shock on individuals. Next, we examine the heterogeneous effects of the wholesale funding shock across individuals because the heterogeneity of information issues (e.g., moral hazard, adverse selection) can affect the cost of extending credit differentially across consumers ([79]).

3.4.1 Credit card–level results

Effect of the funding shock on credit limits

In this section, we use the granular credit card–level data to estimate the effect of the wholesale funding shock on a bank's credit supply. As described in Section 3.2, we isolate changes in credit limits at the credit card–level in the presence of $Individual$ fixed effects. Table 3.2 documents these results, which are obtained by estimating Equation 3.1. Our regression sample consists of 158 million credit card accounts belonging to 54 million individuals after applying the filters described in Section 3.3 to the raw data from the credit bureau, which cover the entire U.S. population. We also standardize the exposure variable for ease of interpretation. Finally, all control variables are constructed by averaging over the pre-shock period.

Table 3.2: Effect of funding shock on credit card limits

This table shows the relation between the pre-shock dependence on short-term wholesale funding and the change in credit card limits using the credit bureau data. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately in the pre-shock and post-shock periods by averaging across time. Then, the change in credit card limits is computed by taking the log difference from the post- to the pre-shock period. The dependence on short-term wholesale funding is measured as the ratio of short-term wholesale funding to total deposits. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

	Individual FE				OLS
Depvar: Δ*CC Limit*	(1)	(2)	(3)	(4)	(5)
Exposure	-3.811***	-6.613***	-5.050***	-4.750***	-4.035***
	(-9.85)	(-16.11)	(-13.32)	(-12.89)	(-8.66)
Bank characteristics					
Assets (log)	-1.353***	-1.068***	-0.362***	-0.359***	-0.519***
	(-17.98)	(-33.86)	(-4.62)	(-4.85)	(-5.95)
Assets^2 (log)	0.035***	0.028***	0.010***	0.010***	0.015***
	(17.99)	(33.40)	(5.00)	(5.22)	(6.39)
Risk-based capital ratio	-0.021***	-0.020***	-0.004*	-0.004**	-0.006***
	(-14.89)	(-23.54)	(-1.74)	(-1.98)	(-2.67)
CC business (%)	-0.001***	-0.002***	-0.002***	-0.001***	-0.001***
	(-9.12)	(-16.88)	(-9.45)	(-9.35)	(-6.19)
Bank performance					
ROE		3.059***	1.351***	1.366***	0.778**
		(11.51)	(4.36)	(4.62)	(2.02)
Non-perf loans (%)		-0.182***	-0.026	-0.020	0.052
		(-5.24)	(-0.85)	(-0.66)	(1.37)
Lending quality					
Avg. credit score			-0.001*	-0.001*	-0.002***
			(-1.86)	(-1.78)	(-3.20)
Avg. DTI			-0.035***	-0.035***	-0.040***
			(-6.17)	(-6.22)	(-5.72)
Avg. CC balance (log)			0.301***	0.283***	0.196***
			(7.51)	(7.10)	(3.99)
Avg. mortgage balance (log)			0.041	0.051	-0.012
			(0.97)	(1.10)	(-0.25)
Credit card controls					
CC utilization				0.002***	0.001***
				(23.57)	(8.07)
Months CC open (log)				-0.006**	-0.016***
				(-2.13)	(-6.18)
Accounts open (log)				0.023***	-0.023***
				(6.94)	(-8.09)
N	158,432,533	158,432,533	158,432,533	158,432,533	158,432,533
Adj. R^2	0.072	0.082	0.084	0.090	0.036

The summary statistics in Table 3.1 show that larger banks, in general, have a greater fraction of wholesale funding. Therefore, in Table 3.2, Column (1), we control for bank size by including the logarithm of bank assets and its square to account for any nonlinear effects due to bank size in our analysis. Column (1) also controls for a bank's ability to sustain losses by including the risk-based capital ratio. To mitigate the concern that our results are driven by correlated shocks to the credit card industry, we also include the amount of credit card loans as a fraction of the bank's assets as a control in Column (1). After controlling for these initial sets of bank characteristics, we find that the coefficient of interest, which is associated with the $Exposure$ variable, is negative and statistically significant. This indicates that the banks that had a greater dependence on short-term wholesale funding in the pre-shock period reduced credit limits more in the post-shock period. In Column (2) and Column (3), we show that our point estimates remain virtually unchanged even after controlling for the performance and lending quality of the bank. These specifications suggest that our results are unlikely to be driven by banks with poor fundamentals such as poor performance, more risk-taking, or a low-quality customer base.

In Column (4), we also control for credit card–specific and individual–bank-specific characteristics than can affect credit limits, such as the age of the credit card account and the relationship between an individual and her credit card–issuing bank. We measure the relationship between an individual and her credit card–issuing bank by observing the number of open accounts (e.g., mortgage loans, auto loans) that the individual has with the bank. Column (4) show that our results remain unchanged, indicating that such individual–bank-specific factors are less likely to confound our results. The coefficient estimated in Column (4) suggests that a one standard deviation increase in the dependence on short-term wholesale funding in the pre-shock period leads to a 4.75% decline in credit card limits from the pre- to post-shock period.[13] Given that the average pre-shock credit limit is $9,131.60, a one standard deviation increase in a bank's exposure to the short-term wholesale funding shock decreases the credit limit by $434, on average. Or equivalently, a 10 percentage point

[13]The standard deviation of the exposure measure is 16%.

(pp) increase in a bank's short-term wholesale funding dependence in the pre-shock period leads to a 3% ($271) decline in credit limits in the post-shock period, on average.

In Column (5), we re-estimate the specification in Column (4) without individual fixed effects. We note that the coefficient associated with the $Exposure$ variable is relatively unchanged. This implies that the individual demand factors, which we absorb using the fixed-effects specifications in Column (4), is mostly uncorrelated with the *Exposure* variable. This result is useful when we attempt to trace the impact of the wholesale funding shock on an individual's aggregate credit card balances through the changes in credit limits.

In Figure 3.5, we provide evidence for *parallel trends* in the credit limits extended by the high- and low-exposure banks in the pre-shock period. Figure 3.5 attempts to replicate the cross-sectional "within" individual analysis in Table 3.2 over time (see [17]). The plot in Figure 3.5 is equivalent to first obtaining the residual from regressing credit limits on *Individual*×*Archive* fixed effects, then plotting the residual after sorting it based on whether it is associated with a high- or a low-exposure bank. However, given the large number of fixed effects that must be employed in such a regression, we choose to implement this task in the following equivalent manner.[14] For each individual in every semiannual archive, we compute the deviation of each credit card's credit limit from the individual's mean credit limit in that archive. Next, we sort the deviations into two groups based on whether the individual's credit card was issued by a high- or a low-exposure bank. Then, we plot the mean of the deviations computed for each group–archive in Figure 3.5. The high- and low-exposure bank groups are defined "within" individual, based on the mean of our wholesale funding exposure measure computed for each individual.

Figure 3.5 shows parallel trends for the (within-individual) credit limits extended by the high- and low-exposure banks in the pre-shock period. However, in the post-shock period, the credit limits for the cards issued by high-exposure banks trend lower relative to the mean individual credit limit.

[14] Our sample consists of 54 million credit card consumers and covers seven semiannual archives during our sample period from 2007–2010 to plot Figure 3.5. Thus, we have $54 \times 7 = 378$ million fixed effects.

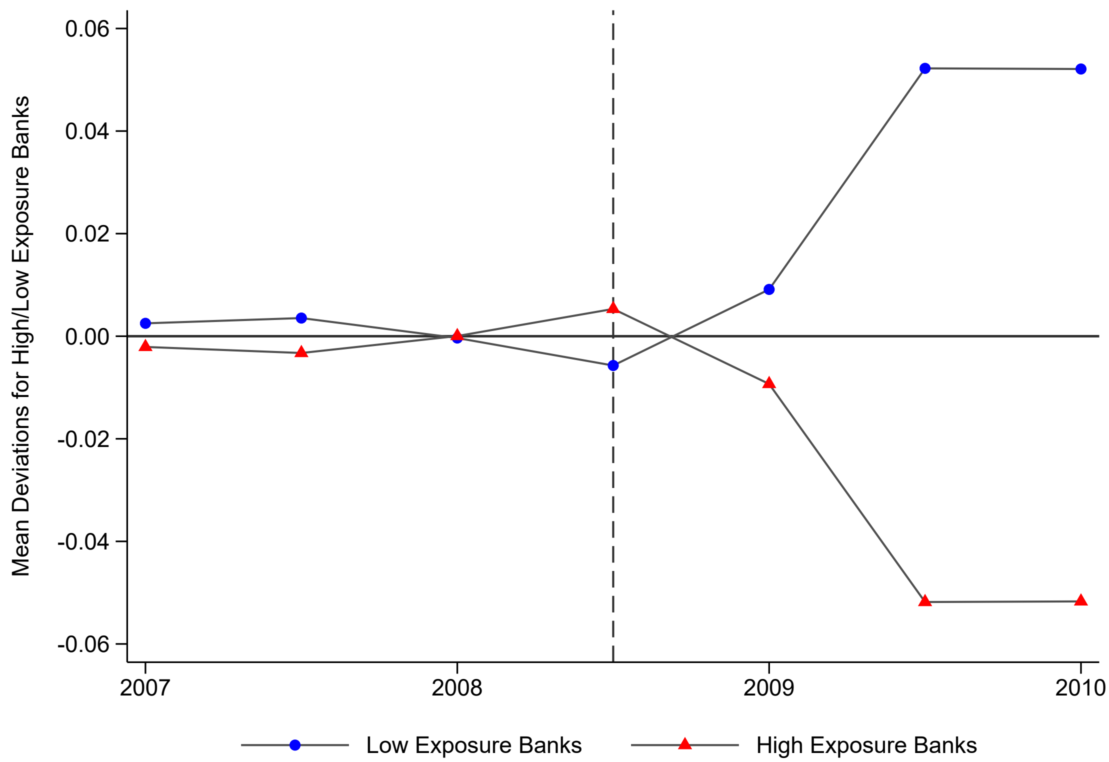


Figure 3.5: Credit limits extended *within*-individual over time

The figure plots the mean credit limit deviations computed "within" individual for the low- and high-exposure banks over time. For each individual in every semiannual archive, we compute the deviation of each credit card's credit limit from the individual's mean credit limit in that archive. Next, we sort the deviations into two groups based on whether the individual's credit card was issued by a high- or a low-exposure bank. Banks are classified as low- and high-exposure banks within each individual based on the mean exposure computed at the individual level. The figure plots the mean of the deviations computed across all credit cards in each semiannual archive for the low- and high-exposure banks separately. Our data sample is gathered from one of the three major credit bureaus in the U.S. and ranges from 2007–2010 at a semiannual frequency.

The within-individual credit limits trend for credit cards issued by the low-exposure banks is a mirror image of the trend for high-exposure banks because we sum up the deviations within each individual and archive. The unconditional mean change in credit limits from the pre- to post-shock period is -3.95% for the high-exposure banks and -0.30% for the low-exposure banks. These unconditional means indicate that our results are not driven by credit limit increases on cards issued by the low-exposure banks. Rather, these changes occur due to the credit limit reductions on cards issued by the high-exposure banks.

So far, our analysis has focused on the effects of the sudden decline in short-term wholesale funding on the credit limits extended on the intensive margin. Next, we estimate the effect of the bank liquidity shock on the extensive margin. Specifically, we test whether banks that were more affected by the shock are less likely to issue new credit cards and more likely to close existing credit card accounts. In Table C.3, Columns (1) and (2), we consider all new credit card accounts that were opened by the banks in our sample in the post-shock period, but did not exist in the pre-shock period. We define an indicator variable $New_{i,c,b}$, which takes the value 1 if a new card c is issued to individual i by bank b in our post-shock period, and takes the value 0 otherwise. Column (1) estimates the OLS regression and shows that a one standard deviation increase in a bank's dependence on short-term wholesale funding reduces its likelihood of issuing new credit cards by 1.97%. In Column (2), we include *Individual* fixed effects to control for individual demand factors that might affect the issuance of credit cards to an individual. The fixed effects estimator in Column (2) is slightly higher and indicates that banks are 2.34% less likely to issue new credit cards in the post-shock period if they had a one standard deviation greater dependence on short-term wholesale funding in the pre-shock period.

Similarly, we define an indicator variable $Closed_{i,c,b}$, which takes the value 1 if a card c issued to individual i by bank b was closed in the post-shock after the sudden decline in short-term wholesale funding, and takes the value 0 otherwise.[15] Columns (3) and (4) present results for the closed credit card accounts. The point estimates with *Individual* fixed effects (Column (4)) and without them (Column (3)) are similar. These results suggests that a bank with one standard deviation greater exposure to the short-term wholesale funding shock was 4.35% more likely to close credit cards in the post-shock period. Overall, our extensive margin results suggest that banks that were more

[15]Creditors can close a credit card account with no advance notice if (a) the card is inactive, (b) the creditor no longer offers the same terms on the credit card, or (c) the borrower has defaulted. Creditors can also close credit card accounts for undisclosed reasons (see https://blog.equifax.com/credit/credit-tips-what-to-do-when-an-issuer-closes-your-credit-card/)

exposed to the short-term wholesale funding shock were more likely to close existing cards and less likely to open new credit cards.

Effect of the funding shock on credit balances

Ex ante, controlling for individual demand factors, it is not obvious how changes in credit limits should affect credit card balances. For instance, if consumers are not liquidity constrained then a change in credit limits should not lead to a change in credit card balances.[16] Table 3.3 shows how consumers change their their credit card usage when it experiences a credit limit cut. In Column (1) we estimate the OLS regression to obtain the relation between credit limit changes and credit balance changes at the credit card level. We find that a 1% reduction in credit limits leads to a 0.74% increase in credit card balances. It is important to note that Column (1) does not include Individual fixed effects and thus captures the cross-sectional variation across individuals.

Clearly, endogenous demand factors can bias the results in Column (1) because consumer demand is arguably the primary driver of changes in credit balances. For example, consumers can receive an increase in credit limits by applying for it with the intention of utilizing more credit in the future. Similarly, if lenders anticipate future demand changes (e.g., a reduction in future income), then they could reduce credit limits in anticipation of such demand factors. Therefore, in Column (2), we re-estimate our specification in Column (1) with *Individual* fixed effects. This allows us to control for individual demand factors and compare how credit limit changes affect credit balance changes within individuals who use multiple credit cards. The *Individual* fixed effects estimator in Column (2) shows that a 1% change in credit limits leads to a 0.85% change in credit card balances.

[16]For instance, under the permanent income hypothesis, credit limit changes should not affect credit balances or consumption. However, if liquidity constraints are binding, or if they are expected to bind in the future (buffer stock models), then credit limit changes can lead to changes in credit balances or consumption.

Table 3.3: Effect of funding shock on credit card balances

The table shows the relation between the pre-shock dependence on short-term wholesale funding and the change in credit card balances at the credit card level. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately in the pre-shock and post-shock periods by averaging across time. Then, the change in credit card balance is computed by taking the log difference from the post- to the pre-shock period. The dependence on short-term wholesale funding (exposure) is measured as the ratio of short-term wholesale funding to total deposits. In Column (5), Δ *CC limit* is instrumented by the *Exposure* variable. Column (6) re-estimates the specification in Column (5) by using dollar changes in credit limits and balances. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar:	ΔCC Balance					$ΔCC Balance
	(1)	(2)	(3)	(4)	(5)	(6)
Δ CC limit	0.744*** (46.40)	0.854*** (25.05)				
Exposure			-3.080 (-1.02)	-9.805*** (-4.57)		
Δ CC limit (instrumented)					2.064*** (4.52)	
$Δ CC limit (instrumented)						0.235*** (7.30)
Individual FE		✓		✓	✓	✓
Bank characteristics	✓	✓	✓	✓	✓	✓
Bank performance	✓	✓	✓	✓	✓	✓
Lending quality	✓	✓	✓	✓	✓	✓
Credit card controls	✓	✓	✓	✓	✓	✓
N	158,432,533	158,432,533	158,432,533	158,432,533	158,432,533	158,432,533
Adj. R^2	0.038	0.164	0.024	0.146	0.128	0.273
F-stat (Excl. instru)					97.1	177.15

Columns (3) and (4) estimate the OLS and the *Individual* fixed effects estimator using our bank *Exposure* variable to the short-term wholesale funding shock. Both columns suggest that the credit card consumers of banks that had a greater exposure to the short-term wholesale funding shock reduced balances on those credit card to a greater extent. The *Individual* fixed effects estimator in Column (4), which corrects for changes in individual demand factors, indicates that a one standard deviation increase in a bank's exposure to the short-term wholesale funding shock leads to reduction of 9.81% in balances on the credit cards issued by the bank. A comparison of the estimates in Columns (3) and (4) reveals that the individual demand factors are positively correlated with our bank *Exposure* variable resulting in a positively biased coefficient in Column (3). This suggests that omitting demand factors should work against finding the negative relation between a bank's exposure to the liquidity shock and the reduction in the balances on the credit cards issued by the bank.

In Column (5), we estimate the 2SLS specification in Equation 3.2, which instruments the change in credit card limits from the pre- to post-shock period with our bank *Exposure* variable. Therefore, the estimate in Column (5), which captures the local average treatment effect (LATE), shows that a 1% reduction in credit limits due to the short-term wholesale funding shock reduces credit card balances by 2.06%.[17] In Column (6), we re-estimate Column (5) by using dollar changes in credit limits and credit balances instead of log changes. The result suggests that $1 dollar decrease in credit limits led to 23.5 cents lower credit balances. This point estimate, which shows the marginal propensity to consume (MPC) out of an additional $1 in credit card liquidity, is consistent with previous studies (e.g., [48]).

Are the results being driven by individual-bank-specific demand?

If endogenous demand factors exist only at the individual level, then our fixed-effects specification can control for them entirely. However, if there are confounding demand factors at the credit card level, then our results could still be biased. For instance, if individuals prefer to use certain credit cards over others, then the true measure of individual demand will be reflected only through those frequently used credit cards. Moreover, if banks tend to reduce credit limits for those cards that are infrequently used, then our results should matter less economically. Therefore, in Table 3.4, Panel A, we consider only the active credit cards in our sample. These are cards that had a nonzero balance in both the pre- and the post-shock period. This additional condition results in dropping approximately

[17] When consumers substitute away from a more affected card to a less affected card, a balance reduction on the more affected card is a balance *gain* on the less affected card. So the treatment effect captures the sum of these two effects, and thus is more than a one-for-one effect. Further, as balances are smaller than limits, the same dollar change is a greater percentage on balances than limits.

24% of the credit card accounts in our sample.

Column (1) shows how a bank's exposure to the short-term wholesale funding shock affects the change in credit limits for the subsample of active cards. We find a stronger effect compared to the baseline results in Table 3.2. Namely, a one standard deviation increase in the dependence on short-term wholesale funding leads to a 5.20% reduction in the credit limits of an actively used card. This suggests that the credit cards that were in greater use saw a larger reduction in credit limits in the post-shock period, which makes our analysis economically relevant. Column (3) re-estimates the IV specification in Table 3.3, Column (5). We find that a 1% reduction in credit limits for active cards due to the short-term wholesale funding shock leads to a 3.42% decrease in credit card balances for active cards. This effect is about 1.6 times greater than the estimate in Table 3.3, Column (5), which also includes cards that are not actively used. Moreover, recall that the fixed-effects specification for the actively used cards should be better able to control for the endogenous changes in the demand-side factors at the individual level. Thus, it is likely that the higher IV estimate for the active credit cards subsample better reflects the LATE stemming from the financially constrained individuals.

To further mitigate endogeneity concerns due to individual-bank-specific demand factors, we construct a "leave-out-mean" credit limit change for each credit card. For every individual i's credit card c issued by bank b, we first compute the "leave-out-mean" credit limit $CreditLimit_{i,-c,b}$ as the average credit limit using all the credit cards issued by bank b except credit card c. Next, we compute the change in this "leave-out-mean" credit limit for each credit card.[18] By construction, this measure excludes the credit limit changes made by a bank due to individual-bank-specific demand factors such as individual requests for increases in credit limits. At the same time, this measure captures the bank's average change in credit supply through its issued credit cards.

[18] The change in credit limit for the new supply measure for an individual i's credit card c is given by the log difference: $\Delta CreditLimit_{i,-c,b} = Log\left(\sum_{c\neq j} CreditLimit_{i,j,b}^{post-shock}\right) - Log\left(\sum_{c\neq j} CreditLimit_{i,j,b}^{pre-shock}\right)$.

Table 3.4: Effect of funding shock on credit cards: Robustness

The table shows the robustness of the relation between the pre-shock dependence on short-term wholesale funding and the change in credit card limits and balances. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately in the pre-shock and post-shock periods by averaging across time. Then, the change in credit card limits and balances are computed by taking the log difference from the post- to the pre-shock period. The dependence on short-term wholesale funding (shock exposure) is measured as the ratio of short-term wholesale funding to total deposits. The standard errors are clustered at the bank level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: $\Delta CCLimit$ using active cards only

Depvar:	ΔCC Limit	ΔCC Balance	
	(1)	(2)	(3)
Exposure	-5.204***	-17.793***	
	(-14.58)	(-8.77)	
Δ CC limit (instrumented)			3.419***
			(8.18)
N	120,497,687	120,497,687	120,497,687
Adj. R^2	0.099	0.192	0.104
F-stat (Excl. instru)			212.629

Panel B: $\Delta CCLimit_{i,-c,b}$: computed independent of credit card-specific limits

Depvar:	ΔCC Limit	ΔCC Balance	
	(1)	(2)	(3)
Exposure	-4.254***	-9.805***	
	(-18.11)	(-4.57)	
Δ CC limit (instrumented)			2.305***
			(4.57)
N	158,432,533	158,432,533	158,432,533
Adj. R^2	0.981	0.146	0.152
F-stat (Excl. instru)			251.00

Panel C: Homeowner

Depvar:	ΔCC Balance	
Homeowner:	No	Yes
	(1)	(2)
Δ CC limit (instrumented)	1.809***	2.213***
	(2.95)	(5.36)
N	68,474,761	89,957,772
Adj. R^2	0.127	0.133
F-stat (Excl. instru)	108.469	199.268

Controls for all specifications in Panel A, B, and C

Individual FE	✓
Bank characteristics	✓
Bank performance	✓
Lending quality	✓
Credit card controls	✓

We re-estimate our baseline model with the leave-out mean credit limit change measure in Table 3.4, Panel B. The point estimate in Column (1) indicates that a one standard deviation increase in bank exposure to short-term wholesale funding led to a -4.30% reduction in credit limits. This point estimate is remarkably similar to the point estimate of -4.75% in the comparable specification shown in Table 3.2, Column (4), with individual fixed effects. This further suggests that individual-bank-specific demand factors are less likely to influence our results, and individual fixed effects seem to adequately control for confounding demand-related factors in our setting. Similarly, for the credit card balance regressions, the point estimates in Columns (2) and (3) of Table 3.4, Panel B, are similar to the point estimates with comparable specifications shown in Table 3.3, Columns (4) and (5), with individual fixed effects.

In Table 3.4, Panel C, we mitigate the concern that our results might still be demand driven due to household balance sheet effects in the post-2008 period after the housing market crash. [91] show that areas with greater house price declines and more levered households reduced their consumption more in the post-2008 period. Their results highlight the role of negative housing-wealth shocks and debt overhang in reducing household consumption (see also [92]). Thus, our results might be confounded by the household balance sheet effect if homeowners are more likely to borrow from banks that have a greater dependence on short-term wholesale funding. To address this concern, we re-estimate the model in Table 3.3, Column (5), by splitting the sample based on whether an individual owns a home. Table 3.4, Panel C, shows that the point estimates for homeowners and non-homeowners are similar, suggesting that our results are less likely to be confounded by the household balance sheet channel.

Are the results driven by other differences across banks?

A potential concern is that our results are driven by differences in other characteristics between the high- and low-exposure banks, as opposed to their differential exposure to the short-term wholesale funding shock. For instance, the summary statistics in Table 3.1 show that banks dependent on wholesale funding were larger banks. It is plausible that larger banks were subject to greater scrutiny and regulation in the post-2008 period, which led them to be more conservative while extending credit. To mitigate such a concern, we classify the banks into large (above median) and small (below median) size groups, and we include the interactions of the size indicator variable with the *Individual* fixed effects in our regression specification (i.e., $Individual \times SizeGroup$ fixed effects). First, these fixed effects control for any differences in the credit extended by smaller and larger banks to consumers in a flexible and nonparametric manner. Moreover, the coefficient of interest, which is associated with *Exposure*, is also identified *within* large and small banks. In other words, if the reduction in credit limits in the post-shock period is in fact driven by differences in bank size, then the $Individual \times SizeGroup$ fixed effects should subsume all the variation in the *Exposure* variable across banks and render its estimated coefficient statistically insignificant.

Table C.4, Column (1), presents the results after including the $Individual \times SizeGroup$ fixed effects. The coefficient associated with *Exposure* in Table C.4, Column (1), is -4.79. This coefficient is practically unchanged when compared to the coefficient of -4.75 in our baseline specification in Table 3.2, Column (4), with *Individual* fixed effects and the same set of control variables. Thus, our baseline results, which show a reduction in credit limit due to the wholesale funding liquidity shock, are unlikely to be driven by differences in size between the high- and low-exposure banks. Similarly, based on the summary statistics in Table 3.1, one might argue that the high-exposure banks were riskier because they had lower capital ratios, on average, and they suffered greater losses in the post-shock period as a consequence. Table 3.1 also shows that high-exposure banks have lower utilization ratios, and they may have sought to reduce their unused credit card commitments in the post-shock period because such commitments are costly to maintain. Table 3.1 also shows that the high-exposure banks were lending to relatively safer consumers in the pre-shock period. Thus, it is plausible that the high-exposure banks were more risk-averse and consequently reduced credit limits more in the post-shock period.

In order to mitigate the above potential concerns, we follow the same empirical strategy as we did with bank size in Table C.4, Column(1). We classify banks into above-median and below-median groups based on their capital ratios, their unused credit card commitments, and their percentage of subprime consumers. Next, we interact the *Individual* fixed effects with the aforementioned group indicator variables, and include them in our analysis. These results are presented in Table C.4, Columns (2)–(4), and they remain qualitatively unchanged when compared to our baseline specification in Table 3.2, Column (4). Overall, our results are robust to controlling for the potential differences in bank characteristics that could drive our results.

We also show that our results are not driven by any particular bank. We re-estimate the baseline regression specification shown in Table 3.2, Column (4), by excluding one bank from the analysis each time and estimating the regression on the sample consisting of the remaining 17 banks. Consequently, there are 18 such regressions, and we plot the 18 estimated coefficients of interest associated with $Exposure$, along with their standard errors, in Figure 3.6. As can be seen, the estimated coefficients are significantly negative and relatively stable across all the 18 specifications, which indicates that our results are not driven by any particular bank.

Other robustness tests

We also show that our results are unchanged if we conduct the analysis at the individual–bank level as opposed to our baseline models in Equation (3.1) and Equation (3.2) which are at the individual–bank–card level. If an individual has multiple credit cards from a bank, then we sum the limits and balances across those multiple credit cards to construct our dataset at the individual–bank level. We also aggregate our credit card-level controls (i.e., *CC utilization*, *Months CC open*) to the individual–bank level. We compute the *CC utilization* at the individual–bank level by dividing the summed balance by the summed limit. *Months CC open* at the individual–bank level is assigned based on the oldest credit card issued to the individual by the bank. The results using the individual–bank level analysis are qualitatively similar to baseline results and are presented in Table C.5 in the Appendix. However, we prefer our baseline analysis at the individual–bank–card level because it allows us to

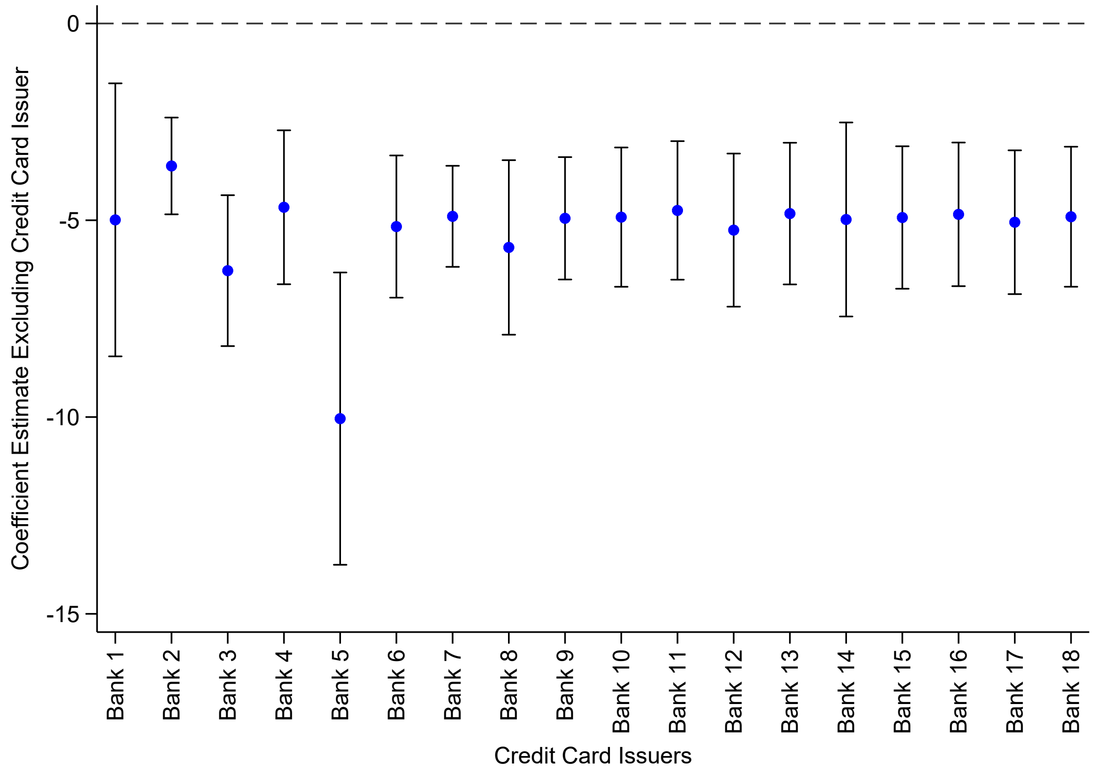


Figure 3.6: Estimation excluding each bank

This figure shows the coefficient β associated with our variable of interest, $Exposure$, from Equation 3.1 estimated by excluding each BHC from our sample to address the potential concern that a particular BHC might be driving our results. *Exposure* is defined as the ratio of a bank's short-term wholesale funding to its total deposits. The circles in the plot represent the coefficient estimate β associated with the $Exposure$ variable from estimating Equation 3.1 after excluding the bank shown below the plotted point on the *x*-axis. The solid vertical lines represent the 95% confidence intervals for the estimated coefficients. The point estimates are ordered based on bank size. Thus, "Bank 1" represents the coefficient estimate after removing the largest bank in our sample, and "Bank 18" represents the coefficient estimate after removing the smallest bank in our sample.

flexibly control for credit card level variables related to credit demand.

Table C.6 shows that our baseline results in Table 3.2 are robust to using alternate measures of bank exposure and using different levels of clustering. We use short-term wholesale funding as a fraction of total assets as an alternate measure for bank exposure in Columns (2) and (4), and we find that our results are unchanged. Consistent with Figure C.1, this suggests that our results capture the variation in the numerator of the bank exposure measure (i.e., a bank's dependence on short-term wholesale funding) as opposed to the denominator. We also cluster the standard errors at the bank level instead of the bank–state level as in Columns (3) and (4), and we find that our results are robust. Although the bank-level cluster is larger and can completely account for within-bank correlations, we cluster at the bank–state level for the rest of the analysis because we have only 18 banks and the standard errors can be biased when there are too few clusters ([45, 47]).

3.4.2 Individual-level results: Does the funding shock affect *total* credit card balances?

So far, we have provided evidence for how negative liquidity shocks to banks transmit through credit limits and result in lower balances on the affected credit card. In this section, we test whether the liquidity shocks to banks that are transmitted through credit limits have an aggregate effect on credit card borrowing and spending by consumers.

If credit card consumers are hedged with respect to their bank's liquidity shocks, then the bank liquidity shocks transmitted through credit limits should not affect consumers' total credit card balances. However, if consumers are constrained, either due to high aggregate credit card utilization ratios or due to high costs of substituting to other credit cards, then the liquidity shocks transmitted from banks can have real consequences by reducing consumers' total credit card balances and consumption through credit cards. Our data allow us to test for the impact of the short-term wholesale funding shock on total credit card balances, because we can observe balances and credit limits on all the credit cards of a consumer in our pre- and post-shock period. Therefore, we aggregate the credit limits and credit balances for all the credit cards at the consumer level for the pre- and post-shock period, then we take the log-difference to construct the change in total credit card limits and balances from the pre- to the post-shock period.

Next, we construct a weighted average exposure measure at the consumer level called *Weighted Exposure*, which measures the exposure of a consumer to the funding shock through their credit limits. *Weighted Exposure* is constructed by weighting the bank's exposure variable (i.e., the ratio of short-term wholesale funding to deposits) associated with a credit card by its credit limit as a proportion of the consumer's total credit limit. For the credit card–issuing banks that are not among the 18 banks in our sample, we assume that their exposure to the short-term wholesale funding (and thus their exposure to the liquidity shock) is zero. If we rely on this assumption, we could misclassify some banks as unexposed banks when in fact they experienced a liquidity shock due to the contraction of short-term wholesale funding. As a result, some individuals would be misclassified as unexposed or low-exposure individuals when they are actually high-exposure individuals. However, such a misclassification will only underestimate any effect on the change in aggregate credit card limits and spending.[19]

Table 3.5 presents results for the impact of the short-term wholesale funding shock on total credit card balances. The coefficient associated with the *Weighted Exposure* variable in Column (1) captures the aggregate effect of the funding shock on total credit card balances. If credit card consumers are hedged with respect to their bank's liquidity shocks, then the coefficient associated with the *Weighted Exposure* should be close to zero and statistically insignificant.

It is also important to note that in Table 3.5, we do not control for *Individual* fixed effects because our unit of observation is at the individual level rather than at the credit card level. As a consequence, our results in Table 3.5 rely on a stronger identification assumption that credit demand and supply factors are uncorrelated. However, our credit card–level analysis shows that it is less likely that credit demand factors confound our analysis, because the coefficient associated with the exposure variable is similar with or without the addition of *Individual* fixed effects (see Table 3.3), and this coefficient also remains unchanged when we construct a leave-out mean credit supply measure for a credit card account by excluding that credit card's credit limit (see Table 3.4).

[19]This underestimation occurs for two reasons. First, the changes in credit limits and balances for the low-exposure individuals will be biased downwards. This should be the case, because our prior "within" individual credit card–level analysis, which was confined to the set of 18 banks that had a nonzero exposure to the short-term wholesale funding shock, shows that a high exposure to the liquidity shock negatively affects credit limits and balances. Second, the coefficient on the weighted average exposure measure captures the change in aggregate credit limits and balances for the high-exposure individuals relative to the low-exposure individuals.

In any case, to control for unobserved confounding factors, we include the 5-digit ZIP code fixed effects and controls for the individual's credit quality, such as the individual's pre-shock credit score, monthly income, debt-to-income ratio, credit card utilization, and credit card and mortgage balances. The ZIP code fixed effects allow us to compare the changes in credit limits and balances for two individuals *within* the same ZIP code. As a result, we can control for any common shocks at the ZIP code level (e.g., changes in unemployment, house prices) that can affect an individual's total credit card balance.

Table 3.5: Effect of funding shock on *aggregate* credit card balances

The table shows the relation between funding shock-induced changes in total credit card limits on total credit card balances. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately in the pre-shock and post-shock periods by averaging across time. Then, the credit card limits and balances are aggregated at the individual level and the change in total credit card limits and total balances are computed by taking the log difference from the post- to the pre-shock period. *Weighted exposure* is computed at the individual level by aggregating the weighted *Exposure* measure at the credit card–level, where the weights assigned to a credit card are proportional to its credit limit. In Column (4), Δ *Aggregate CC limit* is instrumented by the *Weighted exposure* variable. Column(5) re-estimates the specifiction in Column (4) by using dollar changes in credit limits and balances. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar:	Δ Agg. CC Limit	Δ Agg. CC Balance			\$$\Delta$ Agg. CC Balance
	(1)	(2)	(3)	(4)	(5)
Weighted exposure	-3.827***	-1.216**			
	(-9.56)	(-2.55)			
Δ Agg. CC limit			0.859***		
			(43.56)		
Δ Agg. CC limit (instrumented)				0.318***	
				(2.87)	
\$$\Delta$ Agg. CC limit (instrumented)					0.071***
					(13.03)
Zip-code FE	✓	✓	✓	✓	✓
Consumer quality	✓	✓	✓	✓	✓
N	133,501,009	133,501,009	133,501,009	133,501,009	133,501,009
Adj. R^2	0.027	0.032	0.141	0.098	-0.938
F-stat (excl. instru)				91.386	104.48

Table 3.5, Column (1) and Column (2), show that a one standard deviation increase in the weighted exposure measure reduces an individual's total credit limit by 3.83% and the individual's total credit card balance by 1.22% . Column (3) shows the OLS regression of the change in

total credit balance on the change in total credit limit. The result in Column (3) indicates that a 1% reduction in total credit limits leads to a 0.86% reduction in total credit card balances. Column (4) estimates the IV regression for the total credit balance changes on the total credit limit changes by instrumenting the total credit limit by the weighted exposure variable. The point estimates in Column (4) indicate that a 1% reduction in total credit limits leads to a 0.32% reduction in total credit card balances. This estimate is smaller than the IV estimates in Table 3.3, Column (5), which suggests that the effect of transmitted bank shock is smaller at the consumer level. However, the estimate in Column (5) is economically significant and thus suggests that the consumers were not able to completely hedge away the short-term wholesale funding shock. In Column (5), we re-estimate the specification in Column (4) by using dollar changes in credit limits and balances. The estimates suggest that a $1 reduction in total credit limit led to 7 cents reduction in total credit balance. The economic magnitude is comparable to that in [91], who estimate that a $1 decline in housing values is associated with 5-7 cents reduction in consumption.

3.5 Heterogeneity of the funding shock: Are all consumers equally affected?

3.5.1 Credit card–level limits

In this section, we examine whether banks transmit their funding shocks equally across all consumers. The cost of extending credit may vary significantly in the cross-section due to information issues such as moral hazard and adverse selection ([79, 110, 109, 14]). For instance, the marginal cost of extending credit should be higher for those individuals who are more likely to borrow out of it and then default.[20] Table 3.6 examines the heterogeneous effect of the short-term wholesale funding shock on credit limits across credit cards. In Panel A, we explore the cross-sectional cuts across credit cards that have different utilization ratios, because the marginal cost of extending credit

[20] For example, higher debt levels for consumers can cause higher defaults, because either (a) higher debt levels increase a consumer's sensitivity to liquidity shocks, (b) the consumer has an incentive to default strategically (i.e., moral hazard), or (c) consumers overborrow and default due to behavioral biases.

to such consumers should be higher, since they are more likely to borrow on it. We group the credit cards in our sample into three groups based on their pre-shock utilization ratios: *low* ($\leq$ 50%), *high* (50–90%), and *very high* ($>$ 90%). The results in Table 3.6, Panel A, show that a one standard deviation increase in a bank's exposure to the wholesale funding shock reduces credit card limits by 4.30% and 6.59% more for the high- and very high-utilization ratio credit cards, respectively, relative to the low-utilization credit cards. Overall, the results in Table 3.6, Panel A, suggest that banks transmitted the short-term wholesale funding shock disproportionately more to credit cards with a higher utilization ratio.

In Table 3.6, Panels B, we perform cross-sectional cuts at the individual-level utilization ratio, which is computed as the ratio of an individual's total credit balance to the individual's total credit limit. The results in Panel B are similar, but stronger when compared Table 3.6, Panel A. For instance, the results in Panel B indicate that a one standard deviation increase in a bank's exposure to the wholesale funding shock reduces credit card limits by 8.19% more for consumers with a total utilization ratio of greater than 90% relative to the consumer with a total utilization ratio of less than 50%. In Table 3.6, Panel C, we find similar results when we conduct cross-sectional cuts on the credit scores of consumers. Banks pass on their liquidity shocks to a greater extent to the subprime consumers (FICO $<$620) than the prime consumers (FICO$>$680).

Table 3.6: Heterogeneity in bank response to funding shock

This table shows how banks pass on the short-term wholesale funding shock differentially across consumers. The dependence on short-term wholesale funding (exposure) is measured as the ratio of short-term wholesale funding to total deposits. *CC utilization (50–90%)*, and *CC utilization* ($>$ 90%) are indicator variables that equal 1 if the utilization ratio for a credit card is between 50% and 90% or greater than 90%, respectively, and 0 otherwise. *Agg. utilization (50–90%)*, and *Agg. utilization* ($>$ 90%) are indicator variables that equal 1 if an individual's aggregate utilization ratios, computed using all the credit cards, is between 50% and 90% or greater than 90%, respectively, and 0 otherwise. *Near-prime*, and *Subprime* are indicator variables that equal 1 if an individual's credit score is between 620 and 680 or lower than 620, respectively, and 0 otherwise. The standard errors are clustered at the bank–state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Credit-card level utilization		Panel B: Individual-level utilization		Panel C: Credit score	
Depvar: Δ*CC Limit*	(1)	*Depvar:* Δ*CC Limit*	(2)	*Depvar:* Δ*CC Limit*	(3)
Exposure	-4.052***	Exposure	-4.232***	Exposure	-4.038***
	(-10.61)		(-10.87)		(-10.26)
Exposure×CC utilization (50–90%)	-4.298***	Exposure×Agg. utilization (50–90%)	-4.994***	Exposure×Near-prime	-4.145***
	(-10.61)		(-11.68)		(-9.92)
Exposure×CC utilization (>90%)	-6.587***	Exposure×Agg. utilization (>90%)	-8.185***	Exposure×Subprime	-7.887***
	(-15.16)		(-15.37)		(-14.88)
CC utilization (50–90)	9.099***				
	(19.55)				
CC utilization ($\geq$90)	7.997***				
	(10.32)				
Individual FE	✓		✓		✓
Bank characteristics	✓		✓		✓
Bank performance	✓		✓		✓
Lending quality	✓		✓		✓
Credit card controls	✓		✓		✓
N	158,432,533		151,449,029		158,423,518
Adj. R^2	0.089		0.089		0.089

3.5.2 Credit card–level balances

Next, we examine the heterogeneous effect of credit limit cuts on credit balances across individuals in Table 3.7, Panel A, at the credit card level. Consumers can substitute their affected credit cards (i.e., cards with a credit limit cut) for other credit cards or other sources of credit (e.g., personal loans, home equity line of credit). However, the ability to substitute away from an affected card can vary across individuals. For instance, consumers with a high total credit card utilization across all their cards should be less able to substitute away from their affected credit cards to their other credit cards. As a result, for such individuals, a credit limit cut on their affected cards should have a smaller effect on the credit balance on that card.

In Table 3.7, Panel A, we re-estimate our instrumented specification from Equation 3.2 by splitting our sample based on how easily a consumer can substitute away from an affected credit card. Table 3.7, Panel A, shows that the relation between changes in credit limits and changes in credit balances at the credit card level is positive and stronger for individuals with lower aggregate utilization. For instance, consumers with a low aggregate credit card utilization ratio (0–50%) reduce their balances on affected credit cards by 2.65% for a 1% reduction in the credit limit on that card. However, consumers with a higher aggregate credit card utilization appear to be unable to reduce their spending in response to a credit limit cut. We also split our sample based on a consumer's creditworthiness (measured by the FICO score), which proxies for the consumer's ability to substitute the affected credit card with other credit cards. Consistent with the results for aggregate utilization, the elasticity of credit balances to credit limits at the credit card level increases with the creditworthiness of the consumer.

Table 3.7: Heterogeneity in consumer response to funding shock

This table shows consumers' differential response to their credit limit cuts induced by their banks' short-term wholesale funding shock. Panel A shows cross-sectional variation in consumer response at the credit card-level similar to Table 3.3, Column (5). Panel B shows cross-sectional variation in consumer response at the individual level similar to Table 3.5, Column (4). Panel C is similar to Panel B, but the dependent variable is the total change in debt balances across all debt-related accounts of the consumer. Columns (1)–(3) consist of subsamples of individuals whose aggregate utilization ratios, computed using all the credit cards issued to them, is between 0–50%, 50–90%, and greater than 90%, respectively. Columns (4)–(6) consist of subsamples of subprime (< 620), near-=prime ($>= 620$ and < 680), and prime ($>= 680$) individuals, classified based on their credit score. The standard errors are clustered at the bank–state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Credit card balances

	Utilization			Credit score		
	0-50%	50-90%	90%+	Sub-prime	Near-prime	Prime
Depvar: Δ CC Balance	(1)	(2)	(3)	(4)	(5)	(6)
Δ CC limit (instrumented)	2.650***	-0.827**	-2.393***	-2.018***	-0.172	2.959***
	(5.58)	(-2.31)	(-3.15)	(-4.16)	(-0.51)	(6.04)
Individual FE	✓	✓	✓	✓	✓	✓
Bank characteristics	✓	✓	✓	✓	✓	✓
Bank performance	✓	✓	✓	✓	✓	✓
Lending quality	✓	✓	✓	✓	✓	✓
Credit card controls	✓	✓	✓	✓	✓	✓
N	121,040,448	23,846,428	13,111,138	17,331,618	25,369,629	115,731,258
Adj. R^2	0.102	0.131	-0.284	-0.326	0.197	0.069
F-stat (Excl. instru)	158.877	156.524	43.442	70.802	244.376	136.286

Panel B: Total credit card balances

	Utilization			Credit score		
	0-50%	50-90%	90%+	Sub-prime	Near-prime	Prime
Depvar: Δ Agg. CC Balance	(1)	(2)	(3)	(4)	(5)	(6)
Δ Agg. CC limit (instrumented)	0.232	1.078***	1.325***	1.475***	0.639***	0.120
	(1.14)	(21.11)	(44.16)	(36.59)	(12.50)	(0.66)
N	98,625,046	20,357,771	14,237,721	19,181,224	19,924,108	94,395,673
Adj. R^2	0.079	0.285	0.254	0.099	0.205	0.058
F-stat (Excl. instru)	45.184	147.687	250.590	251.084	132.804	58.953

Panel C: Total debt balances

	Utilization			Credit score		
	0–50%	50–90%	90%+	Sub-prime	Near-prime	Prime
Depvar: Δ Agg. Debt Balance	(1)	(2)	(3)	(4)	(5)	(6)
Δ Agg. CC limit (instrumented)	-0.890***	-0.001	0.199***	0.596***	0.123**	-0.723***
	(-4.91)	(-0.03)	(6.48)	(10.23)	(2.13)	(-5.43)
N	99,045,677	20,421,935	14,380,579	19,551,761	20,007,886	94,598,483
Adj. R^2	-0.238	0.058	0.117	0.070	0.073	-0.128
F-stat (Excl. instr)	45.863	148.713	249.066	246.854	133.895	58.965

Controls for Panels B and C

Zip-code FE	✓	✓	✓	✓	✓	✓
Consumer quality	✓	✓	✓	✓	✓	✓

3.5.3 Individual-level total credit card balances

Table 3.5 shows the average impact of the short-term wholesale funding shock on total credit card balances. However, as discussed previously, the impact of the funding shock should be greater for consumers who are more constrained and cannot costlessly substitute away from an affected bank's credit card. For instance, the transmission of the funding shock should have a greater impact on the total credit card balances of individuals who have higher aggregate credit card utilization. Thus, for such consumers, in contrast to the credit card–level analysis, a greater cut in their total credit card limits should also lead to a greater reduction in their total credit card balances—i.e., we should expect a positive relation between total credit limit changes and total balance changes at the consumer level for consumers who *cannot* easily substitute away from their affected credit cards. In Table 3.7, Panel B, we study the heterogeneous response of the total credit card balances to changes in total credit limits due to the transmission of the short-term wholesale funding shock by splitting the sample based on the aforementioned factors similar to Table 3.7, Panel A.

Consistent with our expectation, Columns (1)–(3) show that the relation between total credit limit changes and total credit balance changes at the consumer level monotonically increases with the aggregate utilization ratio. For instance, consumers with a low aggregate utilization ratio (0–50%) do not change their total credit card balances despite experiencing credit limit cuts due to the transmission of the short-term wholesale funding liquidity shock from their credit card–issuing bank. That is, consumers with lower aggregate credit card utilization ratios are hedged from bank liquidity shocks. However, for consumers with high aggregate utilization ratios ($> 90\%$), a 1% cut in total credit limits resulting from the transmission of the bank liquidity shock leads to an equivalent 1.33% reduction in total credit card balances.

In Columns (4)–(6), we split our sample based on a consumer's creditworthiness and her ability to access other less affected or unaffected credit cards proxied by her credit score. Consistent with the results for the sample split on aggregate credit utilization, we find that the elasticity of total credit balances to total credit limits at the consumer level monotonically decreases with the creditworthiness of the consumer. While prime borrowers' credit card balances are unaffected by the transmitted funding shocks, subprime borrowers reduce their total credit card balances by 1.48% for every 1% reduction in total credit limits.

3.5.4 Individual-level total debt balances

Section 3.5.3 indicates that the liquidity shocks to banks that are transmitted through credit card limits have an effect on the total credit card balances of consumers. This effect is more pronounced for credit-constrained consumers who have higher credit card utilization ratios or lower credit scores. In this section, we examine whether consumers seek other sources of credit to finance their consumption and offset their credit limit cuts. If so, who is able to hedge away the liquidity shocks transmitted through credit card limits by substituting to other sources of credit? We answer these questions by studying the change in total debt balances aggregated over all credit accounts for an individual. For instance, the total debt balance for an individual includes debt balances for home equity line of credit (HELOC) and personal installment loans, which are likely substitutes for credit card debt. We use the individual-level change in total debt balances as our dependent variable and conduct a similar IV analysis as in Table 3.7, Panel B. Table 3.7, Panel C, reports the results.

Column (1) in Table 3.7, Panel C shows that consumers with a low aggregate credit card utilization ratio (0–50%) do not exhibit a reduction in total debt balances, even though they experience credit card limit cuts due to the funding shock. However, Column (3), which analyzes the high aggregate credit card utilization ratio ($> 90\%$) consumers, indicates that a 1% reduction in total credit card limit resulting from the transmission of the funding shock leads to a 0.20% reduction in total debt balances. We find similar results when splitting the sample based on credit scores in Columns (4)–(6). Subprime and near-prime consumers have a 0.60% and 0.12% reduction in total debt balances, respectively, for a 1% reduction in total credit card limits, while prime consumers do not exhibit any reduction in their total debt balances in response to the short-term wholesale funding shock induced credit limit cuts.

Overall, these results show that the elasticity of total debt balances decreases in consumers' ability to hedge. Our results on total debt balances show that some individuals, such as consumers with higher utilization ratios or lower credit scores, were not able to hedge away the wholesale funding shock to their banks. As a result, these consumers were likely forced to reduce their aggregate borrowing, which lowers their ability to smooth consumption.

There is an important caveat for the exclusion restriction of the IV analysis in the total debt balance regressions in Table 3.7, Panel C. The IV's exclusion restriction may be violated if the short-term wholesale funding liquidity shock to banks also affects the credit supply of other types of consumer credit. That is, if consumers also borrow other types of credit from their credit card lenders, then the bank liquidity shock may be transmitted to the consumers' total debt balances through other types of credit in addition to credit card limits. However, this concern is somewhat mitigated since we use credit card limits as weights to compute our weighted exposure. Regardless, we present the reduced form analysis for Table 3.7, Panel C in Table C.7 where we regress the total debt balance change on our weighted exposure measure directly. The main takeaways from Table C.7 are unchanged as Table C.7 again shows that the high aggregate utilization ratio consumers and the subprime consumers could not hedge away from their banks' wholesale funding liquidity shock.

Next, we conduct a similar analysis as in Table 3.7, Panel C, with HELOC revolving debt, which is a substitute for credit card revolving debt. However, unlike the unsecured credit card debt, HELOC is secured by an individual's home. We compare the transmission of the wholesale funding shock through HELOCs and credit cards using a horse race and find that banks mainly transmit their funding shock through credit cards. Table C.8 presents these results. We compute a HELOC exposure measure for an individual in similar fashion as the credit card exposure measure for an individual: We weight the bank's exposure variable (i.e., the ratio of short-term wholesale funding to deposits) by the bank's HELOC credit limit to the individual as a proportion of the individual's total HELOC credit limit.[21] We then instrument the individual's HELOC credit limit change by the weighted HELOC exposure measure in our IV analysis.

In order to compare the credit card channel to the HELOC channel, we limit our analysis to consumers who have both a credit card and a HELOC. Table C.8 shows that the elasticity of the total debt balances to the instrumented credit card limits is largely unchanged even after adding the instrumented HELOC credit limits to our regressions. Moreover, the magnitude of the point estimates for the subprime and near-prime borrowers indicate that banks transmitted the wholesale funding shock primarily through credit cards as opposed to HELOCs even though both revolving credit accounts were available to banks for transmitting the funding shock. Moreover, we find that only 5.5% of the subprime borrowers and 11% of the near-prime borrowers in our sample have HELOCs. This again indicates the the importance of the credit card channel in terms of its availability to banks to pass on their funding shocks.

3.6 Long run effect of funding shock

Figure 3.7 plots the average credit limits extended by the high- and low-exposure banks relative to the pre-shock period. The average credit limits are computed by averaging the total credit limits extended by banks to all consumers across the high exposure (above median) and the low exposure (below median) bank groups. The figure shows that, relative to pre-shock levels, the aggregate credit limits extended by the high-exposure banks reduced more than the low-exposure banks in the first two years after the short-term wholesale funding liquidity shock. This trend is consistent with the high-exposure banks facing greater liquidity constraints than the low-exposure banks due to their greater exposure to the short-term wholesale funding liquidity shock. After the first two years, the aggregate credit limits extended by both types of banks recover close to their pre-shock levels within 10 years after the wholesale funding shock.

[21] Typically, individuals have only one HELOC account. Thus, for most individuals, HELOCs are aggregated over only one bank (i.e., weight$=1$).

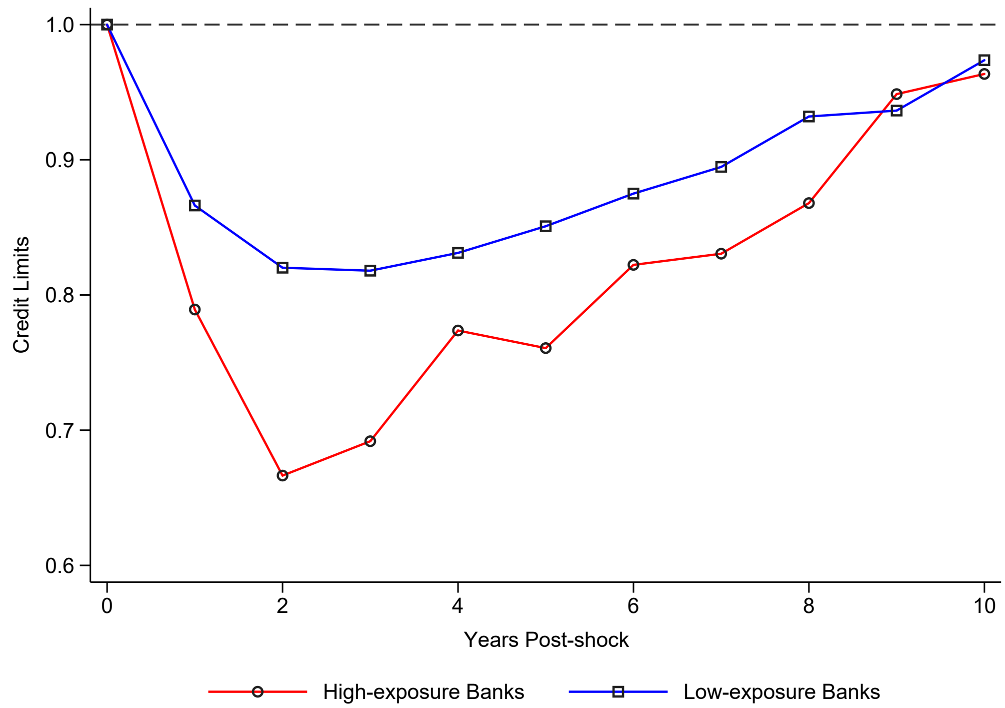


Figure 3.7: Long-run effect of the funding shock on aggregate credit supply

This figure plots the average credit limits extended by the high- and low-exposure banks relative to the pre-shock period. The average credit limits are computed by averaging the total credit limits extended by banks to all consumers across the high exposure (above median) and the low exposure (below median) bank groups.

Figure 3.8 plots the long-run effect of the funding shock on total credit card balances for the subprime, near-prime, and prime individuals. The coefficients in the plot are the elasticities estimated using the specifications in Columns (4)–(6) in Table 3.7, Panel B, for each category of borrower quality over time. We plot the negative elasticites (as opposed to elasticities) for the ease of interpreting the coefficients as the change in total credit card balances for a 1% reduction in the total credit limits induced by the short-term wholesale funding shock. For each period, the changes in total credit limits and total credit balances for each individual are computed relative to their pre-shock period values. Figure 3.8 shows a striking result — the effect of the short-term wholesale funding shock was persistent for the subprime and the near-prime consumers. That is the credit balances for the more-exposed subprime and near-prime consumers were lower than the less-exposed subprime and near-prime consumers even in the long run. However, Figure 3.8 shows that the effect of the transmitted bank funding shock gradually dissipated over time for the prime consumers. Our results suggest that either financing frictions for lower-quality borrowers are binding for a very long time, or that the transmitted funding shock can itself weaken a borrower's fundamentals, thereby limiting her access to credit in the future.

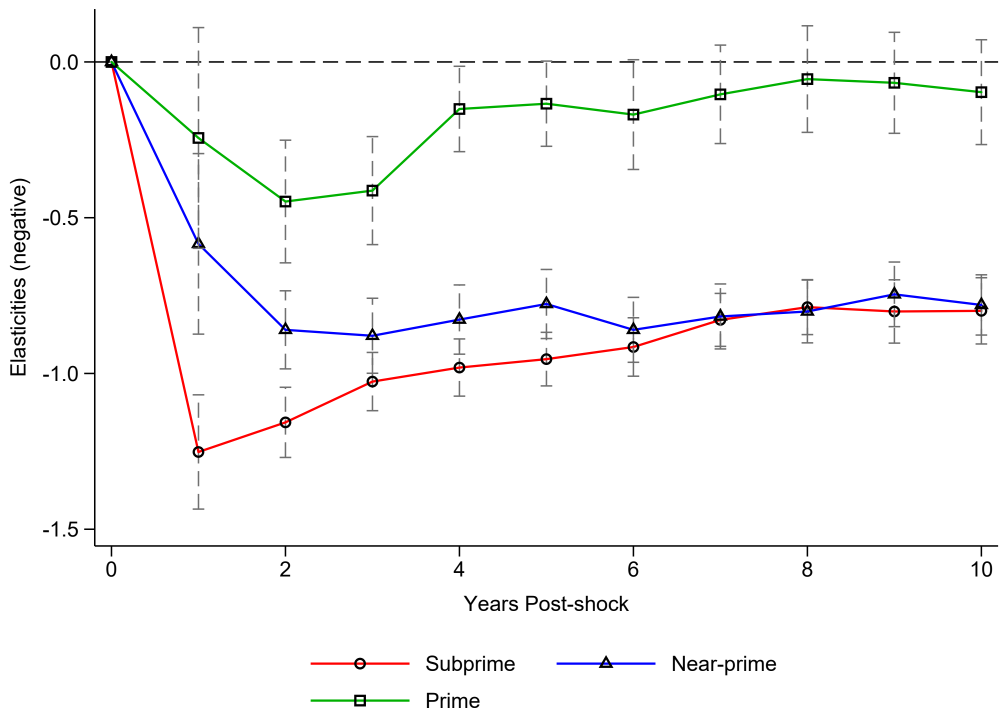


Figure 3.8: Long-run effect of the funding shock on total credit card balances

This figure plots the negative elasticities of total credit card balances to total credit card limits for subsamples of individuals with subprime (< 620), near-prime (≥ 620 and < 680), and prime (≥ 680) credit scores. To obtain these elasticities, total credit card limits are instrumented by the individual's pre-shock weighted exposure to the short-term wholesale funding shock using the specification in Table 3.5, Column (4). The regressions are estimated at the individual level. An individual's weighted exposure is computed by aggregating the weighted $Exposure$ measure at the credit card level, where the weights assigned to a credit card are proportional to its credit limit. The points represent coefficient estimates, and the dashed vertical lines represent the 95% confidence intervals for the estimated coefficients.

3.7 Conclusion

In this paper, we show that liquidity shocks to banks can transmit to consumers with significant distributional consequences. We use micro data on credit limits and credit balances for the near universe of 500 million credit cards issued to 134 million consumers to show how the dry-up of the short-term wholesale funding for banks transmitted to consumers through credit cards. We document a new channel – namely, credit card limits, through which banks transmitted their short-term wholesale funding liquidity shocks to their consumers and affected their consumption through credit cards.

We document significant heterogeneity in how banks transmit their liquidity shocks to consumers through credit card limits. In general, banks passed on their liquidity shock to a greater extent to credit-constrained consumers (e.g., consumers with lower credit scores and higher credit card utilization). As credit-constrained consumers generally face greater credit market frictions, they were unable to hedge away from the transmitted bank liquidity shocks and were forced to reduce their consumption. Our results show that when banks face liquidity shocks, they are more likely to pass on these shocks to those consumers who are least able to cope with them. Consequently, our results show *who* bears the real costs of fragile bank funding structures and for how long.

Our results also contribute to the debate on post-crisis regulatory reform on banks' funding structures that have become heavily reliant on wholesale funding. Our analysis provides estimates for the elasticities of consumer-level credit limits and credit balances to wholesale funding across different consumer groups, such as prime, subprime, and near-prime consumer groups. Thus, by providing these elasticities, our results enrich the debate on the distributional effects of banks' funding fragility on consumers.

We also add to the understanding of why consumption declined during the Great Recession and recovered slowly after the recession. Our results suggest that the impaired balance sheets of financial intermediaries played an important role. We document the credit card limits channel through which the financial health of intermediaries was transmitted to consumers. Although the total credit supplied by banks through credit cards recovered subsequently, the impact of the funding shock was still persistent for some borrowers even a decade after the shock.

Appendices

APPENDIX A
APPENDIX FOR "REDUCING RISK OR REACHING FOR YIELD? IMPACT OF STRESS TESTS ON CREDIT CARD LENDING"

Table A.1: Are results driven by bank-specific consumer demand?

This table shows the robustness of the relation between stress test exposure and bank risk-taking through credit card limit growth to bank-specific consumer demand. Each pre-collection and post-results periods for any given stress test cycle consists of two semiannual archives. Credit card–level data are first collapsed to obtain a single credit card–level cross section separately in the pre– and post–stress test periods by averaging across time. Then, the dependent variable is constructed as the growth in limits at the credit card–level from the pre– to the post–stress test period. $Exposure$ is computed as the difference between the starting value of banks' Tier 1 ratios at the outset of the stress test and the lowest capital ratio implied by the severely adverse stress scenario. *Non-prime* is an indicator that equals 1 if an individual's credit score is below 680 at the outset of a given stress test cycle, and 0 otherwise. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: Δ CC Limit	(1)	(2)	(3)
Exposure	-2.249***	-2.442***	
	(-6.73)	(-7.55)	
Exposure $\times$ Non-prime		1.125***	1.024***
		(6.31)	(5.79)
Consumer $\times$ ST Cycle FE	✓	✓	✓
Bank $\times$ ST Cycle FE			✓
Observations	10,071,409	10,071,409	10,071,409
Adj. R^2	0.213	0.509	0.972
Trade-level controls	✓	✓	✓
Bank-level controls	✓	✓	

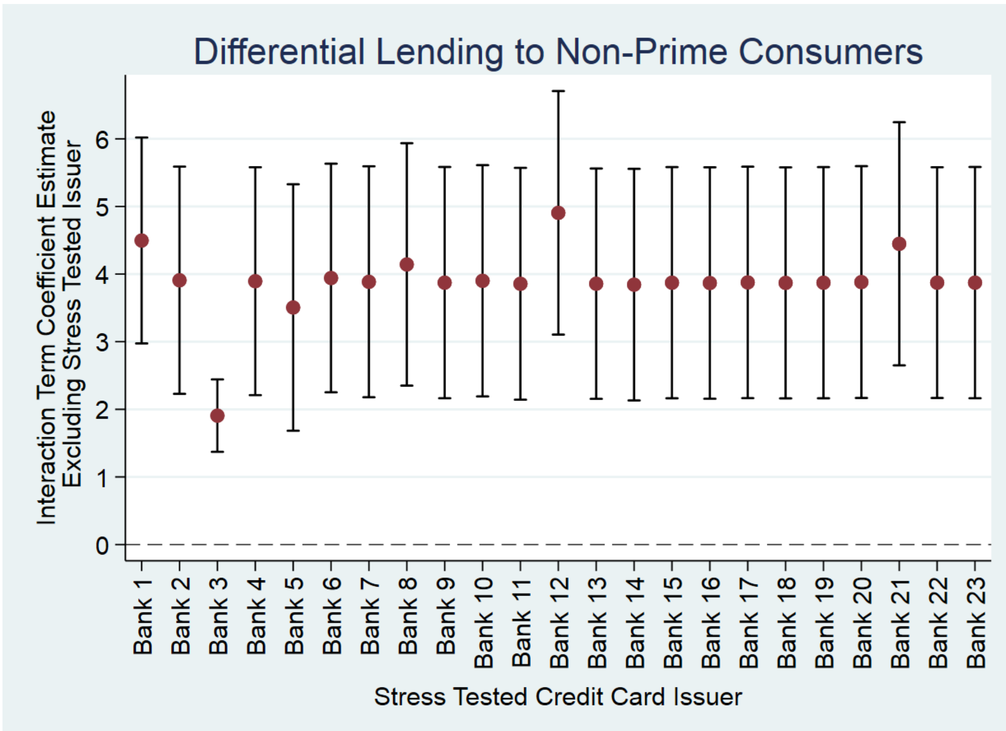


Figure A.1: Estimation excluding each bank

This figure shows the coefficient β_2 associated with my variable of interest, $StressTestExposure \times NonPrime$, from Equation (1.4) estimated by excluding each BHC from my sample one at a time. $StressTestExposure$ is defined as the difference between the tested BHC's risk-based Tier 1 capital ratio at the outset of a stress test and the lowest implied equivalent ratio in the severely adverse scenario for that testing cycle. $NonPrime$ is dummy indicator that equals 1 for consumers with credit scores under 680, and 0 otherwise. The circles in the plot represent the coefficient estimate β_2 associated with $StressTestExposure \times NonPrime$ from Equation (1.4) after excluding the bank shown below the plotted point on the *x*-axis. The solid vertical lines represent the 95% confidence intervals for the estimated coefficients. The point estimates are ordered based on bank size. Thus, "Bank 1" ("Bank 23") represents the coefficient estimate after removing the largest (smallest) bank in my sample.

Table A.2: Are stressed banks cutting limits to risky consumers? Additional proxies for consumer constraints

This table reports the robustness of the relation between stress test exposure and bank risk-taking through credit card limit growth to different proxies for consumer constraints. $Exposure$ refers to stress test exposure and is computed as the difference between banks' starting Tier 1 ratio at the outset of a stress test cycle and the lowest implied Tier 1 ratio in the severely adverse stress scenario. In Panel A (Panel B), consumer constraints are captured through credit card–level utilization (individual-level utilization). *CC Utilization (50–90%)* and *CC Utilization (>90%)* are indicator variables that equal 1 if the utilization ratio of a credit card is between 50% and 90% or greater than 90%, respectively, and 0 otherwise. *Ind. Utilization (50–90%)* and *Ind. Utilization (>90%)* are indicator variables that equal 1 if the individual's aggregate utilization ratio, computed using all credit cards issued to the consumer, is between 50% and 90% or greater than 90%, respectively, and 0 otherwise. In Panel C, consumer constraints are measured using the debt-to-income ratio. Across all panels, consumers are sorted into constrained and non-constrained groups at the outset of any given stress test cycle. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: *CC utilization*		**Panel B**: *Consumer utilization*		**Panel C**: *Debt-to-income*	
Depvar: ΔCC Limit	(1)	*Depvar:* ΔCC Limit	(2)	*Depvar:* ΔCC Limit	(3)
Exposure × CC util (50–90%)	5.112***	Exposure × Ind. util (50–90%)	3.888***	Exposure × DTI quintile	0.431**
	(5.34)		(4.38)		(2.12)
Exposure × CC util (>90%)	4.564***	Exposure × Ind. util (>90%)	3.837***		
	(5.31)		(4.33)		
CC util (50–90%)	13.266***				
	(15.80)				
CC util (>90%)	10.813***				
	(8.93)				
N	10,071,409		10,071,409		10,071,409
Adj. R^2	0.207		0.214		0.214

Table A.3: Robustness check: Alternative definitions of stress test exposure

This table reports the robustness of the relation between stress test exposure and bank risk-taking through credit card limit growth to alternative definitions of stress test exposure. In Panel A, $Exposure$ is computed as the difference between banks' starting total risk-based capital ratio at the outset of a stress test cycle and the lowest implied total risk-based ratio in the severely adverse stress scenario under DFAST disclosures. In Panel B, $Exposure$ is defined in terms of banks' Tier 1 leverage ratios under DFAST disclosures. In Panel C, $Exposure$ is defined in terms of banks' Tier 1 risk-based capital ratios under CCAR disclosures. In Panel D, stress-tested banks are compared to non-tested banks. For the 2012 and 2013 testing cycles, non-tested banks with assets greater than $50 billion are chosen as a control group for stress-tested banks. For the 2014–2016 cycles, non-tested banks with assets greater than $10 billion are chosen as a control group for stress-tested banks. *Non-Prime* is an indicator variable that equals 1 if an individual's credit score is under 680 at the outset of a given stress test cycle, and 0 otherwise. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

	Panel A: DFAST Total Risk-Based Capital Ratio		**Panel B**: DFAST Tier 1 Leverage Ratio		**Panel C**: CCAR Tier 1 Risk-Based Capital Ratio		**Panel D**: Comparing Stressed/Non-Stressed	
Depvar: Δ CC Limit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure	-4.422***		-3.778*		-5.349**		-9.449***	
	(-2.97)		(-1.93)		(-2.51)		(-6.93)	
Exposure $\times$ Non-prime	4.009***	3.602***	3.289**	2.916**	5.553***	4.803***	3.269***	2.302***
	(4.41)	(3.94)	(2.57)	(2.39)	(4.68)	(4.51)	(4.59)	(3.06)
Consumer $\times$ ST Cycle FE	✓	✓	✓	✓	✓	✓		
Bank $\times$ ST Cycle FE		✓		✓		✓		
Observations	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409	4,942,746	4,942,746
Adj. R^2	0.193	0.202	0.192	0.202	0.194	0.213	0.086	0.225
Trade-level controls	✓	✓	✓	✓	✓	✓	✓	✓
Bank-level controls	✓		✓		✓		✓	

Table A.4: Robustness check: Bank-level clustering

This table reports the robustness of the relation between stress test exposure and bank risk-taking through credit card limit growth to alternative clustering techniques. $Exposure$ refers to stress test exposure and is computed as the difference between banks' starting Tier 1 ratio at the outset of a stress test cycle and the lowest implied Tier 1 ratio in the severely adverse stress scenario. *Non-Prime* is an indicator variable that equals 1 if an individual's credit score is below 680 at the outset of a given stress test cycle, and 0 otherwise. The standard errors are clustered at the bank level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: Δ CC Limit	(1)	(2)	(3)	(4)	(5)
Exposure	-5.382**	-5.893***	-4.676**	-4.925***	
	(-2.56)	(-2.58)	(-2.43)	(-2.61)	
Exposure × Non-prime		3.018**	3.010**	3.868***	3.617***
		(2.21)	(2.21)	(2.72)	(2.59)
Consumer × ST Cycle FE	✓	✓	✓	✓	✓
Bank × ST Cycle FE					✓
Observations	10,071,409	10,071,409	10,071,409	10,071,409	10,071,409
Adj. R^2	0.169	0.169	0.181	0.193	0.213
Trade-level controls			✓	✓	✓
Bank-level controls				✓	

Table A.5: Are cuts in credit limits driven along the extensive margin on the intensive margin?

This table examines whether the findings presented in Table 1.2 are driven along the intensive or extensive margins. Intensive margin results are presented in Panel A. The sample is restricted to credit cards that remain open for up to one year after the disclosure of stress test results for any given cycle. Panel B reports results for credit card closures along the extensive margin. $Exposure$ is computed as the difference between banks' starting risk-based Tier 1 ratios at the outset of a stress test cycle and the lowest implied Tier 1 ratio in the severely adverse stress scenario. *Non-Prime* is an indicator variable that equals 1 if an individual's credit score is below 680 at the outset of a given stress test cycle, and 0 otherwise. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

	Panel A: Intensive margin		**Panel B**: Extensive margin	
	(1)	(2)	(3)	(4)
Exposure	-1.617***		2.229**	
	(-8.11)		(2.19)	
Exposure × Non-prime	1.253***	1.736***	-2.222***	-2.035***
	(5.77)	(6.67)	(-3.36)	(-3.01)
Consumer × ST Cycle FE	✓	✓	✓	✓
Bank × ST Cycle FE		✓		✓
Observations	8,116,284	8,116,284	10,071,409	10,071,409
Adj. R^2	0.119	0.125	0.188	0.198
Trade-level controls	✓	✓	✓	✓
Bank-level controls	✓		✓	

Table A.6: Impact of consumer characteristics on trade-level delinquencies: Horse-racing specifications

This table reports results examining how the relationship between banks' stress test exposure and credit card–level performance is affected by consumer characteristics in a horse-racing setting. $Exposure$ is computed as the difference between the starting value of banks' Tier 1 ratios at the outset of the stress test and the lowest capital ratio implied by the severely adverse stress scenario. *Non-prime* is an indicator variable that equals 1 if an individual's credit score is below 680 at the outset of a given stress test cycle, and 0 otherwise. Panel A examines whether consumer income impacts card-level performance. Panel B examines the role of consumer education, where *No College* is an indicator that equals 0 if the consumer holds a college degree, and 1 otherwise. Lastly, Panel C examines the role of consumers' job sophistication, where *Non-Soph Job* is an indicator variable that equals 1 if the consumer holds a job that does not require sophisticated skills, and 0 otherwise. The standard errors are clustered at the bank–year level. T-statistics are reported in parentheses. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: $\mathbb{1}$(Delinquency)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure × Non-Prime	0.453***		0.421**		0.440**	0.434***		0.467***	0.464**
	(2.78)		(2.48)		(2.28)	(2.74)		(2.95)	(2.34)
Exposure × Income (log)		-0.143***	-0.052**			-0.037*			-0.041**
		(-4.87)	(-2.37)			(-1.85)			(-2.49)
Exposure × No college				0.041***	0.022**	0.017*			0.018*
				(4.08)	(2.34)	(1.94)			(1.69)
Exposure × Non-soph job							0.021	0.016	0.004
							(1.45)	(1.10)	(0.34)
Consumer × ST Cycle FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bank × ST Cycle FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	10,071,409	8,082,699	8,082,699	8,088,998	8,088,998	8,082,699	2,786,075	2,786,075	2,784,200
Adj. R^2	0.587	0.578	0.578	0.577	0.577	0.577	0.558	0.558	0.556
Trade-level controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

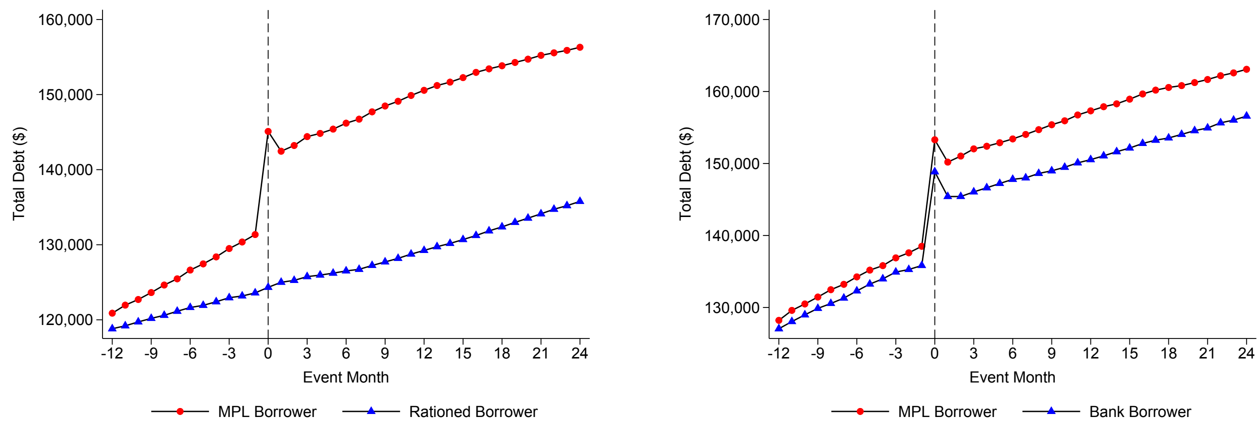
Table A.7: Stress-tested banks in each cycle

This table lists stress-tested banks for each round of stress tests. The '✓' symbol indicates that a bank was tested in the respective year, and passed the test. The 'X' symbol identifies banks that did not pass the test conducted in the respective year. Results for the 2011 CCAR are not publicly available.

	SCAP	CCAR		CCAR/DFAST			
Bank Holding Company	2009	2011	2012	2013	2014	2015	2016
Ally Financial Inc.	X	✓	X	X	✓	✓	✓
American Express Company	✓	✓	X	✓	✓	✓	✓
Bank of America Corporation	X	✓	✓	✓	✓	X	✓
The Bank of New York Mellon Corporation	✓	✓	✓	✓	✓	✓	✓
BB&T Corporation	✓	✓	✓	X	✓	✓	✓
Capital One Financial Corporation	✓	✓	X	✓	✓	✓	✓
Citigroup Inc.	X	✓	✓	✓	X	✓	✓
Fifth Third Bancorp	X	✓	✓	✓	✓	✓	✓
The Goldman Sachs Group, Inc.	✓	✓	✓	X	✓	✓	✓
JPMorgan Chase & Co.	✓	✓	✓	X	✓	✓	✓
KeyCorp	X	✓	✓	✓	✓	✓	✓
Metlife, Inc.	✓	✓	X				
Morgan Stanley	X	✓	✓	✓	✓	✓	X
The PNC Financial Services Group, Inc.	X	✓	✓	✓	✓	✓	✓
Regions Financial Corporation	X	✓	✓	✓	✓	✓	✓
State Street Corporation	✓	✓	✓	✓	✓	✓	✓
SunTrust Banks, Inc.	X	✓	✓	✓	✓	✓	✓
U.S. Bancorp	✓	✓	✓	✓	✓	✓	✓
Wells Fargo & Company	X	✓	✓	✓	✓	✓	✓
Banco Bilbao Vizcaya Argentaria, S.A.					✓	✓	✓
Banco Santander, S.A.					X	X	X
BancWest, Inc.							✓
Bank of Montreal					✓	✓	✓
Citizens Financial Group, Inc.					✓	✓	✓
Comerica Incorporated					✓	✓	✓
Deutsche Bank						X	X
Discover Financial Services					✓	✓	✓
HSBC Holdings PLC					X	✓	✓
Huntington Bancshares Incorporated					✓	✓	✓
M&T Bank Corporation					✓	✓	✓
Mitsubishi UFJ Financial Group, Inc.					✓	✓	✓
Northern Trust Corporation					✓	✓	✓
The Toronto-Dominion Bank							✓
Zions Bancorporation					X	✓	✓

APPENDIX B
APPENDIX FOR "IMPACT OF MARKETPLACE LENDING ON CONSUMERS' BORROWING CAPACITIES AND BORROWING OUTCOMES"

Total Debt



Unmet Cohort | Bank Cohort

Figure B.1: Impact of MPL loans on total debt

This figure presents the average monthly trends for the total debt balances for the unmet and bank cohorts. In both panels, the *x*-axis displays event time relative to the month of loan origination and the *y*-axis represents the total debt.

Table B.1: Impact of MPL loans on future borrowing

This table presents the evolution of MPL borrowers' overall debt levels in the months surrounding MPL loan origination. Columns (1)–(2) and Columns (3)–(4) present the evolution of total non-mortgage debt and monthly debt payment, respectively. Columns (1) and (3) (Columns (2) and (4)) report the regression results for the change in the borrowing outcomes for the MPL borrowers relative to the matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Log(Total Non-MTG Debt)		Log(Monthly Debt Payment)	
Monthly DID	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)
Pre-period				
-12	3.962***	0.236	-0.339**	-1.039***
	(0.417)	(0.301)	(0.136)	(0.134)
-6	2.874***	0.197	-0.002	-0.600***
	(0.584)	(0.218)	(0.066)	(0.081)
-3	0.986***	0.126	-0.100	-0.271**
	(0.110)	(0.135)	(0.144)	(0.121)
-2	0.591***	0.133	-0.079	-0.217**
	(0.058)	(0.106)	(0.106)	(0.095)
Post-period				
$+0$	36.493***	3.849***	29.579***	6.342***
	(0.303)	(0.222)	(0.221)	(0.154)
$+1$	27.570***	4.376***	22.415***	8.071***
	(0.643)	(0.194)	(0.329)	(0.182)
$+2$	28.645***	5.837***	22.177***	9.235***
	(0.990)	(0.212)	(0.485)	(0.161)
$+3$	30.403***	6.996***	23.403***	10.106***
	(1.038)	(0.262)	(0.505)	(0.212)
$+6$	32.129***	7.399***	24.783***	10.570***
	(1.075)	(0.310)	(0.562)	(0.181)
$+12$	32.368***	7.698***	28.693***	12.186***
	(1.005)	(0.511)	(0.532)	(0.286)
$+18$	30.111***	6.845***	30.483***	12.835***
	(1.097)	(0.514)	(0.602)	(0.446)
$+24$	27.374***	6.069***	30.072***	12.661***
	(1.056)	(0.498)	(0.604)	(0.468)
Cohort FE	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
# Cohorts	347,172	118,148	347,172	118,148
Avg. Adj. R^2	0.139	0.133	0.298	0.31

Table B.2: Impact of MPL loans on future borrowing capacities (5% unmatched sample)

This table presents the evolution of the borrowing capacities of MPL borrowers in the months surrounding MPL loan origination. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) report results documenting the evolution of credit card utilization ratios, credit scores, and credit card limits, respectively. Columns (1), (3), and (5) (Columns (2), (4), and (6)) report the regression results for the change in borrowing capacities for the MPL borrowers relative to the unmet credit demand borrowers (bank borrowers) in a 5% random sample of U.S. consumers. Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Depvar:	CC Utilization		Credit Score		Log(CC Limit)	
Monthly DID	MPL vs Unmet	MPL vs Bank	MPL vs Unmet	MPL vs Bank	MPL vs Unmet	MPL vs Bank
MPL Coefficient $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
-12	-1.387***	-2.038***	-0.783***	-4.878***	-3.395***	-4.545***
	(0.085)	(0.150)	(0.302)	(0.336)	(0.320)	(0.316)
-6	-0.825***	-1.167***	-0.550**	-3.564***	-1.393***	-1.678***
	(0.132)	(0.141)	(0.233)	(0.253)	(0.195)	(0.183)
-3	0.093	0.057	-0.192	-2.544***	-0.314	-0.492***
	(0.127)	(0.110)	(0.188)	(0.213)	(0.221)	(0.134)
-2	0.260***	0.282***	0.304**	-1.598***	-0.079	-0.054
	(0.097)	(0.100)	(0.152)	(0.150)	(0.144)	(0.122)
Post-period						
$+0$	-8.507***	-2.659***	12.013***	1.098***	0.219	0.653***
	(0.493)	(0.327)	(0.555)	(0.395)	(0.246)	(0.185)
$+1$	-27.184***	-9.151***	37.003***	13.291***	0.271	1.124***
	(0.512)	(0.397)	(0.747)	(0.847)	(0.801)	(0.236)
$+2$	-25.654***	-7.749***	36.287***	12.581***	2.859***	2.757***
	(0.410)	(0.400)	(0.799)	(0.763)	(0.994)	(0.298)
$+3$	-22.098***	-6.483***	26.294***	4.353***	5.849***	4.266***
	(0.384)	(0.400)	(0.603)	(0.597)	(0.920)	(0.364)
$+6$	-14.164***	-3.371***	17.247***	-0.649	11.435***	7.470***
	(0.409)	(0.392)	(0.623)	(0.620)	(1.035)	(0.452)
$+12$	-7.066***	-0.976***	5.660***	-5.287***	15.821***	10.027***
	(0.291)	(0.349)	(0.718)	(0.582)	(1.054)	(0.561)
$+18$	-4.493***	-0.833***	0.666	-7.200***	15.215***	10.465***
	(0.249)	(0.307)	(0.745)	(0.602)	(1.098)	(0.655)
$+24$	-3.729***	-1.084***	-3.130***	-8.815***	13.999***	10.644***
	(0.217)	(0.322)	(0.692)	(0.605)	(0.987)	(0.746)
Zipcode FE	✓	✓	✓	✓	✓	✓
Origination Month FE	✓	✓	✓	✓	✓	✓
Credit Score Bin FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
Observations	410,964	136,177	410,964	136,177	410,964	136,177
Avg. Adj. R^2	0.172	0.208	0.198	0.207	0.215	0.189

Table B.3: Impact of MPL loans on future borrowing outcomes (5% unmatched sample)

This table presents the evolution of the borrowing outcomes of MPL borrowers in the months surrounding MPL loan origination. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) report results documenting the evolution of credit card balances, default rates on credit cards, and default rates on non–credit card products, respectively. Columns (1), (3), and (5) (Columns (2), (4), and (6)) report the regression results for the change in borrowing outcomes for the MPL borrowers relative to the unmet credit demand borrowers (bank borrowers) in a 5% random sample of U.S. consumers. Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Depvar:	Log(CC Balance)		CC Defaults		Non-CC Defaults	
Monthly DID	MPL vs Unmet	MPL vs Bank	MPL vs Unmet	MPL vs Bank	MPL vs Unmet	MPL vs Bank
MPL Coefficient $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
-12	3.016***	3.591***	-0.017	0.057	0.190*	0.307***
	(0.730)	(0.900)	(0.047)	(0.049)	(0.101)	(0.108)
-6	2.297***	1.262	0.049	0.076**	-0.019	0.116
	(0.752)	(0.771)	(0.048)	(0.037)	(0.078)	(0.079)
-3	1.323**	0.714	-0.023	0.036	0.024	0.092
	(0.631)	(0.677)	(0.037)	(0.044)	(0.069)	(0.058)
-2	1.275***	0.430	-0.031	0.013	-0.0001	0.056
	(0.487)	(0.448)	(0.027)	(0.032)	(0.044)	(0.046)
Post-period						
+0	-24.044***	-3.179***	-0.159***	-0.035	-0.245***	0.028
	(1.669)	(1.221)	(0.026)	(0.027)	(0.047)	(0.049)
+1	-98.564***	-16.122***	-0.332***	-0.073***	-0.495***	-0.030
	(1.671)	(1.923)	(0.041)	(0.025)	(0.065)	(0.074)
+2	-82.731***	-4.569**	-0.779***	-0.160***	-0.798***	-0.060
	(1.833)	(1.808)	(0.064)	(0.042)	(0.078)	(0.076)
+3	-59.137***	1.573	-1.259***	-0.263***	-1.143***	-0.121
	(1.777)	(1.744)	(0.073)	(0.045)	(0.092)	(0.083)
+6	-18.564***	12.635***	-2.004***	-0.363***	-1.370***	-0.109
	(1.456)	(1.551)	(0.117)	(0.095)	(0.117)	(0.133)
+12	10.158***	18.648***	-0.718***	0.155	-0.603***	-0.005
	(1.526)	(1.644)	(0.139)	(0.139)	(0.121)	(0.137)
+18	17.730***	17.069***	0.811***	0.664***	0.520***	0.523***
	(1.651)	(2.036)	(0.227)	(0.178)	(0.150)	(0.162)
+24	19.016***	15.714***	1.088***	1.033***	0.743***	0.818***
	(1.537)	(1.778)	(0.177)	(0.171)	(0.144)	(0.128)
Zipcode FE	✓	✓	✓	✓	✓	✓
Origination Month FE	✓	✓	✓	✓	✓	✓
Credit Score Bin FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
Observations	410,964	136,177	410,964	136,177	410,964	136,177
Avg. Adj. R^2	0.168	0.158	0.188	0.109	0.028	0.023

Table B.4: Impact of MPL loans on borrowing capacities with loan term controls

This table presents the evolution of MPL borrowers' borrowing capacities in the months surrounding MPL loan origination for the bank cohort after controlling for loan terms. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) present the evolution of credit card utilization ratios, credit scores, and credit card limits, respectively. Columns (1), (3), and (5) report the regression results that control for loan amount and maturity. Columns (2), (4), and (6) report regression results that additionally control for loan interest rates. Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	CC Utilization		Credit Scores		Log(CC Limits)	
Monthly DID	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort
MPL Coefficient $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period						
-12	-1.435***	-1.234***	-3.879***	-3.120***	-3.471***	-3.017***
	(0.108)	(0.114)	(0.316)	(0.291)	(0.165)	(0.163)
-6	-0.734***	-0.588***	-2.910***	-2.326***	-1.321***	-1.161***
	(0.080)	(0.094)	(0.209)	(0.186)	(0.114)	(0.116)
-3	-0.118**	-0.007	-2.027***	-1.675***	-0.463***	-0.370***
	(0.052)	(0.056)	(0.197)	(0.176)	(0.077)	(0.075)
-2	-0.042	0.028	-1.419***	-1.145***	-0.245***	-0.203***
	(0.042)	(0.046)	(0.166)	(0.156)	(0.067)	(0.064)
Post-period						
$+0$	-0.907**	-1.208***	-0.090	0.791	0.330***	0.281***
	(0.394)	(0.391)	(0.559)	(0.528)	(0.088)	(0.090)
$+1$	-2.059***	-2.549***	5.880***	7.433***	1.164***	0.983***
	(0.441)	(0.408)	(1.152)	(1.065)	(0.189)	(0.181)
$+2$	-0.512	-1.280***	5.049***	7.088***	2.163***	1.911***
	(0.416)	(0.360)	(0.955)	(0.802)	(0.238)	(0.224)
$+3$	0.115	-0.796**	-2.957***	-1.265**	3.020***	2.791***
	(0.405)	(0.347)	(0.615)	(0.500)	(0.247)	(0.245)
$+6$	1.645***	0.521*	-5.814***	-3.529***	4.293***	4.122***
	(0.362)	(0.298)	(0.603)	(0.469)	(0.360)	(0.346)
$+12$	2.726***	1.499***	-9.127***	-5.773***	3.980***	4.263***
	(0.225)	(0.246)	(0.591)	(0.592)	(0.483)	(0.450)
$+18$	2.837***	1.667***	-10.737***	-7.128***	3.370***	4.236***
	(0.338)	(0.346)	(0.694)	(0.721)	(0.545)	(0.480)
$+24$	2.913***	1.739***	-12.576***	-9.224***	2.859***	4.114***
	(0.421)	(0.429)	(0.714)	(0.719)	(0.622)	(0.530)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
Amount and Maturity	✓	✓	✓	✓	✓	✓
Interest Rate		✓		✓		✓
# Cohorts	83,393	83,393	83,393	83,393	83,393	83,393
Avg. Adj. R^2	0.27	0.275	0.195	0.205	0.216	0.217

Table B.5: Impact of MPL loans on future borrowing with loan term controls

This table presents the evolution of MPL borrowers' overall debt levels in the months surrounding MPL loan origination for the bank cohort after controlling for loan terms. Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) present the evolution of credit card balances, total balances, and debt-to-income ratios, respectively. Columns (1), (3), and (5) report the regression results that control for loan amount and maturity. Columns (2), (4), and (6) report regression results that additionally control for loan interest rates. Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Log(CC Balance)		Log(Total Debt)		DTI Ratio	
Monthly DID	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort
MPL Coefficient $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period						
-12	4.360***	4.282***	-1.456***	-1.218***	-0.276**	-0.227*
	(0.593)	(0.628)	(0.376)	(0.399)	(0.124)	(0.132)
-6	2.043***	2.044***	-0.906***	-0.642**	-0.403***	-0.388***
	(0.601)	(0.628)	(0.245)	(0.250)	(0.109)	(0.103)
-3	0.037	0.056	-0.562*	-0.323	-0.137*	-0.100
	(0.293)	(0.297)	(0.320)	(0.336)	(0.083)	(0.081)
-2	0.334*	0.345*	-0.415	-0.307	-0.077	-0.066
	(0.199)	(0.199)	(0.255)	(0.271)	(0.057)	(0.058)
Post-period						
$+0$	-2.617	-3.872**	-0.366	-0.535**	0.441***	-0.028
	(1.651)	(1.749)	(0.256)	(0.247)	(0.120)	(0.091)
$+1$	2.459	-1.618	0.764***	0.367	1.564***	1.008***
	(2.708)	(2.593)	(0.287)	(0.280)	(0.140)	(0.113)
$+2$	13.076***	8.035***	1.853***	1.371***	2.377***	1.765***
	(2.119)	(1.852)	(0.379)	(0.399)	(0.151)	(0.134)
$+3$	14.922***	10.024***	2.825***	2.283***	2.360***	1.721***
	(2.091)	(1.910)	(0.446)	(0.469)	(0.156)	(0.140)
$+6$	19.193***	14.591***	2.790***	2.267***	2.834***	2.035***
	(1.362)	(1.368)	(0.320)	(0.345)	(0.156)	(0.131)
$+12$	16.823***	13.423***	2.657***	2.079***	3.526***	2.380***
	(1.509)	(1.743)	(0.421)	(0.463)	(0.188)	(0.193)
$+18$	16.527***	14.029***	3.637***	3.159***	4.899***	3.783***
	(1.959)	(1.989)	(0.532)	(0.516)	(0.282)	(0.296)
$+24$	15.333***	13.128***	3.661***	3.476***	5.567***	4.780***
	(2.336)	(2.272)	(0.770)	(0.786)	(0.290)	(0.316)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
Amount and Maturity	✓	✓	✓	✓	✓	✓
Interest Rate		✓		✓		✓
# Cohorts	83,393	83,393	83,393	83,393	83,393	83,393
Avg. Adj. R^2	0.162	0.163	0.331	0.331	0.217	0.224

Table B.6: Impact of MPL loans on default rates with loan term controls

This table presents the evolution of MPL borrowers' default rates in the months surrounding MPL loan origination for the bank cohort after controlling for loan terms. Columns (1)–(2) and Columns (3)–(4), and Columns (5)–(6) present the evolution of defaults on any kinds of debt, defaults on credit cards, and defaults on non–credit card products, respectively. Columns (1), (3), and (5) report the regression results that control for loan amount and maturity. Columns (2), (4), and (6) report regression results that additionally control for loan interest rates. Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	All Defaults		CC Defaults		Non-CC Defaults	
Monthly DID	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period						
-12	0.200***	0.160***	0.004	0.003	0.199***	0.160**
	(0.070)	(0.061)	(0.035)	(0.037)	(0.072)	(0.064)
-6	0.087	0.081	0.046	0.038	0.077	0.072
	(0.062)	(0.064)	(0.035)	(0.032)	(0.063)	(0.065)
-3	0.130**	0.117**	0.049*	0.056**	0.101**	0.078*
	(0.056)	(0.050)	(0.027)	(0.026)	(0.050)	(0.044)
-2	0.060*	0.049*	0.001	0.009	0.072**	0.049*
	(0.031)	(0.028)	(0.022)	(0.019)	(0.030)	(0.026)
Post-period						
$+0$	0.028	0.030	0.021	0.017	0.014	0.019
	(0.030)	(0.029)	(0.014)	(0.013)	(0.031)	(0.031)
$+1$	0.086*	0.068	0.052**	0.051**	0.045	0.028
	(0.048)	(0.044)	(0.024)	(0.024)	(0.045)	(0.039)
$+2$	0.094**	0.096**	0.002	0.012	0.096**	0.088**
	(0.048)	(0.049)	(0.031)	(0.031)	(0.045)	(0.042)
$+3$	0.059	0.096	-0.014	0.003	0.071	0.104**
	(0.064)	(0.061)	(0.044)	(0.043)	(0.057)	(0.053)
$+6$	0.237**	0.139	0.197***	0.155**	0.094	0.030
	(0.121)	(0.110)	(0.072)	(0.066)	(0.088)	(0.079)
$+12$	0.844***	0.482***	0.676***	0.409***	0.410***	0.195*
	(0.156)	(0.128)	(0.129)	(0.101)	(0.105)	(0.108)
$+18$	0.933***	0.488***	0.861***	0.516***	0.514***	0.225**
	(0.149)	(0.135)	(0.166)	(0.167)	(0.114)	(0.104)
$+24$	1.155***	0.784***	0.832***	0.588***	0.744***	0.513***
	(0.170)	(0.147)	(0.159)	(0.140)	(0.132)	(0.122)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
Amount and Maturity	✓	✓	✓	✓	✓	✓
Interest Rate		✓		✓		✓
# Cohorts	83,393	83,393	83,393	83,393	83,393	83,393
Avg. Adj. R^2	0.115	0.116	0.122	0.123	0.061	0.061

Table B.7: Adverse selection on liquidity constraints

This table presents results that examine the role of different measures of borrower indebtedness in explaining trends in Non-CC defaults. Columns (1)–(2) and Columns (3)–(4) present the results for the unmet cohort and the bank cohort, respectively. Columns (1) and (3) (Columns (2) and (4)) report the regression results that control for change in total debt (change in debt-to-income ratio). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	CC Defaults				Non-CC Defaults			
Monthly DID	Unmet Cohort		Bank Cohort		Unmet Cohort		Bank Cohort	
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-period horizon								
-12	0.014	0.013	0.030	0.030	0.164***	0.182***	0.178***	0.188***
	(0.021)	(0.021)	(0.022)	(0.022)	(0.032)	(0.033)	(0.041)	(0.041)
-6	0.043**	0.042**	0.039**	0.039**	0.079***	0.088***	0.082	0.086*
	(0.020)	(0.020)	(0.020)	(0.020)	(0.027)	(0.027)	(0.051)	(0.051)
-3	-0.033*	-0.032*	0.031**	0.032**	0.048**	0.052**	0.035	0.036
	(0.018)	(0.018)	(0.016)	(0.016)	(0.023)	(0.023)	(0.022)	(0.022)
-2	-0.024**	-0.024*	0.004	0.004	0.011	0.014	0.016	0.016
	(0.012)	(0.012)	(0.013)	(0.013)	(0.018)	(0.018)	(0.018)	(0.018)
Post-period horizon								
$+0$	-0.093***	-0.175***	0.003	0.003	-0.167***	-0.764***	0.002	0.004
	(0.012)	(0.019)	(0.009)	(0.009)	(0.030)	(0.049)	(0.020)	(0.021)
$+1$	-0.209***	-0.396***	0.010	0.006	-0.245***	-0.821***	-0.018	-0.054*
	(0.017)	(0.026)	(0.016)	(0.017)	(0.044)	(0.060)	(0.031)	(0.032)
$+2$	-0.572***	-1.157***	-0.041*	-0.088***	-0.552***	-1.348***	0.008	-0.111***
	(0.036)	(0.068)	(0.022)	(0.025)	(0.049)	(0.076)	(0.031)	(0.035)
$+3$	-1.017***	-2.027***	-0.064**	-0.200***	-0.921***	-1.976***	-0.046*	-0.255***
	(0.061)	(0.104)	(0.030)	(0.033)	(0.061)	(0.082)	(0.025)	(0.029)
$+6$	-1.646***	-3.473***	-0.207***	-0.708***	-1.133***	-2.774***	-0.216***	-0.756***
	(0.100)	(0.137)	(0.052)	(0.050)	(0.069)	(0.088)	(0.072)	(0.071)
$+12$	-0.423***	-3.001***	0.278**	-0.834***	-0.190	-2.506***	0.114	-0.847***
	(0.134)	(0.057)	(0.125)	(0.079)	(0.124)	(0.067)	(0.087)	(0.063)
$+18$	0.879***	-2.229***	0.802***	-0.831***	0.720***	-1.994***	0.441***	-0.834***
	(0.148)	(0.071)	(0.115)	(0.075)	(0.119)	(0.071)	(0.088)	(0.071)
$+24$	0.946***	-2.064***	0.586***	-0.879***	0.936***	-1.650***	0.505***	-0.594***
	(0.098)	(0.060)	(0.091)	(0.086)	(0.093)	(0.066)	(0.100)	(0.099)
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓	✓	✓
ΔTotal Debt $(-1, t)$	✓		✓		✓		✓	
ΔDTI Ratio $(-1, t)$		✓		✓		✓		✓
# Cohorts	347,172	347,172	118,148	118,148	347,172	347,172	118,148	118,148
Avg. Adj. R^2	0.086	0.173	0.082	0.166	0.029	0.091	0.042	0.097

Table B.8: Impact of credit utilization changes on credit scores

This table presents results examining the factors that drive the increase in MPL borrowers' credit scores after MPL loan take-up. Columns (1) and (2) (Columns (3) and (4)) report the regression results for the change in credit scores for the MPL borrowers relative to the matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Credit Score			
Monthly DID	Unmet Cohort		Bank Cohort	
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)
Pre-period				
-12	-1.597***	-1.597***	-3.434***	-3.434***
	(0.286)	(0.286)	(0.261)	(0.261)
-6	-0.943***	-0.943***	-2.632***	-2.632***
	(0.126)	(0.126)	(0.169)	(0.169)
-3	-0.172	-0.172	-1.720***	-1.720***
	(0.124)	(0.124)	(0.127)	(0.127)
-2	0.215***	0.215***	-1.084***	-1.084***
	(0.075)	(0.075)	(0.110)	(0.110)
Post-period				
$+0$	12.493***	5.009***	1.025**	-0.438**
	(0.656)	(0.195)	(0.499)	(0.193)
$+1$	38.090***	10.361***	13.029***	6.730***
	(0.878)	(0.484)	(1.062)	(0.642)
$+2$	37.365***	9.749***	12.354***	7.081***
	(0.994)	(0.694)	(0.977)	(0.586)
$+3$	27.472***	3.746***	3.770***	-0.329
	(0.716)	(0.424)	(0.745)	(0.357)
$+6$	18.284***	2.418***	0.131	-1.352***
	(0.724)	(0.450)	(0.727)	(0.269)
$+12$	7.143***	-0.153	-4.050***	-2.722***
	(0.820)	(0.576)	(0.745)	(0.403)
$+18$	1.798**	-1.396**	-5.527***	-3.910***
	(0.870)	(0.642)	(0.713)	(0.423)
$+24$	-2.153***	-3.732***	-6.538***	-5.083***
	(0.725)	(0.541)	(0.629)	(0.445)
Cohort FE	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
ΔCC Util $(-1, t)$		✓		✓
# Cohorts	347,172	347,172	118,148	118,148
Avg. Adj. R^2	0.171	0.451	0.175	0.505

Table B.9: Credit demand around MPL loan take-up

This table presents the evolution of the credit demand of MPL borrowers in the months surrounding MPL loan origination. Columns (1) and (2) proxy credit demand through the number of hard inquiries for credit cards, while Columns (3) and (4) proxy credit demand through the number of credit card accounts. Columns (1) and (3) (Columns (2) and (4)) report the regression results for the change in credit demand for the MPL borrowers relative to a matched sample of unmet credit demand borrowers (bank borrowers). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Depvar:	Log(CC Hard Inquiries (#))		Log(CC Accounts (#))	
Monthly DID	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)
Pre-period				
-12	-3.035***	-4.565***	0.160	-0.557***
	(0.193)	(0.223)	(0.154)	(0.091)
-6	-1.799***	-2.376***	0.194	-0.306***
	(0.174)	(0.139)	(0.180)	(0.053)
-3	-1.134***	-1.216***	-0.153*	-0.108***
	(0.086)	(0.091)	(0.081)	(0.039)
-2	-0.747***	-0.763***	-0.061	-0.028
	(0.063)	(0.070)	(0.046)	(0.027)
Post-period				
$+0$	-0.421	0.054	0.509***	0.295***
	(0.314)	(0.081)	(0.118)	(0.031)
$+1$	0.584*	0.496***	0.680	0.594***
	(0.312)	(0.096)	(0.500)	(0.074)
$+2$	0.996***	0.932***	1.178*	0.939***
	(0.327)	(0.108)	(0.612)	(0.087)
$+3$	1.328***	1.427***	1.729***	1.277***
	(0.343)	(0.119)	(0.584)	(0.093)
$+6$	2.115***	2.356***	2.884***	1.976***
	(0.351)	(0.149)	(0.569)	(0.104)
$+12$	3.117***	3.477***	3.553***	2.450***
	(0.370)	(0.212)	(0.535)	(0.137)
$+18$	3.583***	4.129***	2.889***	2.217***
	(0.341)	(0.289)	(0.536)	(0.193)
$+24$	3.630***	4.614***	2.196***	2.070***
	(0.341)	(0.323)	(0.533)	(0.232)
Cohort FE	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
ΔCredit Score $(-1, +1)$	✓	✓	✓	✓
# Cohorts	347,172	118,148	347,172	118,148
Avg. Adj. R^2	0.084	0.08	0.16	0.135

Table B.10: Impact of MPL loans on default rates by credit segment

This table presents the default rates of MPL borrowers' in three separate credit segments – prime (credit scores over 680), near-prime (credit scores between 620 and 680), and subprime (credit scores under 620). Results are reported for both the immediate horizon (3 months after loan origination) and the longer horizon (24 months after loan origination). Columns (1)–(2), Columns (3)–(4), and Columns (5)–(6) present results for defaults on any kind of debt, defaults on credit cards, and default on non–credit card products, respectively. Columns (1), (3), and (5) (Columns (2), (4), and (6)) report the regression results for the unmet cohort (bank cohort). Each row represents a unique event month relative to the month of MPL loan origination by MPL borrowers. For each event month specification, the independent variable captures the differential response of MPL borrowers relative to the benchmark counterfactual borrowers. The standard errors, reported in parentheses, are double clustered at the 5-digit zip-code and loan origination year-month levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	All Defaults		CC Defaults		Non-CC Defaults	
Monthly DID	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort	Unmet Cohort	Bank Cohort
MPL Coef. $\beta\{t\}$	(1)	(2)	(3)	(4)	(5)	(6)
Short-term (β^{+1})						
MPL	0.010	0.076**	0.071***	0.039*	-0.062	0.045
	(0.047)	(0.038)	(0.019)	(0.021)	(0.044)	(0.038)
MPL × Near-Prime	-0.295***	-0.055	-0.143***	0.009	-0.181***	-0.084
	(0.055)	(0.067)	(0.027)	(0.032)	(0.052)	(0.059)
MPL × Subprime	-1.792***	-0.597***	-1.282***	-0.402***	-0.647***	-0.218
	(0.170)	(0.168)	(0.074)	(0.093)	(0.135)	(0.147)
Long-term (β^{+24})						
MPL	0.078	0.246**	0.101	0.213**	-0.095	0.084
	(0.113)	(0.109)	(0.089)	(0.099)	(0.080)	(0.102)
MPL × Near-Prime	0.991***	0.787***	0.726***	0.609***	0.810***	0.565***
	(0.127)	(0.181)	(0.092)	(0.178)	(0.110)	(0.179)
MPL × Subprime	2.712***	0.766	1.583***	0.239	2.473***	1.009***
	(0.312)	(0.552)	(0.220)	(0.488)	(0.244)	(0.355)
Cohort FE	✓	✓	✓	✓	✓	✓
Matching Controls	✓	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓	✓
# Cohorts	347,172	118,148	347,172	118,148	347,172	118,148
Avg. Adj. R^2	0.106	0.097	0.086	0.082	0.029	0.042

APPENDIX C
APPENDIX FOR "SHOCKED BY BANK FUNDING SHOCKS: EVIDENCE FROM 500 MILLION CONSUMER CREDIT CARDS"

This appendix is divided into two sections. The first section documents excerpts from news articles which provide anecdotal evidence for credit limit cuts in our post shock period which ranges from 2008–2010. The second section provides variable descriptions.

A Anecdotal evidence on credit limit cuts

1. "A July 30, 2008, report by Javelin Strategy & Research says that of the 13 top-tier credit card issuers it surveyed, eight said that as a direct result of current economic conditions, they had reduced consumers' credit lines. It's a move took Jerry Jacobs by surprise. About eight months ago, one credit card bank reduced his $10,000 limit to $6,200, just above his card balance. The Florida resident says he's never missed a payment or been late with a bill, and his phone calls to the company netted no real reason for the change."
   Source: `https://www.creditcards.com/credit-card-news/lending-crisis-credit-score-cut-limits-1270.php`

2. "After years of flooding Americans with credit card offers and sky-high credit lines, lenders are sharply curtailing both, just as an eroding economy squeezes consumers. The pullback is affecting even creditworthy consumers . . . Capital One, another big issuer, for example, has aggressively shut down inactive accounts and reduced customer credit lines by 4.5 percent in the second quarter from the previous period, according to regulatory filings."
   Source: `https://www.nytimes.com/2008/10/29/business/29credit.html`

3. "Credit card companies are not immune to the credit crisis, and one way they're protecting themselves is by lowering credit limits wherever and whenever they can. You might have a perfect payment history and still wake up to a $5,000 card limit that's been reduced to $500. One of the biggest victims in this whole economic meltdown — besides the millions who've lost their jobs – is the world's financial liquidity reserve. That means there's no credit anywhere because there's no money to lend."
   Source: `https://www.gobankingrates.com/credit-cards/advice/why-credit-limits-cut/`

4. "Many Americans have come to rely on credit cards to cover everyday expenses like groceries, gasoline and medical bills, in addition to big-ticket items and luxuries. While consumer spending, the nation's economic engine, has been surprisingly resilient of late, a more sweeping reduction in credit card limits could pose serious challenges for hard-pressed consumers and, in turn, the broader economy."

"Washington Mutual cut back the total credit lines available to its cardholders by nearly 10 percent in the first quarter of the year, according to an analysis of bank regulatory data. HSBC HOldings, Target and Wells Fargo each trimmed their credit card lines by about 3 percent."
Source: `https://www.nytimes.com/2008/06/21/business/21credit.html`

5. "Johann Beukes, a software engineering manager for Bankrate Inc. based in North Palm Beach, Fla., logged on to American Express' Web site recently to make a payment and discovered his credit line had been reduced by $5,000, despite the fact that he's been a cardholder for more than 10 years and has a credit score north of 800.

   "I called them up the next day and asked why they were doing this, since we've never had a late payment," he says.

   After lodging complaints with three company representatives, Beukes finally was told that his credit line was lowered because American Express wants to reduce its risk because of the credit crisis. Nothing personal."
   Source: `https://www.bankrate.com/finance/financial-literacy/coping-with-cut-credit-1.aspx`

6. "About one in five cardholders had their credit limits reduced recently, according to a [2008] July survey by Consumer Action, a San Francisco-based consumer advocacy group."

   "Meredith Whitney, a banking analyst at Oppenheimer & Co., predicts card issuers will cut credit lines by $2 trillion-plus over the next 18 months [2008–2010]."
   Source: `https://www.bankrate.com/finance/financial-literacy/coping-with-cut-credit-1.aspx`

B Variable definitions

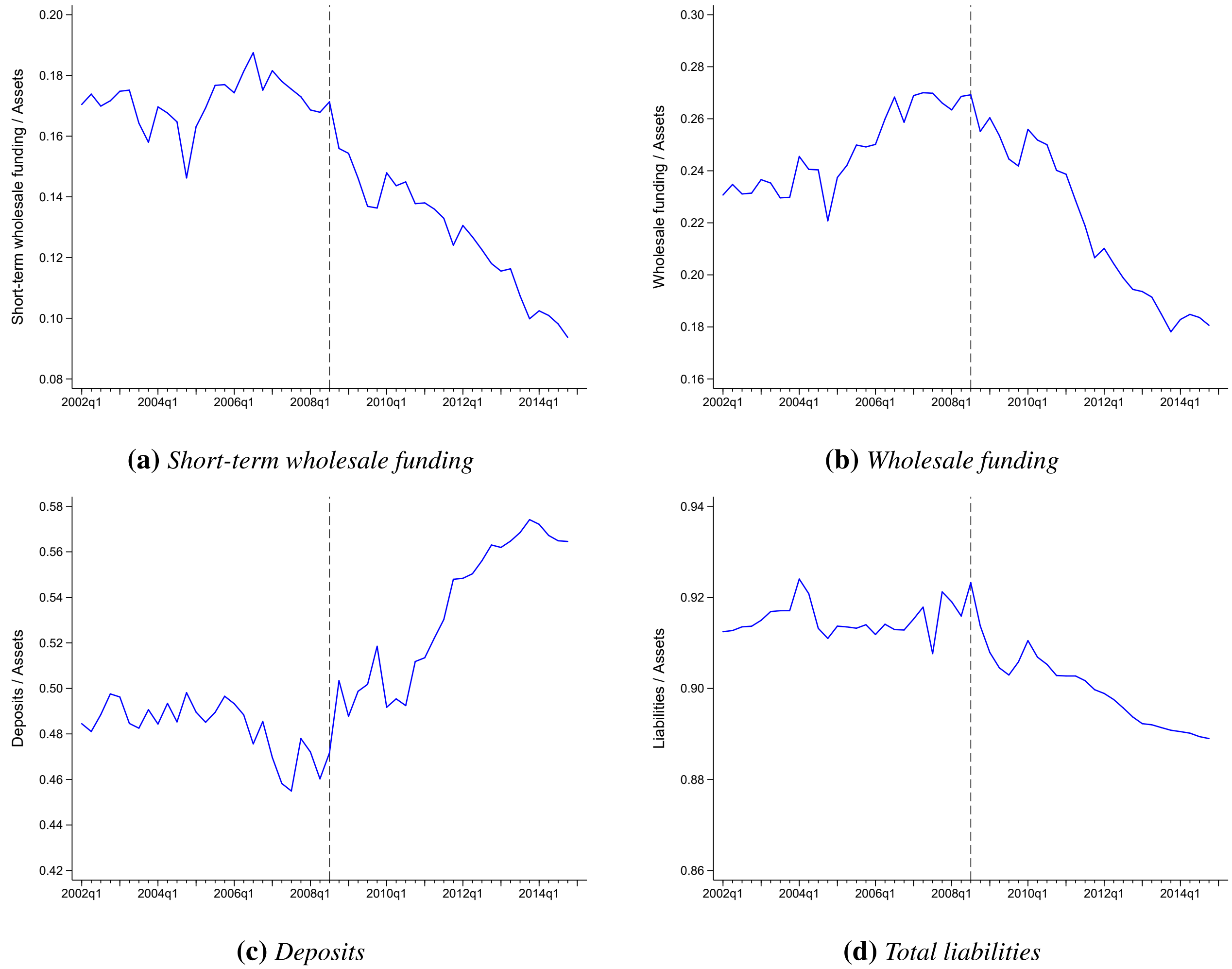
Data for the following variable definitions were gathered from the quarterly bank holding company Y-9C filings.

- *Wholesale funding* = Fed funds purchased $+$ Repo $+$ Other liabilities maturity $<$ 1 yr $+$ Other liabilities maturity $>$ 1 yr
- *Short-term wholesale funding* = Fed funds purchased $+$ Repo $+$ Other liabilities maturity $<$ 1 yr
- Exposure = Short-term wholesale funding / Deposits
- *Fed funds purchased* = BHDMB993
- *Repo* (repurchase agreements) = BHCKB995
- *Other liabilities maturity $<$ 1 yr* = BHCK2332
- *Other liabilities maturity $>$ 1 yr* = BHCK2333

- *Assets* = BHCK2170
- *Deposits* = BHDM6631 + BHDM6636 + BHFN6631 + BHFN6636
- *Equity capital* = BHCK3210
- *Liquid assets* = BHCK0081 + BHCK0395 + BHCK0397 + BHCK1754 + BHCK1773 + BHDMB987 + BHCKB989
- *CC loans* (credit card loans) = BHCKB538
- *Mortgage loans* = BHCK1410
- *C&I loans* (commercial and industrial loans) = BHCK1763 + BHCK1764
- *Net income* = BHCK4340
- *ROE* (return on equity) = Net income/Equity capital
- *Non-perf loans* (non-performing loans) = BHCK5525 + BHCK5526
- *Risk-based capital ratio* = (BHCK3792/BHCKA223)$\times$100
- *CC Business* (%) = (CC loans / Assets)$\times$100
- *Non-Perf Loans* (%) = (Non-perf loans/ Assets)$\times$100

Data for the following variable definitions were gathered from the semi-annual archives of the credit bureau data.

- *Credit card delinquency* = Average percentage of delinquent credit cards at the bank level in the pre-shock period, where 'delinquency' is defined as being 90 days past due on the minimum payment requirement on credit cards
- *Total-debt related accounts* = Total number of mortgage, auto, and student loan accounts
- *Subprime (%)* = Average percentage of subprime customers at the bank level in the pre-shock period; customers with credit scores below 620 on the Vantage 3.0 scale are identified as "subprime"
- *Months CC open (log)* = Number of months the credit card is open as of January 2008
- *Accounts open (log)* = Number of distinct open debt accounts the borrower has with the credit card issuing bank



(a) *Short-term wholesale funding*

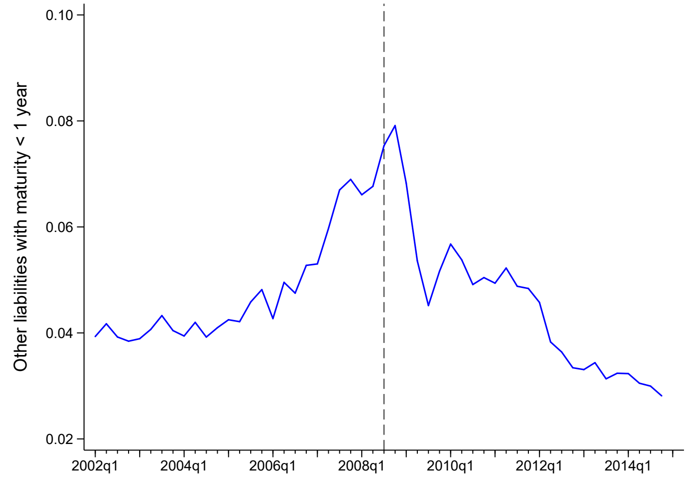
(b) *Wholesale funding*

(c) *Deposits*

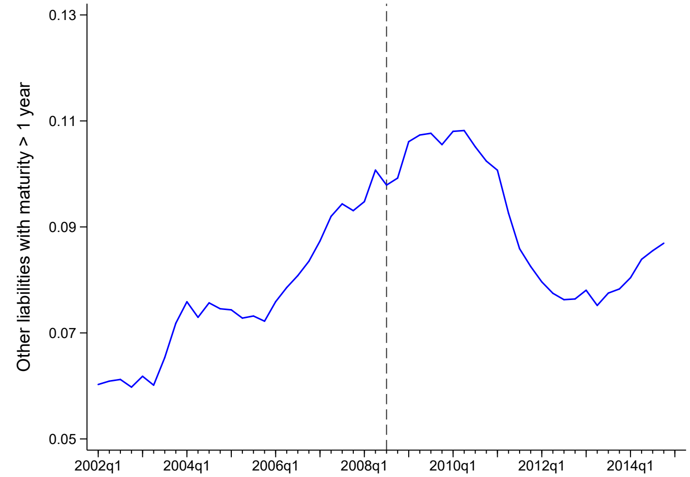
(d) *Total liabilities*

Figure C.1: Bank funding measures over time

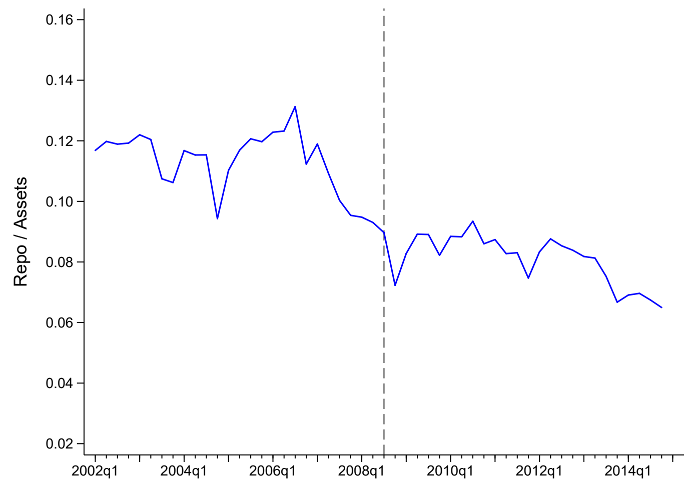
The figure presents the trend of the various sources of funding as a proportion of assets for the banks in our sample over time. These data are gathered from the quarterly Y-9C filings of U.S. bank holding companies (BHCs).



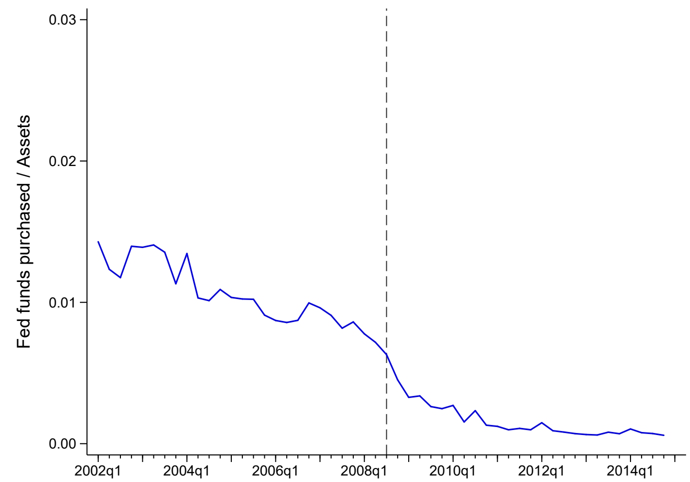
(a) *Liabilities with maturity < 1 year*



(b) *Liabilities with maturity > 1 year*



(c) *Repos*



(d) *Fed funds purchased*

Figure C.2: Components of wholesale funding over time

The figure presents the trend of the various components of wholesale funding as a proportion of assets for the banks in our sample over time. These data are gathered from the quarterly Y-9C filings of U.S. BHCs.

Table C.1: Summary statistics at the consumer level

This table presents the summary statistics at the consumer level for the full sample and fixed effects model (FE) sample using credit bureau data in the pre-shock period. The pre-shock period consists of three semiannual archives namely, January 2007, July 2007, and January 2008. The data are collapsed to obtain a single consumer-level cross-section in the pre-shock period by averaging across time.

	Full Sample				FE Sample			
	N	Mean	Median	Std. dev.	N	Mean	Median	Std. dev.
Borrower fundamentals								
Credit score	133,507,048	725	743	83.9	54,174,946	735	751	76.1
Monthly income ($)	133,507,048	3,735	3,583	1,436	53,867,498	3,963	3,750	1,388
Debt-to-income ratio (DTI)	133,507,048	28.4	23	26.8	53,127,992	31.2	27.3	25.7
Credit card debt								
Credit card accounts	133,507,048	3.7	3	2.54	54,174,952	5.34	5	2.86
Credit card balance	133,507,048	4,985	1,980	7,459	54,174,952	7,505	3,655	9,724
Credit card utilization	120,364,050	0.29	0.16	0.31	51,166,136	0.27	0.15	0.29
Other debt								
Total debt related accounts	132,104,446	8.69	7.67	5.3	53,385,332	10.8	10	5.65
Mortgage balance ($)	53,407,329	184,645	134,378	171,887	24,113,774	184,457	135,569	169,268

Table C.2: Bank-level aggregate evidence

This table shows the relation between the pre-shock dependence on short-term wholesale funding, the change in bank funding, and the total credit card loans. The pre-shock period ranges from 2006Q1 to 2007Q4, and the post-shock period ranges from 2009Q1 to 2010Q4. Bank-level variables are obtained from the quarterly BHC Y-9C and Call report regulatory filings. The quarterly data are first collapsed to obtain a single bank-level cross-section separately in the pre-shock and post-shock periods by averaging across time. Then, the change in bank funding and credit card loans is computed by taking the log difference from the post- to the pre-shock period. The dependence on short-term wholesale funding (exposure) is measured as the ratio of short-term wholesale funding to total deposits. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Panel A: Dependence on short-term wholesale funding and change in bank funding

Depvar:	ΔST Wholesale (1)	ΔWholesale (2)	ΔTot Liabilities (3)	ΔTot Equity (4)
Exposure	-0.425***	-0.324***	-0.272***	-0.151
	(-4.03)	(-2.95)	(-3.34)	(-1.75)
Assets (log)	0.946***	0.812***	0.400***	0.283*
	(3.79)	(3.57)	(3.74)	(1.80)
Constant	-0.511**	-0.324	0.148	0.254**
	(-2.39)	(-1.56)	(1.24)	(2.49)
N	18	18	18	18
Adj. R^2	0.469	0.334	0.412	0.181
Orthog-Exposure R^2	0.531	0.412	0.481	0.278

Panel B: Dependence on short-term wholesale funding and change in credit card loans

Depvar: ΔCC Loans	(1)	(2)	(3)
Exposure	-0.211*	-0.197*	-0.210**
	(-1.89)	(-1.81)	(-2.26)
Assets (log)	0.011	-0.032	0.123
	(0.04)	(-0.11)	(0.53)
CC Business (%)		-0.237	-0.251
		(-1.10)	(-1.35)
Non-perf loans (%)			0.261
			(1.61)
Constant	0.764***	0.734***	0.656***
	(3.40)	(3.41)	(3.12)
N	18	18	18
Adj. R^2	0.333	0.324	0.373
Orthog-Exposure R^2		0.411	

Table C.3: Effect of funding shock on extensive margin

This table shows the relation between the pre-shock dependence on short-term wholesale funding and the opening and closure of a credit card. The regressions are estimated at the credit card level. The dependent variable in Columns (1) and (2) is *New* which is a dummy variable which takes the value 1 if a new card is issued by the bank in the post-shock period (i.e., after July 2008), and is 0 otherwise. The dependent variable in Columns (3) and (4) is *Closed* which is a dummy variable that takes the value 1 if an existing credit card in the pre-shock period (i.e., prior to July 2008) is closed in the post-shock period, and is 0 otherwise. The dependence on short-term wholesale funding (exposure) is measured as the ratio of short-term wholesale funding to total deposits. The standard errors are clustered at the bank-state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

	New Cards		Closed Cards	
	(1)	(2)	(3)	(4)
Exposure	-1.974***	-2.340***	4.358***	4.345***
	(-3.68)	(-5.16)	(4.76)	(5.41)
Individual FE		✓		✓
Bank characteristics	✓	✓	✓	✓
Bank performance	✓	✓	✓	✓
Lending quality	✓	✓	✓	✓
Credit card controls			✓	✓
N	164,445,583	164,445,583	344,803,931	344,803,931
Adj. R^2	0.020	0.057	0.028	0.192

Table C.4: Robustness: Controlling for potential alternate channels

This table presents results assuaging concerns of a particular type of bank driving the baseline findings presented in Table 3.2. The table shows the relation between the pre-shock dependence on short-term wholesale funding and the change in credit card limits using the credit bureau data, where the focus is on individuals who hold multiple credit cards issued by the same 'type' of bank. In column (1), bank type is determined by the size of the bank's assets. Banks with assets larger (smaller) than the sample median are classified as large (small) banks. Similarly, in columns (2), bank type is determined through the banks' capital ratios. In column (3), the sample of banks is partitioned into high- and low-groups on the basis of unused credit card limits as a percentage of total extended credit cards limits. Finally, in column (4), bank type is identified through lending quality, as proxied by the percentage of subprime borrowers constituting the bank's clientele. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Credit card–level data are first collapsed to obtain a single credit card–level cross-section separately in the pre-shock and post-shock period by averaging across time. Then the change in credit card limits is computed by taking the log difference from the post- to the pre-period. The dependence on short-term wholesale funding (exposure) is measured as the ratio of short-term wholesale funding to total deposits. The standard errors are clustered at the bank–state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

Individual FE interacted with Group:	Size indicator	Cap ratio indicator	Unused CC limits indicator	% Subprime indicator
Depvar: ΔCC Limit	(1)	(2)	(3)	(4)
Exposure	-4.792***	-5.558***	-5.911***	-3.195***
	(-15.88)	(-14.31)	(-15.76)	(-9.40)
Individual×Group FE	✓	✓	✓	✓
Bank characteristics	✓	✓	✓	✓
Bank performance	✓	✓	✓	✓
Lending quality	✓	✓	✓	✓
Credit card controls	✓	✓	✓	✓
N	131,652,681	118,081,930	155,904,113	131,574,880
Adj. R^2	0.086	0.126	0.090	0.086

Table C.5: Effect of funding shock on credit card limits: Individual-bank level analysis

This table shows the robustness of the relation between the pre-shock dependence on short-term wholesale funding and the change in credit card limits and balances using individual-bank level analysis. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Individual-bank level data are first collapsed to obtain a single individual-bank level cross-section separately in the pre-shock and post-shock periods by averaging across time. Then, the change in credit card limits is computed by taking the log difference from the post- to the pre-shock period. The dependence on short-term wholesale funding is measured as the ratio of short-term wholesale funding to total deposits. We aggregate credit-card controls to the individual-bank level. The standard errors are clustered at the bank–state level. *, **, and *** indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar:	ΔCC Limit	ΔCC Balance	
	(1)	(2)	(3)
Exposure	-4.964***	-12.798***	
	(-11.87)	(-5.72)	
Δ CC limit (instrumented)			2.578***
			(5.21)
N	123,335,977	123,335,977	123,335,977
Adj. R^2	0.062	0.112	0.067
F-stat (Excl. instru)			140.87
Individual FE	✓	✓	✓
Bank characteristics	✓	✓	✓
Bank performance	✓	✓	✓
Borrower quality	✓	✓	✓
Credit card controls	✓	✓	✓

Table C.6: Effect of funding shock on credit card limits: Robustness

This table presents robustness for the baseline results in Table 3.2 with alternate measures for bank exposure and different levels of clustering. The table shows the relation between the pre-shock dependence on short-term wholesale funding and the change in credit card limits using the credit bureau data. Both the pre-shock and post-shock periods consist of three semiannual archives. The pre-shock period includes the January 2007, July 2007, and January 2008 semiannual archives, while the three semiannual archives for the post-shock period are January 2009, July 2009, and January 2010. Credit card–level data are first collapsed to obtain a single credit card-level cross-section separately in the pre-shock and post-shock period by averaging across time. Then the change in credit card limits is computed by taking the log difference from the post- to the pre-period. The dependence on short-term wholesale funding (exposure) is measured as the ratio of short-term wholesale funding to total deposits. The standard errors are clustered at the bank–state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

Depvar: Δ*CC Limit*	(1)	(2)	(3)	(4)
Exposure (w.r.t. deposits)	-4.750***		-4.750***	
	(-12.89)		(-9.04)	
Exposure (w.r.t. assets)		-4.532***		-4.532***
		(-11.72)		(-7.17)
Individual FE	✓	✓	✓	✓
Bank characteristics	✓	✓	✓	✓
Bank performance	✓	✓	✓	✓
Lending quality	✓	✓	✓	✓
Credit card controls	✓	✓	✓	✓
N	158,432,533	158,432,533	158,432,533	158,432,533
Adj. R^2	0.090	0.090	0.090	0.090
Bank×State clustering	✓	✓		
Bank clustering			✓	✓

Table C.7: Reduced form: Heterogeneity in consumers' total debt response to funding shock: Reduced form

This table shows results from reduced form regressions that examine consumers' differential total debt response to their banks' short-term wholesale funding shock. Columns (1)–(3) consist of subsamples of individuals whose aggregate utilization ratios, computed using all the credit cards issued to them, is between 0–50%, 50–90%, and greater than 90%, respectively. Columns (4)–(6) consist of subsamples of subprime (< 620), near-=prime ($>= 620$ and < 680), and prime ($>= 680$) individuals, classified based on their credit score. The standard errors are clustered at the bank–state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

	Utilization			Credit score		
	0–50% (1)	50–90% (2)	>90% (3)	Subprime (4)	Near-prime (5)	Prime (6)
Weighted exposure	2.548***	0.006	-1.421***	-4.914***	-0.575**	2.318***
	(7.18)	(0.03)	(-5.76)	(-10.12)	(-2.00)	(7.35)
Zip-code FE	✓	✓	✓	✓	✓	✓
Consumer quality	✓	✓	✓	✓	✓	✓
N	99,045,677	20,421,935	14,380,579	19,551,761	20,007,886	94,598,483
Adj. R^2	0.017	0.059	0.070	0.071	0.043	0.023

Table C.8: Heterogeneous effect of funding shock on total debt balance: Robustness

This table shows the robustness of the heterogeneous effects of credit card limit cuts induced by banks' short-term wholesale funding shock on total debt balance after controlling for HELOC limit cuts induced by banks' short-term wholesale funding shock. The dependent variable is the change in total debt balances across all debt-related accounts of the consumer. The analysis focuses on individuals that have HELOCs prior to the funding shock. Subprime (< 620), near-prime ($>= 620$ and < 680), and prime ($>= 680$) are classified based on the individual's credit score before the funding shock. The standard errors are double clustered at the credit card bank–state and HELOC bank–state level. *, **, and ***, indicate a significance greater than 10%, 5%, and 1%, respectively.

	Sub-prime		Near-prime		Prime	
Depvar: Δ Agg. Debt Balance	(1)	(2)	(3)	(4)	(5)	(6)
Δ Agg. CC limit (instrumented)	0.217***	0.255***	0.068***	0.069***	-0.162***	-0.159***
	(7.88)	(2.91)	(6.82)	(7.37)	(-7.26)	(-8.80)
Δ Agg. HELOC limit (instrumented)		0.246		-0.059		-0.239***
		(0.52)		(-0.56)		(-3.38)
Zip-code FE	✓	✓	✓	✓	✓	✓
Consumer quality	✓	✓	✓	✓	✓	✓
N	1,079,680	1,079,680	2,197,811	2,197,811	15,092,835	15,092,835
Adj. R^2	0.039	0.006	0.031	0.025	-0.008	-0.035
F-stat (Excl. instr)	96.78		153.85		107.81	
Cond. F-stat (Δ Agg. CC limit)		16.29		133.38		105.63
Cond. F-stat (Δ Agg. HELOC limit)		13.61		119.77		24.82

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