# Do Banks Pass Through Credit Expansions? The Marginal Profitability of Consumer Lending During the Great Recession*

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**Abstract**

The effect of bank-mediated stimulus on household borrowing depends on whether banks pass through credit expansions to households with a high marginal propensity to borrow (MPB). We use panel data on 14.2 million U.S. credit card accounts and 812 credit limit regression discontinuities to estimate the MPB for households with different FICO credit scores. We find substantial heterogeneity, with a \$1 increase in credit limits raising total unsecured borrowing after 12 months by 58 cents for consumers with the lowest FICO scores ($\leq$660) while having no effect on total borrowing by consumers with the highest FICO scores (> 740). We use the same credit limit regression discontinuities to estimate banks' marginal propensity to lend out of a decrease in their cost of funds. For the lowest FICO score households, higher credit limits quickly reduce marginal profits, limiting the pass-through of credit expansions to those households. We estimate that a 1 percentage point reduction in the cost of funds raises optimal credit limits by \$135 for consumers with FICO scores below 660 versus \$1,478 for consumers with FICO scores above 740. We conclude that banks have the least incentive to pass through credit expansions to households that want to borrow the most and discuss the implications for bank-mediated stimulus.

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During the Great Recession, policymakers in Europe and the U.S. sought to stimulate the economy by providing banks with lower-cost capital and liquidity. The hope was that banks would pass through these credit expansion to households and businesses, thereby increasing equilibrium borrowing, spending, and investment.[1] Due in part to the slow recovery, this approach has been questioned, with many concluding that credit expansions were less successful than anticipated at stimulating economic activity (e.g., Taylor, 2014; Goodhart, 2015; Sufi, 2015).[2]

The effect of bank-mediated stimulus on household borrowing and spending depends on whether banks pass through credit expansions to households with a high marginal propensity to borrow (MPB). A growing body of research finds that low credit score households are the most credit constrained, suggesting that credit expansions that target these households will have the largest aggregate effects (e.g., Gross and Souleles, 2002). A separate literature shows that the low credit score segment of the lending market exhibits substantial asymmetric information (e.g., Adams, Einav and Levin, 2009). As we discuss below, asymmetric information can reduce banks' incentives to expand credit, because higher credit limits lead to higher rates of default. These findings raise the concern that banks' marginal propensity to lend (MPL) is lowest exactly for those households with the highest MPB, reducing the impact of bank-mediated stimulus through this channel.

We evaluate this concern by estimating heterogeneous MPBs and MPLs in the U.S. credit card market during the Great Recession. We use panel data on all credit cards issued by the 8 largest U.S. banks. These data, assembled by the Office of the Comptroller of the Currency (OCC), provide us with account-level information on contract terms, utilization, payments, and costs at the monthly level for more than 400 million credit card accounts between January 2008 and December 2014.

Our research design exploits the fact that banks sometimes set credit limits as discontinuous func-

[1]Bernanke (2008) explained that "the expansion of Federal Reserve lending is helping financial firms cope with reduced access to their usual sources of funding and thus is supporting their lending to nonfinancial firms and households." When introducing the Financial Stability Plan, Treasury Secretary Geithner (2009) argued that "the capital will come with conditions to help ensure that every dollar of assistance is used to generate a level of lending greater than what would have been possible in the absence of government support." In Europe, similar schemes were put in place in order to reduce the cost of capital of those banks that expand lending to the non-financial sector and households (e.g., the "Funding for Lending" scheme of the Bank of England, and the "Target Long-Term Refinancing Option" of the European Central Bank).

[2]The *Wall Street Journal* reports that "Fed officials have been frustrated in the past year that low interest rate policies haven't reached enough Americans to spur stronger growth the way economics textbooks say low rates should. By reducing interest rates – the cost of credit – the Fed encourages household spending, business investment and hiring, [. . . ]. But the economy hasn't been working according to script." The *Financial Times* reports: "[Lending data] illustrate the scale of challenge for a Bank of England scheme aimed at giving banks cheap money to lend. [T]he quantity of new loans to businesses and households has not improved, and the price of mortgage money is rising, not falling." The *Economist* concludes: "[I]t seems clear that current circumstances are causing these monetary policy actions to be far less effective than they otherwise would be. Marginal spenders are constrained by their desire (or need) to retrench. Most of the people who get the biggest benefit from central bank action are the people who already own lots of financial assets (the rich)."

tions of consumers' FICO scores. For example, a bank might grant a $2,000 credit limit to applicants with a FICO score below 720 and a $5,000 credit limit to applicants with a FICO score of 720 or above. If other borrower and contract characteristics trend smoothly through this cutoff, we can treat the different credit limits as assigned quasi-randomly in the vicinity of the cutoff and use a regression discontinuity strategy to identify the causal impact of providing extra credit at prevailing interest rates. We identify a total of 812 credit limit discontinuities for different credit cards originated in our sample. These discontinuities are detected at all parts of the FICO score distribution, and we observe 14.2 million new credit cards issued to borrowers within 50 FICO score points of a cutoff.

Using this regression discontinuity design, we estimate substantial heterogeneity in the MPB across the FICO score distribution. For the lowest FICO score group ($\leq 660$), a $1 increase in credit limits raises borrowing volumes on the treated credit card by 55 cents at 12 months after origination. This effect is due to increased spending and is not explained by a shifting of borrowing across different credit cards. For the highest FICO score group ($> 740$), we estimate a 22% effect on the treated card that is entirely explained by a shifting of borrowing across credit cards, with an increase in credit limits having no effect on total borrowing. These estimates suggest that bank-mediated stimulus will only raise aggregate borrowing if credit expansions are passed through to low FICO score households, which might conflict with other policy objectives, such as the de-risking of bank balance sheets.

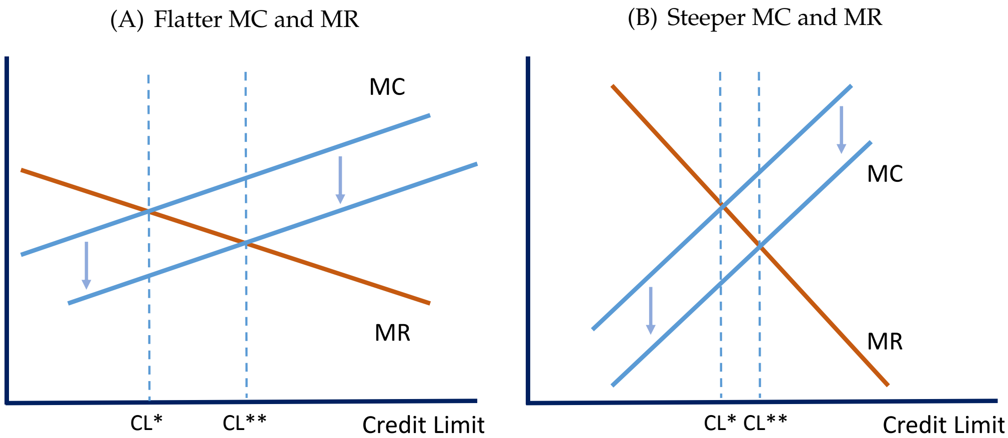
We next consider how banks pass through credit expansions to different households. Directly estimating a bank's marginal propensity to lend out of a change in its cost of funds is difficult because shocks to banks' cost of funds are typically correlated with shocks to the expected profitability of lending. For instance, reductions in the federal funds rate generally occur during recessions when banks are also updating their expectations about future default rates. Time series analysis, therefore, can lead to substantially biased estimates of the effect of changes in the cost of funds on credit supply.

Our approach is to build a simple model of optimal credit limits that characterizes a bank's MPL with a small number of parameters we can estimate directly.[3] This approach requires that we assume bank lending responds optimally on average to a *change* in the cost of funds and that we can measure the incentives faced by banks. We think both assumptions are reasonable in our setting. Credit card lending is highly sophisticated and our estimates of bank incentives are fairly precise.[4]

[3]We show that in the credit card market it is credit limits, not interest rates, that are the main margin of response for lenders (see Ausubel, 1991; Calem and Mester, 1995; Stavins, 1996; Stango, 2000).

[4]Even if banks did not behave optimally during the Great Recession, our approach is useful for predicting bank responses during future crises, since assuming banks will behave optimally in the future seems better than alternative assumptions.

**Figure 1:** Pass-Through into Credit Limits




**Note:** Figure shows marginal cost (MC) and marginal revenue (MR) for lending to observationally identical borrowers. A reduction in the cost of funds shifts the marginal cost curve down, and raises equilibrium credit limits (CL* $\rightarrow$ CL**). Panel A considers a case with relatively flat MC and MR curves; Panel B considers a case with steeper MC and MR curves.

In our model, banks sets credit limits at the level where the marginal revenue from a further increase in credit limits equals the marginal cost. A decrease in the cost of funds – e.g., due to an easing of monetary policy, a reduction in capital requirements, or a market intervention that reduces financial frictions – reduces the cost of extending a given unit of credit and corresponds to a downward shift in the marginal cost curve. As shown in Figure 1, such a reduction has a larger effect on credit limits when marginal revenue and marginal costs are relatively flat (Panel A) than when these curves are relatively steep (Panel B).

An important determinant of the slope of marginal costs is the degree of asymmetric information, which includes both adverse selection and moral hazard. With adverse selection, an increase in credit limits disproportionally raises borrowing by households with a higher probability of default, increasing marginal costs and reducing the MPL. Higher credit limits can also raise marginal costs holding the distribution of marginal borrowers fixed. Since higher credit limits increase debt levels and higher debt levels make default more likely, an increase in credit limits can raise default probabilities and marginal costs without any change in the composition of the marginal borrowers. This mechanism arises in models of moral hazard but does not require strategic behavior by the borrower. Estimating the slope of marginal costs therefore allows us to quantify the effect of a broad set of factors for pass-through without requiring us to untangle their relative importance.

Using the same quasi-exogenous variation in credit limits, we find that the (positive) slope of the marginal cost curve is largest for the lowest FICO score borrowers, driven by steeply upward sloping marginal chargeoffs for these households. We also find that the (negative) slope of the marginal revenue curve is steeper, since marginal fee revenue, which is particularly important for low FICO lending, is decreasing in credit limits. Taken together, these estimates imply that a 1 percentage point reduction in the cost of funds increases optimal credit limits by $135 for borrowers with FICO scores below 660 compared with $1,478 for borrowers with FICO scores above 740.[5]

This negative correlation between the MPL and the MPB for households with different FICO scores has important implications for the effectiveness of bank-mediated stimulus. We find that correctly accounting for this negative correlation reduces the estimated effect of a decrease in the cost of funds on total borrowing after 12 months by at least 68%, relative to a naive calculation that estimates this effect as the product of the average MPL and the average MPB in our data.[6]

We view our paper as making two main contributions. First, we think our analysis of the credit card market is important because credit cards are the primary marginal source of credit for many U.S. households. According to the 2010 Survey of Consumer Finances, 68% of households had a credit card versus 10.3% for a home equity line of credit and 4.1% for "other" lines of credit. Moreover, credit cards were particularly important during the Great Recession when many homeowners were underwater and unable to borrow against home equity. For instance, credit cards issued to consumers with FICO scores above 740 had average borrowing volumes of $1,970 at one year after origination, indicating that credit cards were a key source of credit even in the upper range of the FICO distribution.

Second, we believe the conceptual point that the pass-through of changes to banks' cost of funds is muted for borrowers with steeper marginal costs – e.g., because of asymmetric information – applies to a broad set of lending markets. These include small business loans, mortgages, and newly-emerging online lending markets, all of which feature significant potential for adverse selection and moral hazard (see Petersen and Rajan, 1994; Karlan and Zinman, 2009; Keys et al., 2010; Hertzberg, Liberman and Paravisini, 2015). Of course, whether pass-through in these markets is lower for households and businesses with a higher MPB is a question for further research.

Our empirical approach follows a literature that has estimated the importance of credit constraints by analyzing household responses to income shocks (e.g., Souleles, 1999; Stephens, 2003, 2008; John-

[5]Our estimates are obtained from credit cards that were originated during an economic crisis, which is precisely the period during which stimulous is regularly considered. Therefore, even if these slopes varied with aggregate economic activity, our estimates are appropriate for inferring banks' marginal propensity to lend during economic crises.

[6]This muted pass-through applies symmetrically to increases in the cost of funds. This means that attempts by central banks to "lean against" credit bubbles by raising interest rates may also precipitate smaller-than-average changes in credit availability for those households that borrow the most.

son, Parker and Souleles, 2006; Blundell, Pistaferri and Preston, 2008).[7] Most closely related is Gross and Souleles (2002), who estimate MPBs using time-series variation in credit limits. We view our contribution of estimating MPBs and MPLs in the same context as a natural next step in this literature.

Our paper highlights the aggregate and distributional effect of the bank lending channel of monetary policy (Bernanke and Gertler, 1995; Kashyap and Stein, 1995) and, in particular, the effect of monetary policy on bank lending during the Great Recession (see Jiménez et al., 2012, 2014, for recent contributions). Our finding of heterogenous MPLs out of a reduction in bank's cost of funds complements recent work by Doepke and Schneider (2006), Coibion et al. (2012), Scharfstein and Sunderam (2013), Auclert (2014), Keys et al. (2014), Di Maggio, Kermani and Ramcharan (2014) and Hurst et al. (2015), who investigate heterogeneity in the transmission of monetary policy through other channels.

Finally, we relate to a large literature that has identified declining household borrowing volumes as a proximate cause of the Great Recession (Mian and Sufi, 2010, 2012; Hall, 2011).[8] We provide a reason why bank-mediated credit expansions might have been less successful than anticipated in stimulating household borrowing and spending. We also estimate high marginal propensities to borrow out of extra credit for households with low FICO scores. This finding suggests that, at the margin, aggregate borrowing volumes were constrained by restricted credit supply – and not by a decline in credit demand driven by voluntary household deleveraging.

Our findings are subject to a number of caveats. First, our estimates of the direct effects of a reduction in the cost of funds do not capture general equilibrium effects that might arise from the increased spending of low FICO score households. Second, our results are not informative about the effect of monetary policy through other channels, such as a redistribution from savers to borrowers, or in preventing a collapse of the banking sector. Third, while we identify one important aspect of why bank-mediated stimulus was relative ineffective at raising household borrowing during the Great Recession, other forces also played a role. For instance, stress tests and higher capital requirements may have increased the shadow cost of lending, particularly to low FICO score borrowers, and thus might have offset the policies we consider that were designed to reduce banks' cost of funds. Finally, our paper does not assess the desirability of stimulating household borrowing from a macroeconomic sta-

[7] Other papers include Zeldes (1989), Hsieh (2003), Agarwal, Liu and Souleles (2007), Aaronson, Agarwal and French (2012), Agarwal and Qian (2014), Baker (2013), Dobbie and Skiba (2013), Parker et al. (2013), Bhutta and Keys (2014), Gelman et al. (2015), and Sahm, Shapiro and Slemrod (2015). See Jappelli and Pistaferri (2010) and Zinman (2014) for reviews.

[8] The literature includes, among others, Guerrieri and Lorenzoni (2011), Philippon and Midrigin (2011), Eggertsson and Krugman (2012), Justiniano, Primiceri and Tambalotti (2013), Mian, Rao and Sufi (2013), and Korinek and Simsek (2014).

bility or welfare perspective. For example, while extending credit to low FICO households might lead to more borrowing and consumption in the short run, we do not evaluate the effect of the resulting increase in leverage on the probability of a future household balance sheet crisis.

The rest of the paper proceeds as follows: Section 1 presents background on the determinants of credit limits and describes our credit card data. Section 2 discusses our regression discontinuity research design. Section 3 verifies the validity of this research design. Section 4 presents our estimates of the marginal propensity to borrow. Section 5 provides a model of credit limits. Section 6 presents our estimates of the marginal propensity to lend. Section 7 concludes.

# 1 Background and Data

Our research design exploits quasi-random variation in the credit limits set by credit card lenders (see Section 2). In this section, we describe the process by which banks determine these credit limits and introduce the data we use in our empirical analysis. We then describe our process for identifying credit limit discontinuities and present summary statistics on our sample of quasi-experiments.

## 1.1 How Do Banks Set Credit Limits?

Most credit card lenders use credit scoring models to make their pricing and lending decisions. These models are developed by analyzing the correlation between cardholder characteristics and outcomes like default and profitability. Banks use generic or custom models that are either developed internally or purchased from external credit scoring companies. The most commonly used external credit scores are called FICO scores, and are developed by the Fair Isaac Corporation. FICO scores are used by over 80% of the largest financial institutions and primarily take into account a consumer's payment history, credit utilization, length of credit history, and the opening of new accounts (see Chatterjee, Corbae and Rios-Rull, 2011). Scores range between 300 and 850, with higher scores indicating a lower probability of default. The vast majority of the population has scores between 550 and 800.

Each bank develops its own policies and risk tolerance for credit card lending, with lower credit score consumers generally assigned lower credit limits. Setting cutoff scores is one way that banks assign credit limits. For example, banks might split their customers into groups based on their FICO score and assign each group a different credit limit (FDIC, 2007).[9] This would lead to discontinuities

[9]While it might seem more natural to set credit limits as continuous functions of FICO scores, the use of "buckets" for pricing is relatively common across many markets. For example, many health insurance schemes apply common pricing for individuals within age ranges of five years, and large retailers often set uniform pricing rules within sizable geographic

in credit limits extended on either side of the FICO score cutoff. Alternatively, banks might use a "dual-scoring matrix," with the FICO score on the first axis and another score on the second axis, and cuttoff levels on both dimensions. In this case, depending on the distribution of households over the two dimensions, the average credit limit might be smooth in either dimensions, even if both dimensions have cutoffs. The resulting credit supply rules can change frequently and will differ for different credit cards issued by the same bank.

### 1.2 Data

Our main source of data is the Credit Card Metrics (CCM) data set assembled by the U.S. Office of the Comptroller of the Currency (OCC). The OCC supervises and regulates nationally-chartered banks and federal savings associations. In 2008, the OCC initiated a request to the largest banks that issue credit cards to submit data on general purpose, private label, and small business credit cards. The purpose of the data collection was to have more timely information for bank supervision.

The CCM data has two components. The main data set contains account-level information on credit card utilization (e.g., purchase volume, measures of borrowing volume such as ADB), contract characteristics (e.g., credit limits, interest rates), charges (e.g., interest, assessed fees), performance (e.g., chargeoffs, days overdue), and borrower characteristics (e.g., FICO scores) for all credit card accounts at these banks. The second data set contains portfolio-level information for each bank on items such as operational costs and fraud expenses across all credit cards managed by the bank. Both data sets are submitted monthly; reporting started in January 2008 and continues through the present. In the average month, we observe account-level information on over 400 million credit cards. See Agarwal et al. (2015) for more details on these data and summary statistics on the full sample.

In addition, we merge quarterly credit bureau data to the CCM data using a unique identifier. These credit bureau data contain information on an individual's credit cards across all lenders, including information on the total number of credit cards, total credit limits, total borrowing volume, length of credit history, and credit performance measures such as whether the borrower was ever more than 90 days past due on an account. This information captures the near totality of the information on new credit card applicants that was available to lenders at account origination.

areas. This suggests that the potential for increased profit from more complicated pricing rules is likely to be second-order.

### 1.3 Identifying Credit Limit Discontinuities

In our empirical analysis we focus on credit cards that were originated during our sample period. Our data do not contain information on the credit supply function used by banks when the credit card was originated. Therefore, the first empirical step involves backing out this credit supply function from the observed credit limits offered to individuals with different FICO scores. To do this, we jointly consider all credit cards of the same type (co-branded, oil and gas, affinity, student, or other), issued by the same bank, in the same month, through the same loan channel (pre-approved, invitation to apply, take-one branch application, take-one magazine and internet application, or other). It is plausible that the same credit supply function was applied to each card within such an "origination group." For each of the more than 10,000 resulting origination groups between January 2008 and November 2013, we plot the average credit limit as a function of the FICO score.[10]

Panels A to D of Figure 2 show examples of such plots. Since banks generally adjust credit limits at FICO cutoffs that are multiples of 5 (e.g., 650, 655, 660), we pool accounts into such buckets. Average credit limits are shown with blue lines; the number of accounts originated are shown with grey bars. Panels A and B show examples where there are no discontinuous jumps in the credit supply function. Panels C and D show examples of clear discontinuities. For instance, in Panel C, a borrower with a FICO score of 714 is offered an average credit limit of approximately $2,900 while a borrower with a FICO score of 715 is offered an average credit limit of approximately $5,600.

While continuous credit supply functions are significantly more common, we detect a total of 812 credit limit discontinuities between January 2008 and November 2013. We refer to these cutoffs as "credit limit quasi-experiments" and define them by the combination of origination group $\times$ FICO score. Panel E of Figure 2 shows the distribution of FICO scores at which we observe these quasi-experiments. They range from 620 to 785, with 660, 700, 720, 740, and 760 being the most common cutoffs. Panel F shows the distribution of quasi-experiments weighted by the number of accounts originated within 50 FICO points of the cutoffs, which is the sample we use for our regression discontinuity analysis. We observe more than 1 million accounts around the most prominent cuttoffs. Our experimental sample has 14.2 million total accounts or about 18,000 per quasi-experiment.

[10]Since our data end in December 2014, we only consider credit cards originated until November 2013 to ensure that we observe at least 12 months of post-origination data.

### 1.4 Summary Statistics

Table 1 presents summary statistics for the accounts in our experimental sample at the time the account was originated. In particular, to characterize the accounts that identify our effects, we calculate the mean value for a given variable across all accounts within 5 FICO score points of the cutoff for each quasi-experiment. We then show the means and standard deviations of these values across the 812 quasi-experiments in our data. We also show summary statistics within each of the 4 FICO score groups that we use to explore heterogeneity in the data: $\leq 660$, 661-700, 701-740, and $> 740$. These ranges were chosen to split our quasi-experiments into roughly equal groups. In the entire sample, 28% of credit cards were issued to borrowers with FICO scores below 660, 16% and 19% were issued to borrowers with FICO scores between 661-700 and 701-740, respectively, and 37% of credit cards were issued to borrowers with FICO scores above 740 (see Appendix Figure A1).

At origination, accounts at the average quasi-experiment have a credit limit of $5,182 and an annual percentage rate (APR) of 15.7%. Credit limits increase from $2,490 to $6,885 across FICO score groups, while APRs decline from 20.6% to 14.9%. In the merged credit bureau data, we observe utilization on all credit cards held by the borrower. At the average quasi-experiment, account holders have 10.8 credit cards, with the oldest account being more than 15 years old. Across these credit cards, account holders have $9,447 in average daily balances (ADB) and $32,574 in credit limits.[11] Aggregate ADB is hump-shaped in FICO score, while total credit limits are monotonically increasing. In the credit bureau data, we also observe historical delinquencies and default. At the average quasi-experiment, account holders have been more than 90 days past due (90+ DPD) 0.18 times in the last 24 months. This number declines from 0.92 to 0.13 across the FICO score groups.

## 2 Research Design

Our identification strategy exploits the credit limit quasi-experiments identified in Section 1 using a fuzzy regression discontinuity (RD) research design (see Lee and Lemieux, 2010). In our setting, the "running variable" is the FICO score. The treatment effect of a $1 change in credit limit is determined by the jump in the outcome variable divided by the jump in the credit limit at the discontinuity.

We first describe how we recover the treatment effect for a given quasi-experiment and then

[11] ADB are defined as the arithmetic mean of end-of-day balances over the billing cycle. This is the borrowing volume on which credit card borrowers pay interest. If borrowers do not carry over balances from the previous month, and repay end-of-month balances within a grace period, they are not charged interest for that month. See Agarwal et al. (2015).

discuss how we aggregate across the 812 quasi-experiments in the data. For a given quasi-experiment, let $x$ denote the FICO score, $\overline{x}$ the cutoff FICO level, $CL$ the credit limit, and $O$ the outcome variable of interest (e.g., borrowing volume). The fuzzy RD estimator, a local Wald estimator, is given by:

$$\tau = \frac{\lim_{x \downarrow \overline{x}} E[O|x] - \lim_{x \uparrow \overline{x}} E[O|x]}{\lim_{x \downarrow \overline{x}} E[CL|x] - \lim_{x \uparrow \overline{x}} E[CL|x]}. \tag{1}$$

The denominator is always non-zero because of the known discontinuity in the credit supply function at $\overline{x}$. The parameter $\tau$ identifies the local average treatment effect of extending more credit to people with FICO scores in the vicinity of $\overline{x}$.

We follow Hahn, Todd and Van der Klaauw (2001) and estimate the limits in Equation 1 using local polynomial regressions. Let $i$ denote a credit card account and $\mathbb{I}$ the set of accounts within 50 FICO score points on either side of $\overline{x}$. For each quasi-experiment, we fit a local second-order polynomial regression that solves the following objective function separately for observations $i$ on either side of the cutoff, $D \in \{L, H\}$. We do this for two different variables, $y_i \in \{CL, O\}$.

$$\min_{\alpha_{y,D}, \beta_{y,D}, \gamma_{y,D}} \sum_{i \in \mathbb{I}} \left[y_i - \alpha_{y,D} - \beta_{y,D}(x_i - \overline{x}) - \gamma_{y,D}(x_i - \overline{x})^2\right]^2 K\left(\frac{x_i - \overline{x}}{h}\right) \quad \text{for } D \in \{L, H\} \tag{2}$$

Observations further from the cutoff are weighted less, with the weights given by the kernel function $K\left(\frac{x_i - \overline{x}}{h}\right)$, which has bandwidth $h$. Since we are primarily interested in the value of $\alpha_{y,D}$, we choose the triangular kernel that has optimal boundary behavior.[12] In our baseline results we use the default bandwidth from Imbens and Kalyanaraman (2011). For those quasi-experiments for which we identify an additional jump in credit limits within $\mathbb{I}$, we include an indicator variable in Equation 2 that is equal to 1 for all FICO scores above this second cutoff. Given these estimates, the local average treatment effect is given by:

$$\tau = \frac{\hat{\alpha}_{\text{Outcome},H} - \hat{\alpha}_{\text{Outcome},L}}{\hat{\alpha}_{\text{Credit Limit},H} - \hat{\alpha}_{\text{Credit Limit},L}}. \tag{3}$$

## 2.1 Heterogeneity by FICO Score

Our objective is to estimate the heterogeneity in treatment effects by FICO score (see Einav et al., 2015, for a discussion of estimating treatment effect heterogeneity across experiments). Let $j$ indicate quasi-experiments, and let $\tau_j$ be the local average treatment effect for quasi-experiment $j$ estimated using

[12] Our results are robust to using different specifications. For example, we obtain similar estimates when we run a locally linear regression with a rectangular kernel, which is equivalent to running a linear regression on a small area around $\overline{x}$.

Equation 3. Let $FICO_k$, $k = 1, \ldots, 4$ be an indicator variable that takes on a value of 1 when the FICO score of the discontinuity for quasi-experiment $j$ falls into one of our FICO groups ($\leq$ 660, 661-700, 701-740, $>$ 740). We recover heterogeneity in treatment effects by regressing $\tau_j$ on the FICO group dummies and controls:

$$\tau_j = \sum_{k \in K} \delta_k FICO_k + X_j' \delta_X + \epsilon_j. \tag{4}$$

In our baseline specification, the $X_j$ are fully interacted controls for origination quarter, bank, and a "zero initial APR" dummy that captures whether the account has a promotional period during which no interest is charged; we also include loan channel fixed effects.[13] The $\beta_k$ are the coefficients of interest and capture the mean effect for accounts in FICO group $k$, conditional on the other covariates.

We construct standard errors by bootstrapping over the 812 quasi-experiments. In particular, we draw 500 samples of treatment effects with replacement and estimate the coefficients of interest $\beta_k$ in each sample. Our standard errors are the standard deviations of these estimates. Conceptually, we think of the local average treatment effects $\tau_j$ as "data" that are drawn from a population distribution of treatment effects. We are interested in the average treatment effect in the population for a given FICO score group. Our bootstrapped standard errors can be interpreted as measuring the precision of our sample average treatment effects for the population averages.

# 3 Validity of Research Design

The validity of our research design rests on two assumptions: First, we require a discontinuous change in credit limits at the FICO score cutoffs. Second, other factors that could affect outcomes must trend smoothly through these thresholds. Below we present evidence in support of these assumptions.

## 3.1 First Stage Effect on Credit Limits

We first verify that there is a discontinuous change in credit limits at our quasi-experiments. Panel A of Figure 3 shows average credit limits at origination within 50 FICO score points of the quasi-experiments together with a local linear regression line estimated separately on each side of the cutoff. Initial credit limits are smoothly increasing except at the FICO score cutoff, where they jump discontinuously by $1,429. The magnitude of this increase is significant relative to an average credit limit of

[13] To deal with outliers in the estimated treatment effects from Equation 3, we winsorize the values of $\tau_j$ at the 2.5% level.

$5,182 around the cutoff (see Table 3). Panel A of Figure 4 shows the distribution of first stage effects from RD specifications estimated separately for each of the 812 quasi-experiments in our data. These correspond to the denominator of Equation 3. The first stage estimates are fairly similar in size, with an interquartile range of $677 to $1,755, and a standard deviation of $795.[14]

Panel B of Figure 4 examines the persistence of the jump in the initial credit limit. It shows the RD estimate of the effect of a $1 increase in initial credit limits on subsequent credit limits at different time horizons following account origination. The initial effect is highly persistent and very similar across FICO score groups, with a $1 higher initial credit limit raising subsequent credit limits by $0.78 to $0.86 at 36 months after origination. Table 4 shows the corresponding regression estimates.

In the analysis that follows, we estimate the effect of a change in *initial* credit limits on outcomes at different time horizons. A natural question is whether it would be preferable to scale our estimates by the change in contemporaneous credit limits instead of the initial increase. We think the initial increase in credit limits is the appropriate denominator because subsequent credit limits are endogenously determined by household responses to the initial increase. We discuss this issue further in Section 5.4.

### 3.2 Other Characteristics Trend Smoothly Through Cutoffs

For our research design to be valid, the second requirement is that all other factors that could affect the outcomes of interest trend smoothly through the FICO score cutoff. This includes contract terms, such as the interest rate (Assumption 1), characteristics of borrowers (Assumption 2), and the density of new accounts (Assumption 3). Because we have 812 quasi-experiments, graphically assessing the individual validity of our identifying assumptions for each experiment is not practical. Therefore, we show results graphically that pool across all of the quasi-experiments in the data, estimating a single pooled treatment effect and pooled local polynomial. In Table 3 we present summary statistics on the distribution of these treatment effects across the 812 individual quasi-experiments.

*Assumption 1:* **Credit limits are the only contract characteristic that changes at the cutoff.**

The interpretation of our results requires that credit limits are the only contract characteristic that changes discontinuously at the FICO score cutoffs. For example, if the cost of credit also changed at our credit limit quasi-experiments, an increase in borrowing around the cutoff might not only result

[14] For all RD graphs we focus on the set of quasi-experiments without a second quasi-experiment within 50 FICO score points. Tables always include information from all 812 quasi-experiments, where we control for additional discontinuous jumps in credit limits as discussed in Section 2.

from additional access to credit at constant cost, but could also be explained by lower borrowing costs.

Panel C of Figure 3 shows the average APR around our quasi-experiments. APR is defined as the initial interest rate for accounts with a positive interest rate at origination, and the "go to" rate for accounts which have a zero introductory APR.[15] As one would expect, the APR is declining in the FICO score. Importantly, there is no discontinuous change in the APR around our credit limit quasi-experiments.[16] Table 3 shows that, for the average (median) experiment, the APR declines by 4 basis points (1 basis point) at the FICO cutoff; these changes are economically tiny relative to an average APR of 15.7%. Panel E of Figure 3 shows that the length of the zero introductory APR period for the 265 experiments that are for accounts in origination groups with a zero introductory APR. The length of the introductory period is increasing in FICO score but there is no jump at the credit limit cutoff.

A related concern is that while contract characteristics other than credit limits are not changing at the cutoff for the bank with the credit limit quasi-experiment, they might be changing at other banks. If this were the case, the same borrower might also be experiencing discontinuous changes in contract terms on his other credit cards, which would complicate the interpretation of our estimates. To test whether this is the case, for every FICO score where we observe at least one bank discontinuously changing the credit limit for one card, we define a "placebo experiment" as all other cards that are originated around the same FICO score at banks without an identified credit limit quasi-experiment. The right column of Figure 3 shows average contract characteristics at all placebo experiments. All characteristics trend smoothly through the FICO score cutoff at banks with no quasi-experiments.

*Assumption 2:* **All other borrower characteristics trend smoothly through the cutoff.**

We next examine whether borrowers on either side of the cutoff are similar on observable characteristics. Differences in these characteristics might either arise because banks differentially select on those characteristics in their decision to offer credit (e.g., applicants with low FICO scores are only offered credit if they have never defaulted) or because borrowers select around the threshold in their decision to take up a credit card offer (e.g., borrowers who have a lower aggregate credit limit might be more likely to take up a credit card offer with higher credit limits).

Panels A to E of Figure 5 show important borrower characteristics around the FICO score quasi-

[15]The results look identical when we remove experiments for accounts with an initial APR of zero.

[16]We initially identified a few instances where APR also changed discontinuously at the same cutoff as we detected a discontinuous change in credit limits. These quasi-experiments were dropped in our process of arriving at the sample of 812 quasi-experiments that are the focus of our empirical analysis.

experiments. These characteristics were obtained from the credit bureau data at the time the credit card was originated. Panels A and B show the total number of credit cards and the total credit limit on those credit cards, respectively. Both are increasing in FICO score, and there is no discontinuity around the cutoff. Panel C shows the age of the oldest credit card account for consumers, capturing the length of the observed credit history. We also plot the number of payments from this consumer that were 90 or more days past due (DPD), both over the entire credit history of the borrower (Panel D), as well as in the 24 months prior to origination (Panel E). There are no discontinuous changes around the cutoff in any of these (and other unreported) measures of borrower characteristics. Table 3 shows the distribution of discontinuous changes in these characteristics across experiments, confirming our conclusion that they trend smoothly through the FICO score cutoff.

*Assumption 3:* **The number of originated accounts trends smoothly through the cutoff.**

Lastly, we assess whether the number of originated accounts trends smoothly through the FICO score cutoff. This addresses two potential concerns with the validity of our research design.

First, RDs are invalid if individuals can precisely manipulate the forcing variable. In our setting, manipulation of the FICO score is unlikely to be a concern. Even if consumers had some control over their FICO score, Lee (2008) shows that as long as the individuals do not have precise control, the treatment is "as good as random" just around the cutoff. Since the FICO scoring algorithm is proprietary, precise manipulation of the FICO score is not possible. More importantly, the banks' credit supply functions are unknown. This means that an individual with a FICO score just below a threshold is unaware that increasing the FICO score marginally would lead to a significant increase in the credit limit. This makes it unlikely that that assumption of "no precise manipulation" is violated.

An additional concern in our setting is that banks might use the FICO score cutoff to make extensive margin lending decisions. For example, if banks relaxed some other constraint once individuals crossed a FICO score threshold, more accounts would be originated for households with higher FICO scores, but households on either side of the FICO score cutoff would differ along that other dimension. While we did not observe any changes in observable characteristics around the FICO score cutoffs, if banks selected borrowers based on characteristics unobservable to the econometrician, this would invalidate our research design. This does not appear to be the case: Panel F of Figure 5 shows that the number of accounts trends smoothly through our credit score cutoff.

# 4 Borrowing and Spending

Having established the validity of our research design, we turn to estimating the causal impact of an increase in credit limits on borrowing and spending, focusing on how these effects vary across the FICO score distribution.

## 4.1 Average Borrowing and Spending

We start by presenting basic summary statistics on credit card utilization. The left column of Table 2 shows average borrowing and spending by FICO score group at different time horizons after account origination. To characterize the credit cards that identify the causal estimates, we restrict the sample to accounts within 5 FICO score points of a credit limit quasi-experiment.

Average daily balances (ADB) on the "treated" credit card are hump-shaped in FICO score. At 12 months after origination, ADB increase from $1,171 for the lowest FICO score group ($\leq$ 660), to more than $2,100 for the middle FICO score groups, before falling to $1,970 for the highest FICO score group ($>$ 740). ADB are fairly flat over time for the lowest FICO score group but drop sharply for accounts with higher FICO scores. Because the number of credit cards is increasing in FICO score, ADB *across all credit cards* are increasing in FICO score. Since borrowing on the treated cards is a modest share of total borrowing, ADB across all cards do not vary with the time since the treated card was originated.

Despite large differences in credit limits by FICO score, purchase volume over the first 12 months since origination is fairly similar, ranging from $2,437 to $2,807 across FICO score groups. Higher FICO score borrowers spend somewhat more on their cards over longer time horizons, but even at 60 months after origination, cumulative purchase volume ranges between $4,610 and $5,477 across FICO score groups.

## 4.2 Marginal Propensity to Borrow (MPB)

We next exploit our credit limit quasi-experiments to estimate the marginal propensity to borrow out of an increase in credit limits. The top row of Figure 6 shows the effect of a quasi-exogenous increase in credit limits on ADB on the "treated" credit card. Panel A shows the effect on ADB at 12 months after account origination in the pooled sample of all quasi-experiments. ADB increase sharply at the discontinuity but otherwise trend smoothly in FICO score.

Panel B decomposes this effect, showing the impact of a $1 increase in credit limits on ADB at different time horizons after account origination and for different FICO score groups. Panel A of Table

5 shows the corresponding RD estimates. We find that higher credit limits generate a sharp increase in ADB on the treated credit card for all FICO score groups. Within 12 months, the lowest FICO score group raises ADB by 55 cents for each additional dollar in credit limits. The effects of increases in credit limits on ADB are decreasing in FICO score, but even borrowers in the highest FICO score group increase their ADB on the treated card after 12 months by approximately 22 cents for each extra dollar in credit limits. For the lowest FICO score group, the increase in ADB is quite persistent, declining by less than 20% between the first and fourth year. This is consistent with these low FICO score borrowers using the increase in credit to fund immediate spending and then "revolving" their debt in future periods. For the higher FICO score groups, the MPB drops more rapidly over time. This time-series pattern for MPB mirrors the pattern of average ADB documented above.

The middle row of Figure 6 examines the effects on borrowing across all credit cards held by the consumer, using the merged credit bureau data. The reason to look at this broader measure of borrowing is to account for balance shifting across cards. For example, a consumer who receives a higher credit limit on a new credit card might shift borrowing to this card to take advantage of a low introductory interest rate. This would result in an increase in borrowing on the treated card but no increase in overall borrowing. The response of total borrowing across all credit cards is the primary object of interest for policymakers wanting to stimulate household borrowing and spending .

Panel C of Figure 6 shows the effect on total borrowing across all credit cards at 12 months after account origination pooled across all quasi-experiments. Panel D shows the RD estimates of the effect of a $1 increase in credit limits on total borrowing across all cards for different time horizons and FICO score groups. Panel B of Table 5 shows the RD estimates that correspond to this plot. For all but the highest FICO score group, the marginal increase in borrowing on the treated card corresponds to an increase in overall borrowing. Indeed, we cannot reject the null hypothesis that effects across all accounts for these FICO groups are identical to the treated card effects. The one exception is the group with the highest FICO scores for which we find evidence of significant balance shifting. At one year after origination, these consumers exhibit a 22% MPB on the treated card but an essentially zero MPB across all their accounts (the statistically insignificant point estimate is -3%). Importantly, Table 2 showed that high FICO score borrowers have sizable average ADB, both on the treated credit card and across all credit cards. This suggests that while these households routinely use credit cards to borrow, they are not constrained at the margin, and thus do not respond to credit expansions.

The increase in borrowing on both the treated card and across all credit cards is suggestive that higher credit limits raise overall spending. However, at least in the short run, consumers could increase their borrowing volumes by paying off their debt at a slower rate without spending more. To examine whether the increase in borrowing is indeed due to higher spending rather than slower debt repayment, the bottom row of Figure 6 estimates the effect of higher credit limits on cumulative purchase volume on the treated card. Panel C of Table 5 shows the corresponding estimates. Over the first year, the higher borrowing levels on the treated card are almost perfectly explained by increased purchase volume. For the lowest FICO score group, a $1 increase in credit limits raises cumulative purchase volume over the first year by 54 cents, ADB on the treated card by 55 cents, and ADB across all cards by 58 cents. For the highest FICO score group, the increase in cumulative purchase volume is 22 cents, which is also identical to the increase in treated card ADB.

Over longer time horizons, the cumulative increase in purchase volume outstrips the rise in ADB. This is consistent with larger effects on overall spending than borrowing. Since we do not have information on purchase volume across all credit cards or cash spending, we cannot rule out the possibility that excess purchase volumes over longer time horizons result from shifts in the source of payment.

Overall, the quasi-experimental variation in credit limits provides evidence of a large average MPB and substantial heterogeneity across FICO score groups. For the lowest FICO group ($\leq 660$), we find that a $1 increase in credit limits raises total borrowing by 58 cents at 12 months after origination. This effect is explained by more spending rather than less pay-down of debt. For the highest FICO group ($> 740$), we estimate a 22% effect on the treated credit card that is entirely explained by balance shifting, with a $1 increase in credit limits having no effect on total borrowing. While these estimates are not representative of the entire population, they correspond to the set of applicants for new credit cards. This is the population most likely to respond to credit expansions, and thus of particular interest for bank-mediated stimulus.

Our findings suggest that the effects of bank-mediated stimulus on borrowing and spending will depend on whether credit expansions reach those low FICO score borrowers with large MPBs. On the other hand, extending extra credit to low FICO score households who are more likely to default might well conflict with other policy objectives, such as the de-risking of bank balance sheets. By showing which households are responsive to credit expansions, our estimates thus provide an important input into understanding both the effectiveness as well as the desirability of bank-mediated stimulus.

## 5 A Model of Optimal Credit Limits

In this section we present a model of optimal credit limits. We use this model to examine the effect of a change in the cost of funds on credit limits. We also examine how primitives such the degree of asymmetric information create heterogeneity in this effect. In Section 6 we estimate the key parameters of this model, allowing us to characterize banks' marginal propensity to lend (MPL) to borrowers with different FICO scores.

We view this approach of estimating the MPL as superior to looking directly at the correlation between banks' credit supply decisions and their cost of funds. This alternative approach is problematic because large changes in banks' cost of funds generally occur at the same time as changes in economic activity that also affect banks' willingness to lend. For example, during our sample period we observe one large drop of the federal funds rate, between August 2008 and November 2008. However, this was also a period of extraordinary economic distress, and the associated increase in the riskiness of consumer lending also affected banks' optimal credit limits.

### 5.1 Credit Limits as the Margin of Adjustment

In principle, banks could respond to a decline in the cost of funds by adjusting any number of contract terms, including credit limits, interest rates, rewards, and different fees. We follow the empirical literature on contract pricing in credit markets and restrict our analysis to a single dimension of adjustment (see Einav, Jenkins and Levin, 2012). In choosing to focus on credit limits, and not interest rates, we build on a large prior literature, starting with Ausubel (1991), that documents the stickiness of credit card interest rates to changes in the cost of funds. This stickiness of credit card interest rates is robust to our time period (see Appendix Figure A2).[17]

Many explanations for this stickiness of credit card interest rates have been proposed, including limited interest rate sensitivity by borrowers, collusion by credit card lenders, and an adverse selection story whereby lower interest rates disproportionately attract borrowers with higher default rates (Ausubel, 1991; Calem and Mester, 1995; Stavins, 1996; Stango, 2000). While we think determining why interest rates are sticky is an interesting question, we take this feature of the market as given in

[17] Additional evidence in support of this choice comes from Agarwal et al. (2015), who find that the reduction in fee revenue brought about by the 2009 CARD Act was not offset with higher interest rates. In Agarwal et al. (2015)'s model, when fees are non-salient, a decline in fee revenue affects the bank's first order conditions in the exact same way as an increase in marginal costs. As a result, firms offset a decline in fee revenue through increasing interest rates by the same amount as they would pass through an increase in the cost of funds. Agarwal et al. (2014) provide a detailed discussion.

this study. Importantly, while we focus on the pass-through to credit limits, our conceptual point is likely relevant for any pass-through to interest rates that might occur. In particular, if lower interest rates attract worse borrowers or encourage more borrowing, then pass-through of declines in the cost of funds to lower interest rates will also be limited.

### 5.2 Model Setup

Consider a one-period lending problem in which a bank chooses a credit limit $CL$ for a group of observably identical borrowers, such as all consumers with the same FICO score, to maximize profits. Let $q(CL)$ describe how the quantity of borrowing depends on the credit limit, and let $MPB = q'(CL)$ indicate the consumers' marginal propensity to borrow out of a credit expansion.

Banks earn revenue from interest charges and fees. Let $r$ denote the interest rate, which is fixed and determined outside of the model. Let $F(CL) \equiv F(q(CL), CL)$ denote fee revenue, which can depend on credit limits directly and indirectly through the effect of credit limits on borrowing.

The main costs for the bank are the cost of funds and chargeoffs. The bank's cost of funds, $c$, can be thought of as a refinancing cost but more broadly captures anything that affects the banks' cost of lending, including capital requirements and financial frictions. Let $C(CL) \equiv C(q(CL), CL)$ denote chargeoffs, which can also depend directly and indirectly on credit limits.[18]

The objective for the bank is to choose a credit limit to maximize profits.[19]

$$\max_{CL} q(CL)(r - c) + F(CL) - C(CL). \tag{5}$$

The optimal credit limit sets marginal profits to zero, or, equivalently, sets marginal revenue equal to marginal cost:

$$\underbrace{q'(CL)r + F'(CL)}_{=MR(CL)} = \underbrace{q'(CL)c + C'(CL)}_{=MC(CL)}. \tag{6}$$

We assume that marginal revenue crosses marginal cost "from above," i.e., $MR(0) > MC(0)$ and $MR'(CL) < MC'(CL)$, so we are guaranteed to have an interior optimal credit limit.

[18] "Chargeoffs" refer to an expense incurred on the lender's income statement when a debt is considered long enough past due to be deemed uncollectible. For an open-ended account such as a credit card, regulatory rules usually require a lender to charge off balances after 180 days of delinquency.

[19] The model abstracts from the extensive margin decision of whether or not to offer a credit card. To capture this margin, the model could be extended to include a fixed cost of originating and maintaining a credit card account. In such a model, borrowers would only receive a credit card if expected profits exceeded this fixed cost.

We are interested in the effect on borrowing of a decrease in the cost of funds, which is given by the total derivative $-\frac{dq}{dc}$. This can be decomposed into the product of the marginal propensity to lend (MPL) and the marginal propensity to borrow (MPB):

$$-\frac{dq}{dc} = \underbrace{-\frac{dCL}{dc}}_{=\text{MPL}} \times \underbrace{\frac{dq}{dCL}}_{=\text{MPB}} \tag{7}$$

In Section 4, we estimated the MPB directly using the quasi-experimental variation in credit limits. We next discuss how we use our variation to estimate the MPL.

### 5.3 Pass-Through of a Decrease in the Cost of Funds

A decrease in the cost of funds reduces the marginal cost of extending each unit of credit and can be thought of as a downward shift in the marginal cost curve. Since equilibrium credit limits are determined by the intersection of marginal revenue and marginal costs (see Equation 6), the slopes of marginal revenue and marginal costs determine the resulting change in equilibrium credit limits. To see this, consider Figure 1 from the introduction. In Panel A, where marginal costs and marginal revenue are relatively flat, a given downward shift in the marginal cost curve leads to a large increase in equilibrium credit limits. In Panel B, where marginal costs and marginal revenue are relatively steep, the same downward shift in the marginal cost curve leads to a smaller increase in credit limits.

Mathematically, the effect on credit limits of a decrease in the cost of funds can be derived by applying the implicit function theorem to the first order conditions shown in Equation 6:

$$MPL = -\frac{dCL}{dc} = -\frac{q'(CL)}{MR'(CL) - MC'(CL)} \tag{8}$$

The numerator is the marginal propensity to borrow ($q'(CL) \equiv MPB$) and scales the size of the effect because a given decrease in the per-unit cost of funds induces a larger shift in marginal costs when credit card holders borrow more on the margin. The denominator is the slope of marginal profits $MP'(CL) = MR'(CL) - MC'(CL)$. The existence assumption, $MR'(CL) < MC'(CL)$, guarantees the denominator is negative and thus implies positive pass-through, $MPL > 0$. The MPL is decreasing as the downward sloping marginal profits curve becomes steeper. Economically, we view the MPB and the slope of marginal profits as "sufficient statistics" that capture the effect on pass-through of a number underlying features of the credit card market without requiring strong assumptions on the underlying model of consumer behavior (see, Chetty, 2009, for more on this approach).[20]

Perhaps the most important of these features is asymmetric information, which includes both adverse selection and moral hazard. Since banks can adjust credit limits based on observable borrower characteristics, they determine the optimal credit limit separately for each group of observably identical borrowers. By selection we therefore mean selection on information that the lender cannot observe or is prohibited from using by law. With adverse selection, higher credit limits disproportionally raise borrowing among households with a greater probability of default, increasing the marginal cost of extending more credit. This could occur because forward-looking households, who anticipate defaulting in the future, strategically increase their borrowing. Alternatively, it could occur because there are some households that are always more credit constrained, and these households borrowers more today and have a higher probability of default in the future. Regardless of the channel, adverse selection translates into a more positively sloped marginal cost curve, a more negatively sloped marginal profit curve, and less pass-through.[21]

Higher credit limits could also affect marginal costs holding the composition of marginal borrowers fixed. For instance, in Fay, Hurst and White's (2002) model of consumer bankruptcy, the benefits of filing for bankruptcy are increasing in the amount of debt while the costs of filing are fixed. The implication is that higher credit limits, which raise debt levels, lead to higher default probabilities, a more positively sloped marginal cost curve, and a lower rate of pass-through. This mechanism is sometimes called moral hazard because the borrower does not fully internalize the cost of their decisions when choosing how much to borrow and whether to default. However, a positive effect of credit limits on borrowing does not require strategic behavior on the part of the borrower. For example, myopic consumers might borrow heavily out of an increase in credit limits, not because they anticipate defaulting next period, but because they down-weight the future.[22]

The slope of marginal revenue is equally significant in determining the MPL, and revenue from fees (e.g., annual fees, late fees) is a key determinant of the slope of marginal revenue. In particular, fee revenue does not scale one-for-one with credit card utilization. On the margin, an increase in

[20] See Einav, Finkelstein and Cullen (2010) and Mahoney and Weyl (2013) for a more in-depth discussion of how the slope of marginal costs parameterizes the degree of asymmetric information in a market.

[21] In principle, selection could also be advantageous, with higher credit limits disproportionally raising borrowing among households with a lower default probability. In this case, more advantageous selection would translate into a less negatively sloped marginal profit curve, and more pass-through.

[22] If greater debt levels reduce the rate of default – e.g., because increased credit access allows households to "ride out" temporary negative shocks without needing to default – an increase in credit limits would result in lower default probabilities, a less negatively sloped marginal profit curve, and more pass-through.

credit limits might increase fee revenue (e.g., by raising the probability a consumer renews her card and pays next year's annual fee) but not by a large amount. A decline in marginal fee revenue would translate into a more negatively sloped marginal revenue curve, a more negatively sloped marginal profits curve, and less pass-through.

In Section 6, we directly estimate heterogeneity in the slope of marginal costs, marginal revenue, and marginal profits by FICO score. This approach allows us to quantify the joint effect of a broad set of factors such as moral hazard and adverse selection on pass-through without requiring us to untangle their relative importance.

### 5.4 Empirical Implementation

Taking the model to the data involves two additional steps. First, our model of optimal credit limits has one period, while our data are longitudinal with monthly outcomes for each account. To align the data with the model, we aggregate the monthly data for each outcome into discounted sums over various horizons. For instance, for each credit card in our data, we construct a discounted sum of post-origination fee revenue using a monthly discount factor of 0.996, which translates into an annual discount factor of 0.95.[23] With these aggregated data, the objective function for the bank is to set initial credit limits to maximize the discounted flow of profits, which is a one period problem.[24]

A second issue involves the potential divergence between expected and realized profits. While the model does not distinguish between these concepts, realized profits in our data may diverge from expected profits when banks set initial credit limits. We account for this potential divergence by modeling realized marginal profits as expected marginal profits plus a realization shock: $MP(CL) = \mathbb{E}[MP(CL)] + \epsilon_{MP}$, where the expectation is taken at the time of credit card origination. The realization shock $\epsilon_{MP}$ allows the model to rationalize the observed initial credit limits as optimal. Conceptually, the assumption is that any divergence between expectations and realizations can be captured by a level shift in the marginal profit curve, and that there is no divergence between the slope of expected marginal profits and the slope of realized marginal profits.[25]

[23] In 2009, the weighted average cost of capital for the banking sector was 5.86%; in 2010 it was 5.11%, and in 2011 it was 4.27% (`http://pages.stern.nyu.edu/~adamodar/`). Our results are not sensitive to the choice of discount factor.

[24] While initial credit limits are highly persistent (see Section 3.1), credit limits can be changed following origination, which affects the discounted sums. We assume that banks set initial credit limits in a dynamically optimal way, taking into account their ability to adjust credit limits in the future. The envelope theorem then allows us to consider the optimization problem facing a bank at card origination without specifying the dynamic process of credit limit adjustment.

[25] To the extent there is business cycle variation in the slope of $MP(CL)$, the slope of realized $MP(CL)$ during the Great Recession is a good approximation of the expected slope of this object during future macroeconomic crises, and therefore the relevant parameter for estimating the effect of future bank-mediated stimulatory policy.

# 6 Marginal Propensity to Lend

Section 5 highlighted the slope of marginal profits, defined as the slope of marginal revenue minus the slope of marginal costs, as a key factor determining the MPL. We next use the quasi-experimental variation in credit limits to estimate how these slopes vary across the FICO score distribution.

## 6.1 Average Utilization and Profitability

To provide context, we first present basic facts on the profitability of the credit cards in our sample. We define profits for a credit card account as the difference between revenue and costs.

Revenue is the sum of interest charge revenue, fee revenue, and interchange income. We observe interest charge revenue and fee revenue for each account in our data. Interchange fees are charged to merchants for processing credit card transactions and scale proportionally with spending. Following Agarwal et al. (2015), we calculate interchange income for each account as 2% of purchase volume.

Costs are the sum of chargeoffs, the cost of funds, rewards and fraud expenses, and operational costs such as costs for debt collection, marketing, and customer acquisition. Chargeoffs are reported at the account level.[26] We observe a bank's total cost of funds and total credit card lending in the monthly portfolio data and use this information to construct each bank's time-varying cost of funds. We construct the costs of funds for a given account by applying this percentage to the account-level ADB. We calculate that reward and fraud expenses are 1.4% of purchase volume and operational costs are 3.5% of ADB in the portfolio data. We construct account-level values by applying these percentages to account-level purchase volume and ADB. See Agarwal et al. (2015) for additional discussion.

The middle section of Table 2 shows cumulative costs and its components by FICO score group at different time horizons after account origination. As before, we restrict the sample to credit cards originated within 5 FICO score points of a credit limit quasi-experiment. Cumulative costs rise fairly linearly over time and are hump-shaped in FICO score. At 48 months after origination, cumulative costs are \$551 for the lowest FICO group ($\leq 660$), approximately \$800 for the middle groups, and \$449 for the highest FICO group ($> 740$). Cumulative chargeoffs generally account for more than half of these costs, although they are more important for low FICO score accounts and become relatively more important at longer time horizons. The cumulative cost of funds declines from 10% of total costs at 12 months after origination to approximately 5% at 60 months after origination.

[26] We use the term "chargeoffs" to indicate gross chargeoffs minus recoveries, which are both observed in our data.

The right section of Table 2 shows cumulative revenue and profits. Cumulative revenue, like cumulative costs, grows fairly linearly over time. However, while cumulative costs are hump-shaped in FICO score, cumulative revenue is decreasing. For instance, at 48 months after origination, cumulative revenue is more than \$900 for the two lowest FICO groups, \$849 for accounts in the second highest FICO group, and \$522 for accounts with the highest FICO scores. Excluding the first year, interest charges account for approximately two-thirds of cumulative revenue; fee revenue accounts for approximately one-quarter and is particularly important for the low FICO score group. Both interest charges and fees are somewhat less important for the highest FICO group. For these accounts, interchange income is relatively more important, accounting for approximately one-fifth of total revenue.

The data on revenue and costs combine to produce average profits that are U-shaped in FICO score. At 48 months, cumulative profits are \$363 for the lowest FICO score group, \$133 and \$57 for the middle two FICO groups, and \$73 for accounts with the highest FICO score. Cumulative profits within a FICO score group increase linearly over time.

### 6.2 Marginal Probability of Default

We next examine the causal effect of an increase in credit limits on the probability of default. Larger increases of the probability of default contribute to more upward sloping marginal costs. To see this, suppose total costs for lending to a borrower is given $C(CL) = d(CL)q(CL)$, where $d(CL)$ is a default indicator and $q(CL)$ the amount of borrowing. The first order approximation of the slope of marginal costs is $MC'(CL) \approx 2d'(CL)q'(CL) \equiv 2d'(CL)MPB(CL)$, which is the effect of credit limits on the probability of default scaled by the MPB.[27] Intuitively, an increase in the probability of default has a larger impact on costs when borrowing volumes – and therefore the amount that is defaulted upon – is higher.

Figure 7 shows that an increase in credit limits has a large effect on the probability of default for the lowest FICO score households. For the lowest FICO score group, a \$1,000 increase in credit limits raises the probability of serious delinquency (60+ DPD) within 4 years by 1.2 percentage points, on a base of 16.5%. The effect is half as large for accounts with an intermediate FICO score, and close to zero for accounts in the highest FICO score group. Table 7 shows the corresponding estimates.

While the effect on default is intuitive and straightforward to estimate, there are a number of reasons that it is not a sufficient static for the degree of pass-through. First, the effect on default needs

[27] Including second order effects, the term is $MC'(CL) = 2d'(CL)q'(CL) + d''(CL)q(CL) + d(CL)q''(CL)$

to be dollarized. Second, the effect does not capture the effect of selection. For instance, if borrowers with a high probability of default respond more to an increase in credit limits but default at the same rate, there is no effect on the probability of default but marginal costs are still upward sloping. Third, the formula ignores higher order terms that could strengthen or offset the first order effect. For these reasons, we next estimate the slope of marginal costs, which is more directly informative for the MPL.

### 6.3 Slope of Marginal Profits and Components

Section 5 showed that the effect on credit limits of a change in the cost of funds depends on how fast marginal profits fall when the bank increases credit limits – i.e., the MPL depends on the *slope* of marginal profits. We estimate this slope by assuming marginal profits are linear in credit limits so that the slope is pinned down by our estimates of the marginal and average values.[28] To see this, suppose that marginal profits are given by $MP(CL) = \alpha + \beta CL$. This implies that average profits are $AP(CL) = \frac{\int_{X=0}^{CL} \alpha + \beta X.dX}{CL} = \alpha + \frac{1}{2}\beta CL$ and that the slope of marginal profits is $\beta = \frac{2(MP(CL) - AP(CL))}{CL}$. We use the same approach to estimate the slope of marginal cost, marginal revenue, etc.

Table 6 shows the slope of marginal profits and its components by FICO score group. For each outcome, the left column shows the marginal effect of a \$1 increase in credit limits at prevailing equilibrium credit limits, and the right column shows the impact of a \$1,000 increase in credit limits on this marginal effect. The estimates are based on cumulative outcomes over a 4 year horizon, although we will show robustness of our conclusions to different time horizons.

Columns 1-4 of Table 6 consider the effect of increasing credit limits on marginal costs and marginal chargeoffs. The level of marginal cost and marginal chargeoffs at the prevailing credit limit is largest for the low FICO score borrowers: cumulative marginal chargeoffs over the first 4 years fall from 20.9 cents for each extra dollar of credit for the lowest FICO score borrowers ($\leq$ 660) to 3.4 cents for the highest FICO score borrowers ($>$ 740). Since lenders condition their loan terms on the FICO score, these level differences in equilibrium marginal costs across FICO score groups are not informative about the degree of asymmetric information or the MPL. However, the MPL is affected by how these marginal values change with the credit limit, i.e., by the slope of marginal costs and marginal chargeoffs. For the lowest FICO score group, a \$1,000 increase in credit limits raises marginal costs by 7.3 cents relative to a baseline marginal effect of 28.3 cents at prevailing credit limits. The upward slope

[28] This approach is commonly used in the empirical literature on adverse selection in health insurance. See Einav, Finkelstein and Cullen (2010) and Cabral, Geruso and Mahoney (2014) for recent examples.

is driven by higher marginal chargeoffs, which rise by 7.8 cents for a $1,000 increase in credit limits. For the higher FICO score groups, a $1,000 increase in credit limits has little effect on marginal costs, raising them by less than 1.5 cents. The top row of Figure 8 displays these effects graphically.

The steeper slope of the marginal cost curve for low FICO score borrowers reduces the pass-through by banks of a decline in the cost of funds to these borrowers. As discussed in Section 5.3, this steeper slope of marginal costs is evidence of significant asymmetric information among those low FICO score borrowers, either because higher credit limits increase borrowing particularly for high-risk individuals, or because higher credit limits, through their effect on borrowing volumes, have a causal effect on the probability of default. While the MPL only depends on the joint effect of these mechanisms for different FICO score groups, which is captured by the slope of marginal costs, we next look directly at the marginal effect of extra credit on the probability of default. Figure 7 shows that an increase in credit limits has the largest effect on the probability of default for the lowest FICO score households. Table 7 shows the corresponding estimates. For the lowest FICO score group, a $1,000 increase in credit limits raises the probability of serious delinquency (60+ DPD) within 4 years by 1.2 percentage points on a base of 16.5%. The effect is half as large for accounts with an intermediate FICO score and close to zero for accounts in the highest FICO score group. This suggests that higher moral hazard contributes to the steeper slope of marginal costs for low FICO score borrowers.

Columns 5-8 of Table 6 examine the effect of increasing credit limits on marginal revenue and marginal fee revenue. For the lowest FICO score group, a $1,000 increase in credit limits reduces marginal revenue by 12.5 cents relative to a baseline marginal effect of 23.2 cents at prevailing credit limits. The majority of this decline is due to a drop in cumulative marginal fee revenue.[29] The slope of marginal revenue is less negative for higher FICO score accounts, with a $1,000 increase in credit limits reducing marginal revenue by 4.3 cents for the accounts in the second lowest FICO group and by virtually nothing for accounts with higher FICO scores. The steeper slope of marginal revenue at low FICO scores is consistent with Table 2, which shows that fee revenue is more important for accounts with low FICO scores. Since fee revenue does not scale with credit limits, a natural implication is that marginal fee revenue declines more for low FICO score accounts, where it is more important on average. The middle row of Figure 8 displays these effects graphically.

Columns 9-10 of Table 6 bring these results together into an analysis of cumulative marginal prof-

[29]Marginal fee revenue can, in principle, be negative. For instance, a higher credit limit that reduces the frequency of over-limit fees is modeled as a negative marginal fee revenue in our framework.

its at 48 months since account origination.[30] The level of cumulative marginal profits at prevailing credit limits is virtually zero for the lowest and highest FICO groups (0.3 cents and -0.6 cents, respectively) and slightly negative for the middle FICO groups (-3.5 cents and -2.7 cents, respectively). The U-shaped marginal profitability matches the U-shape for average profitability shown in Table 2.

The weakly negative marginal profits during our sample imply that banks were not forgoing lending opportunities in the credit card market that were *ex post* profitable. More broadly, this finding provides support for the "no good risks" explanation for limited credit supply during the Great Recession and pushes against the argument that financial frictions prevented banks from exploiting otherwise profitable consumer lending opportunities.[31] This is consistent with claims by James Chessen, the chief economist of the American Banker's Association, who explained reduced lending volumes by arguing that, "it's a very risky time for any lender because the probability of loss is greater, and they are being prudent in their approach to lending." (Wall Street Journal, 2009).

Column 10 of Table 6 shows that marginal profits are declining in credit limits, with the sharpest drop for accounts with the lowest FICO scores. For the lowest FICO score group, a \$1,000 increase in credit limits reduces marginal profits by 12.2 cents, driven by both lower marginal revenue and higher marginal costs. The slope of marginal profits is becoming less steep in FICO score. In response to a \$1,000 increase in credit limits, marginal profits decline by 4.4 and 2.3 cents for the middle FICO groups, and by 0.3 cents for the group with the highest FICO scores. The bottom row of Figure 8 shows these effects graphically.

### 6.4 Marginal Propensity to Lend (MPL)

The final step in our analysis is to use the estimates above to calculate the MPL in response to a decline in the cost of funds, which is given by the negative ratio of the MBP and the slope of marginal profits: $MPL = -\frac{MPB}{MP'(CL)}$ (see Section 5).

Figure 9 shows the effect on credit limits of a permanent 1 percentage point decrease in the cost of funds by FICO score group. For each FICO score group we show estimates using data on cumulative profits and ADB over time horizons of 12, 24, 36, 48, and 60 months after origination. Using cumu-

[30] We estimate the effect on marginal profits directly rather than constructing it as the difference between marginal revenue and marginal cost. Estimating this effect directly maximizes statistical power but means that the effects do not aggregate perfectly, i.e., our point estimates for the slopes of marginal revenue and marginal cost do not combine to deliver the point estimate for the slope of marginal profit.

[31] The negative marginal realized profits suggest that banks were, if anything, too optimistic in their assessment of the profitability of consumers lending to households with intermediate FICO scores. See the discussion in Section 5.4.

lative flows over longer time horizons involves a tradeoff. On the one hand, it allows us to better capture potential life-cycle effects of credit card profitability. On the other hand, focusing on longer time horizons requires us to restrict the analysis to accounts that were originated in the early part of our panel, which reduces the number of quasi-experiments we can exploit. The vertical bars in the plot show 95% confidence intervals constructed by bootstrapping over quasi-experiments.[32]

The plot shows a sharp increase in the MPL by FICO score. For the lowest FICO score group, a 1 percentage point decrease in the cost of funds raises credit limits by $135 when we use discounted flows over 48 months to estimate the MPB and the slope of marginal profits. For consumers in the highest FICO score group, the increase is an order of magnitude larger at $1,477. The estimates are stable to measuring cumulative profits and ADB over different horizons.

### 6.5 Effect on Aggregate Borrowing

The effect of a decline in the cost of funds on aggregate borrowing is given by the product of MPL and MPB.[33] Panel A of Figure 10 shows the effect of a 1 percentage point decrease in the cost of funds on credit limits by FICO score, with the MPL calculated using cumulative profits and ADB over a time horizon of 48 months. Panel B shows the MPB across all cards at 12 months after origination by FICO score. Table 8 shows the corresponding point estimates.

MPL and MPB are strongly negatively correlated, with the highest MPL occurring for the accounts with the lowest MPB. The bottom panel of Table 8 quantifies the importance of this negative correlation by estimating the impact on aggregating borrowing under alternative assumptions. The first row shows a version of this calculation where the negative correlation is not taken into account, and the effect on borrowing is given by the weighted average MPL $\times$ weighted average MPB, where we weight FICO score groups by the total number of accounts within each FICO group in the full sample (see Section 1.4). The second row accounts for this correlation by first calculating MPL $\times$ MPB for each FICO group and then averaging across the FICO groups. The point estimate for MPB is sometimes slightly negative for the highest FICO group. Therefore, the third row shows our preferred version of the calculation where we account for the correlation but bottom-code the MPB at zero. At a 12 month horizon, accounting for the correlation reduces the effect on aggregate borrowing by 68%,

[32]In particular, we draw 500 sets of experiments with replacement, and calculate $MPL = \frac{MPB}{-MP'(CL)}$ using this bootstrap sample. This procedure effectively allows the standard errors of the numerator and denominator to be correlated.

[33]This approach to calculating the effect on aggregate borrowing abstracts away from the existence of spending multipliers or other general equilibrium effects, such as the possibility that additional spending from extra credit might reduce the rate of default of other borrowers.

relative to the naive estimate. Over longer time horizons, accounting for this correlation reduces the effect by between 40% and 69%.

## 7 Conclusion

The effectiveness of bank-mediated stimulatory policy in raising household borrowing depends on whether banks pass through credit expansions to households that want to borrow. We use panel data on all credit cards issued by the 8 largest U.S. banks together with 812 credit limit regression discontinuities to estimate the heterogeneity in banks' marginal propensity to lend (MPL) to different households and heterogeneity in these households' marginal propensity to borrow (MPB).

We find sharp differences in MPB across the FICO score distribution, with a \$1 increase in credit limits raising total borrowing at 12 month after account origination by 58 cents for households with the lowest FICO scores ($\leq 660$) while having no effect on households the highest FICO scores ($> 740$). Banks' MPLs are negatively correlated with these MPBs, with a 1 percentage point reduction in the cost of funds raising credit limits by \$134 dollars for households with the FICO scores below 660 versus \$1,477 for households with FICO scores above 740. This is explained by steeper sloping marginal revenue and marginal costs for lower FICO scores borrowers. Banks pass through credit expansion least to households that want to borrow the most, reducing the effectiveness of bank-mediated stimulatory policy.

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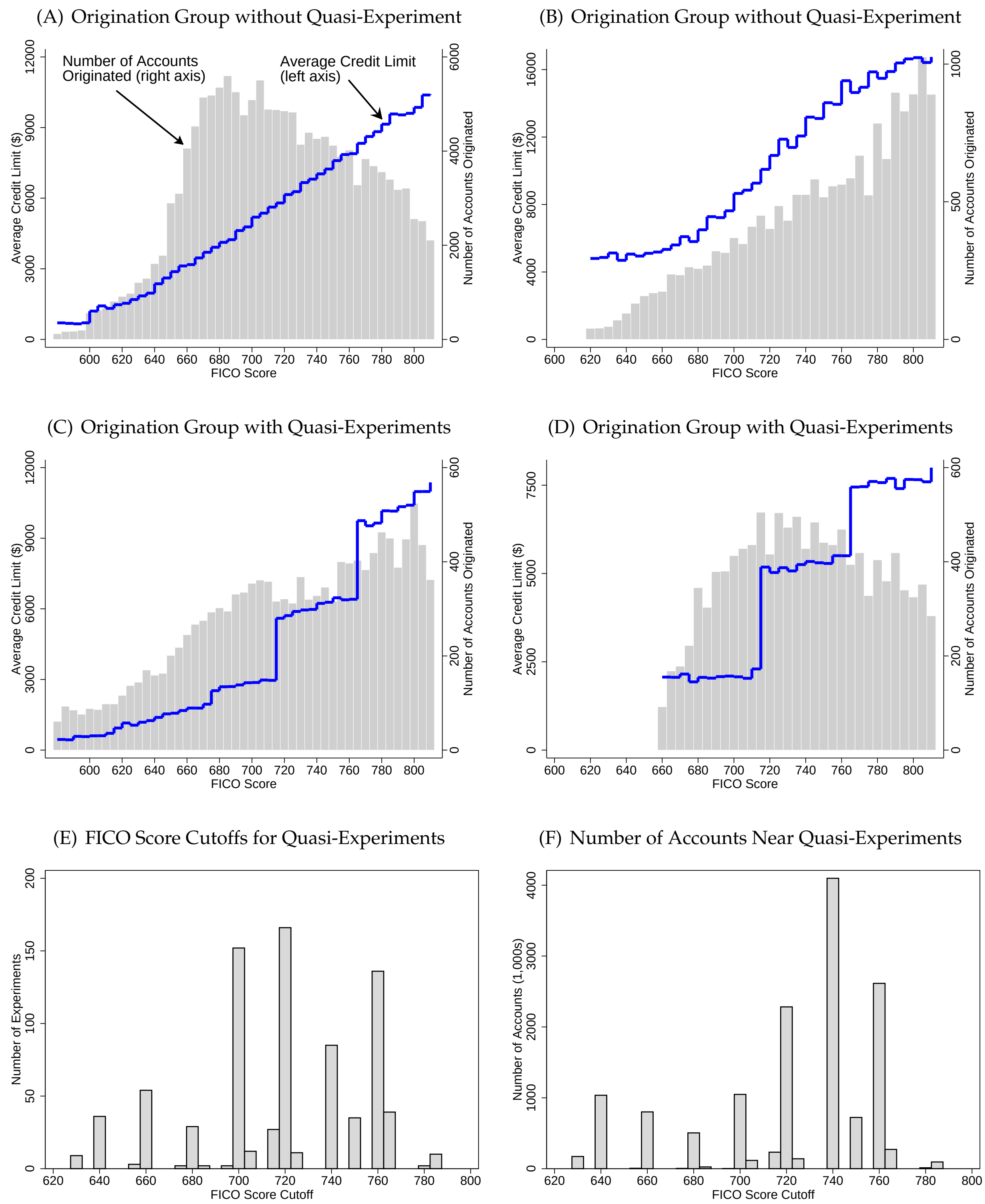
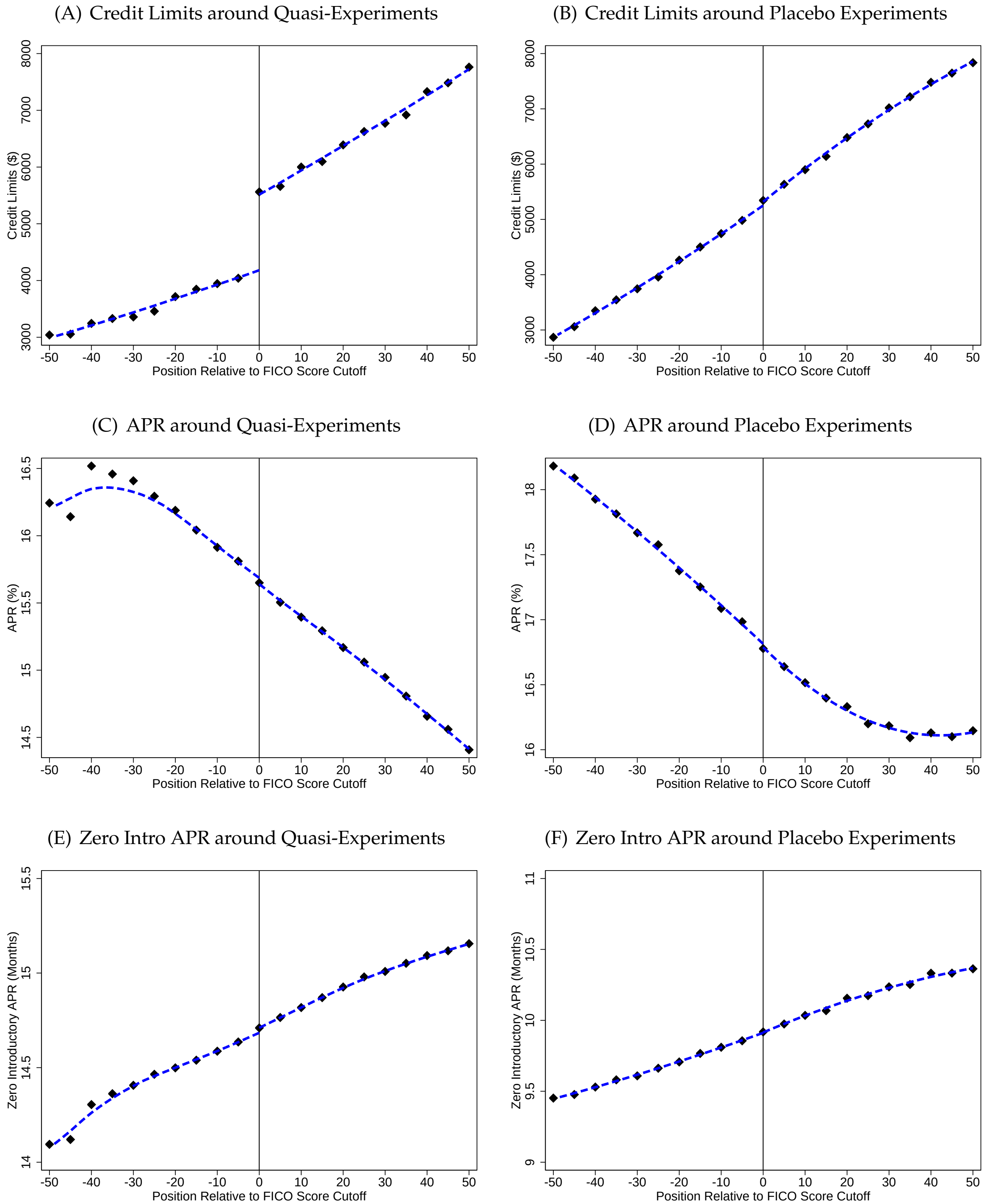
**Figure 2:** Credit Limit Quasi-Experiment: Examples and Summary Statistics




**Note:** Panels A-D show average credit limits by FICO score for accounts in origination groups with and without credit limit quasi-experiments. Origination groups are defined as all credit cards of the same product-type originated by a bank in a particular month through a particular loan channel. The horizontal axis shows FICO score at origination, the blue line plots the average credit limit for accounts in FICO buckets of 5 (left axis). Grey bars show the total number of accounts originated in those buckets (right axis). Panels E and F show summary statistics for the quasi-experiments. Panel E plots the number of quasi-experiments at each FICO score cutoff. Panel F plots the number of accounts within 50 FICO score points of these quasi-experiments by the FICO score cutoff.

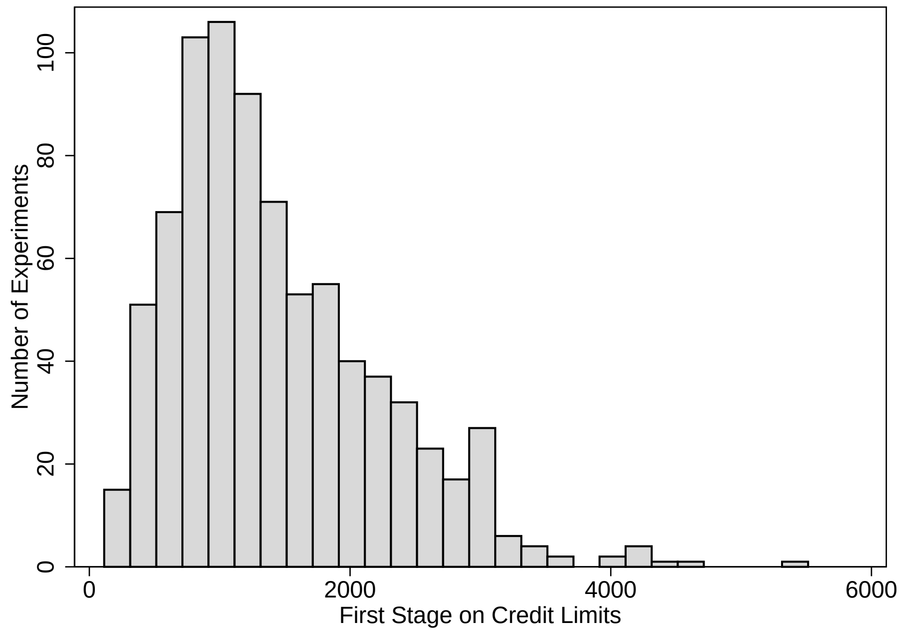
**Figure 3:** Credit Limits and Cost of Credit around Credit Limit Quasi-Experiments



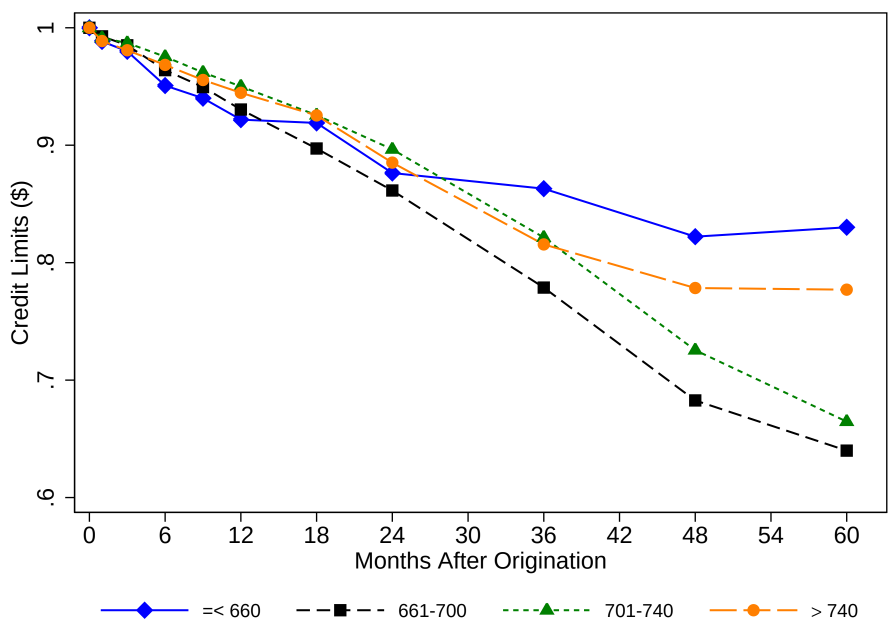

**Note:** Figure plots average credit limits (Panels A and B), average APR (Panels C and D), and the average number of months with zero introductory APR for those originations with zero initial APR (Panels E and F). The left column plots these outcomes around those quasi-experiments without a second quasi-experiment within 50 FICO points in the same origination group. The right column plots the same outcomes around the same FICO score as in the left column, but for credit cards originated in the same month in origination groups with no quasi-experiment at that FICO score. The horizontal axis shows FICO score at origination, centered around the FICO score cutoff. Scatter plots show means of outcomes for 5-point FICO score buckets. Blue lines show the fit from a second-order local polynomial regression estimated separately on either side of the cutoff.

**Figure 4:** Effect of FICO Score Cutoff on Credit Limits

(A) Distribution of First-Stage Coefficients

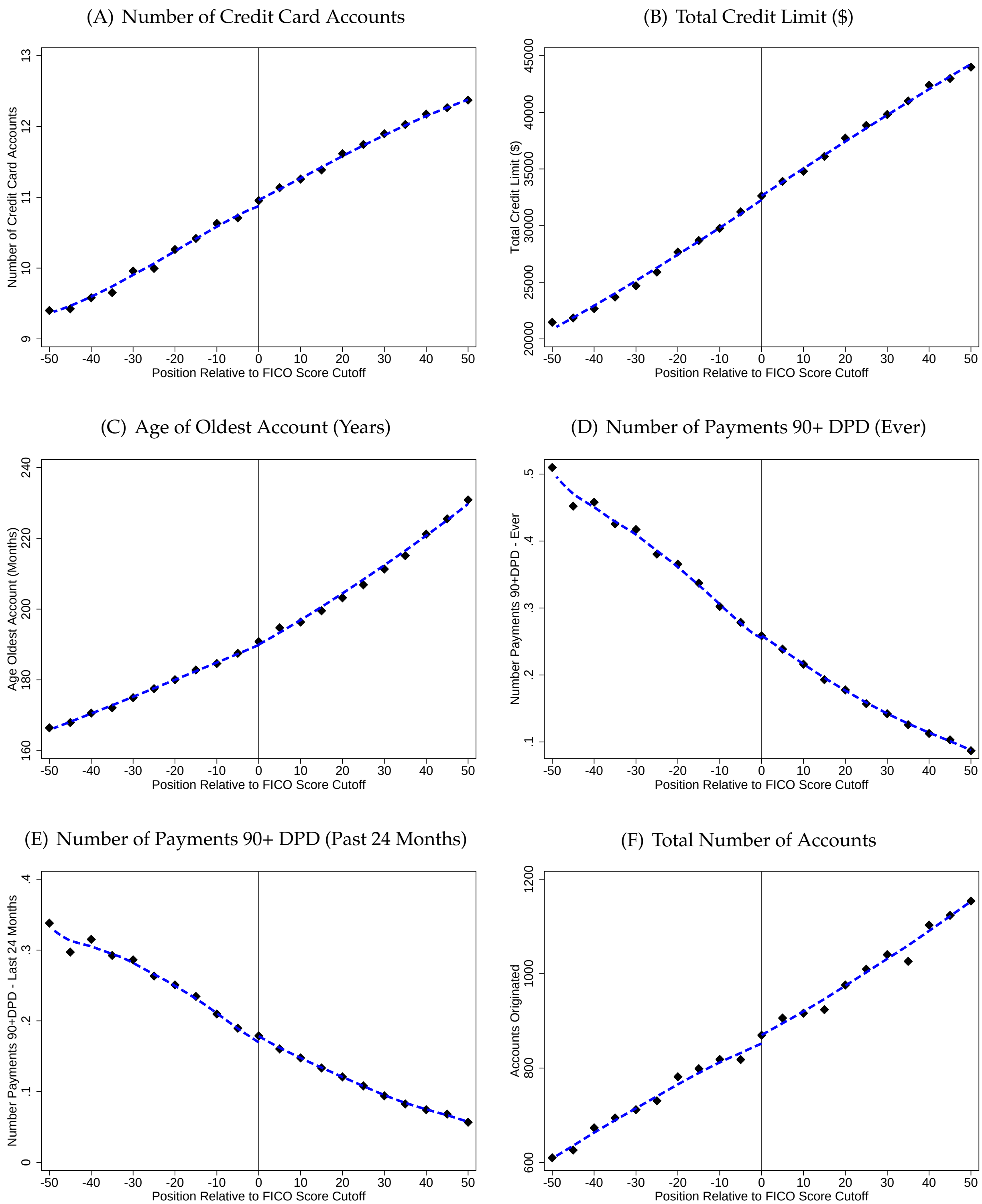



(B) Persistence of a $1 Increase in Credit Limits



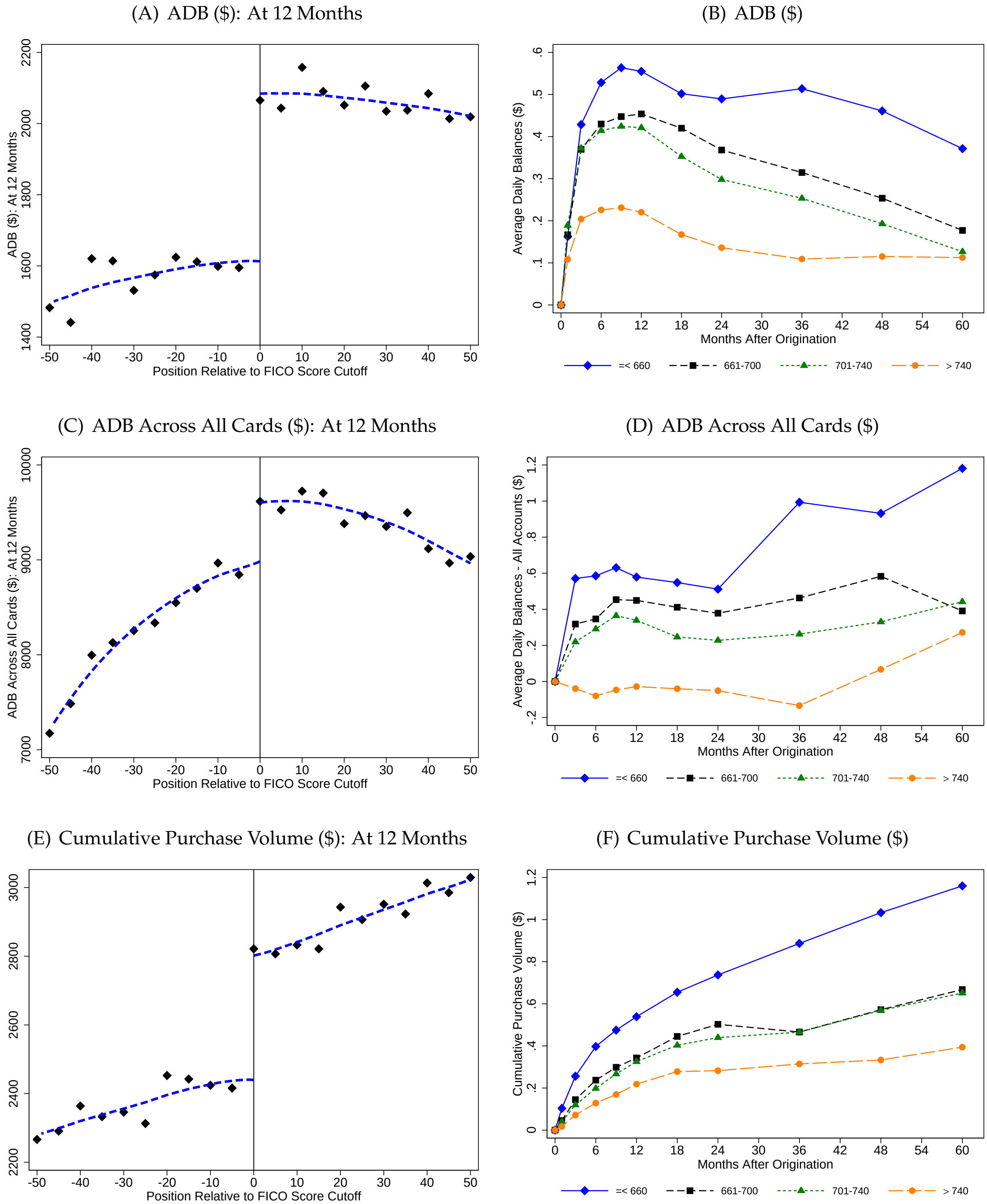

**Note:** Panel A shows the distribution of credit limit jumps at the FICO score cutoff across our 812 credit limit quasi-experiments. Panel B shows regression discontinuity estimates of the effect of a $1 increase in initial credit limits on credit limits at different time horizons after account origination. Estimates are shown for different groups of FICO score at account origination. The corresponding estimates are show in Table 4.

**Figure 5:** Initial Borrower Characteristics around Credit Limit Quasi-Experiments




**Note:** Figure plots average borrower characteristics around credit limit quasi-experiments without a second quasi-experiment within 50 FICO points in the same origination group. The horizontal axis shows FICO score at origination, centered around the FICO cutoff. The vertical axis shows the number of credit card accounts (Panel A), total credit limit on all credit card accounts (Panel B), age of oldest account (Panel C), number of payments ever 90+ days past due (Panel D), number of payments 90+ days past due in last 24 months (Panel E), and the total number of accounts opened (Panel F). All borrower characteristics are as reported to the credit bureau at account origination. Scatter plots show means of outcomes for 5-point FICO score buckets. Blue lines show the fit from a second-order local polynomial regression estimated separately on either side of the cutoff.

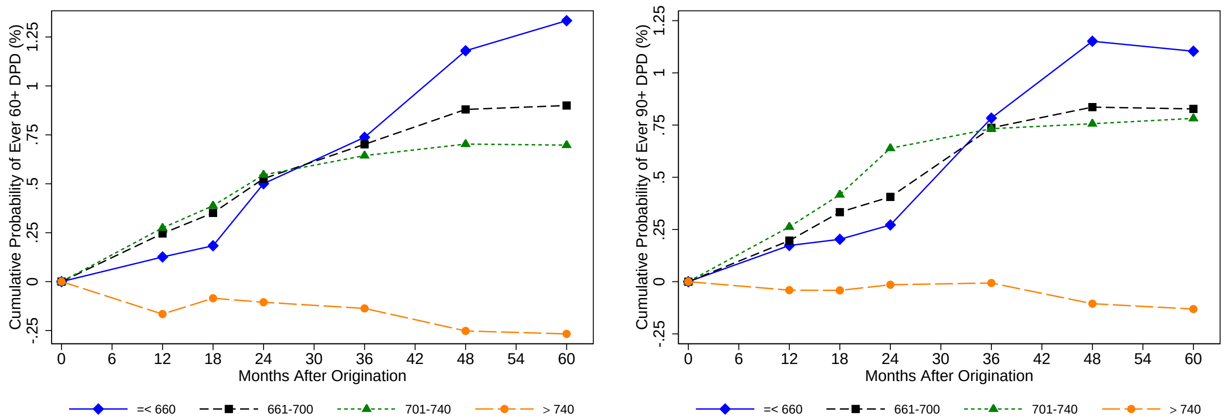
**Figure 6:** Marginal Propensity to Borrow




**Note:** Figure shows the effects of credit limits on borrowing and spending. The top row shows effects on average daily balances on the treated credit card. The middle row shows effects on average daily balances aggregated across all credit cards used by the account holder. The bottom row shows effect on cumulative purchase volume on the treated card. The left column show changes in the outcome variable after 12 months around the credit limit discontinuities, pooled across all quasi-experiments without a second quasi-experiment within 50 FICO points in the same origination group. The right column shows regression discontinuity estimates of the effect of $1 increase in credit limits for different FICO groups and different time horizons after account origination. FICO score groups are determined by FICO score at account origination. The corresponding estimates are show in Table 5.

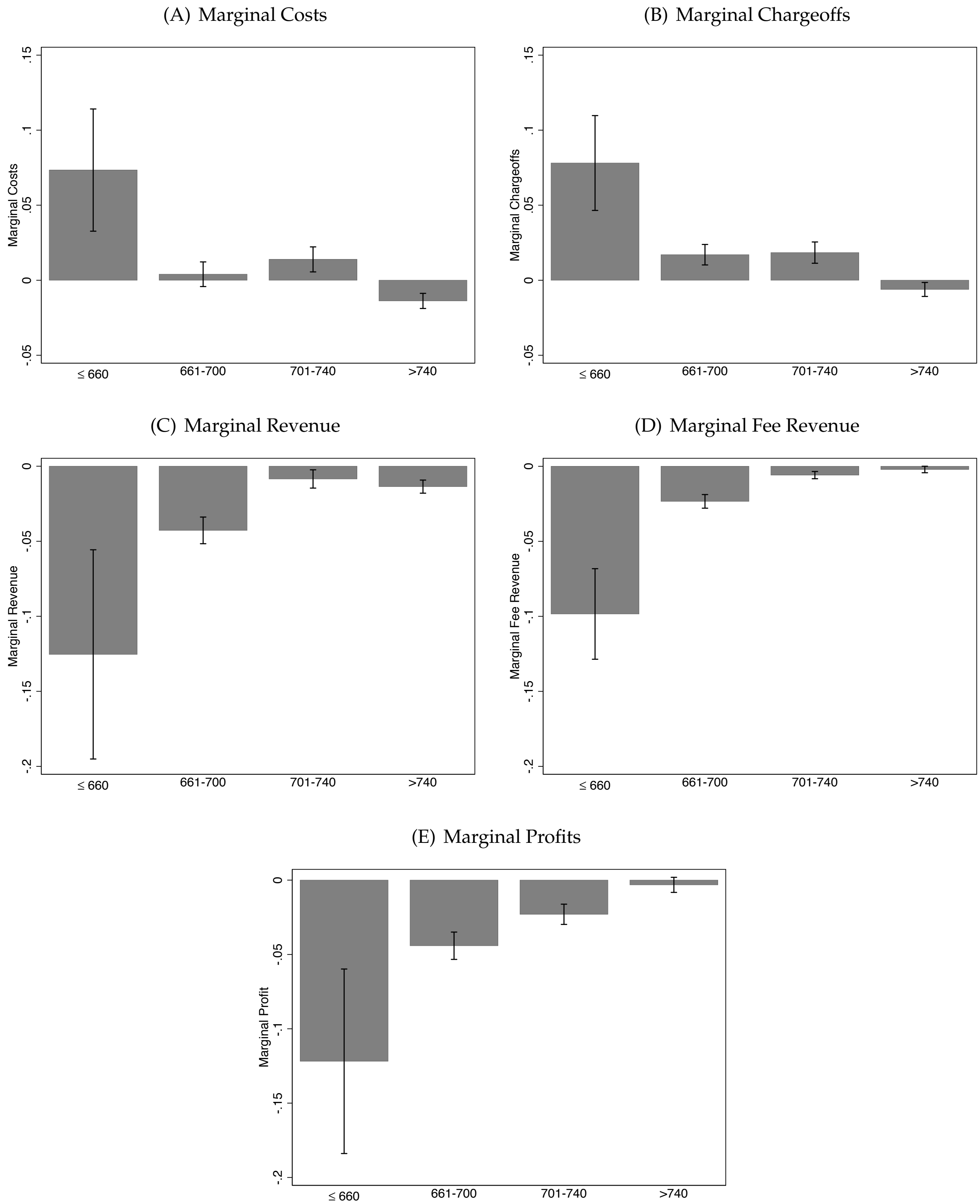
**Figure 7:** Probability of Default

(A) Probability of Default (60+ DPD) (B) Probability of Default (90+ DPD)



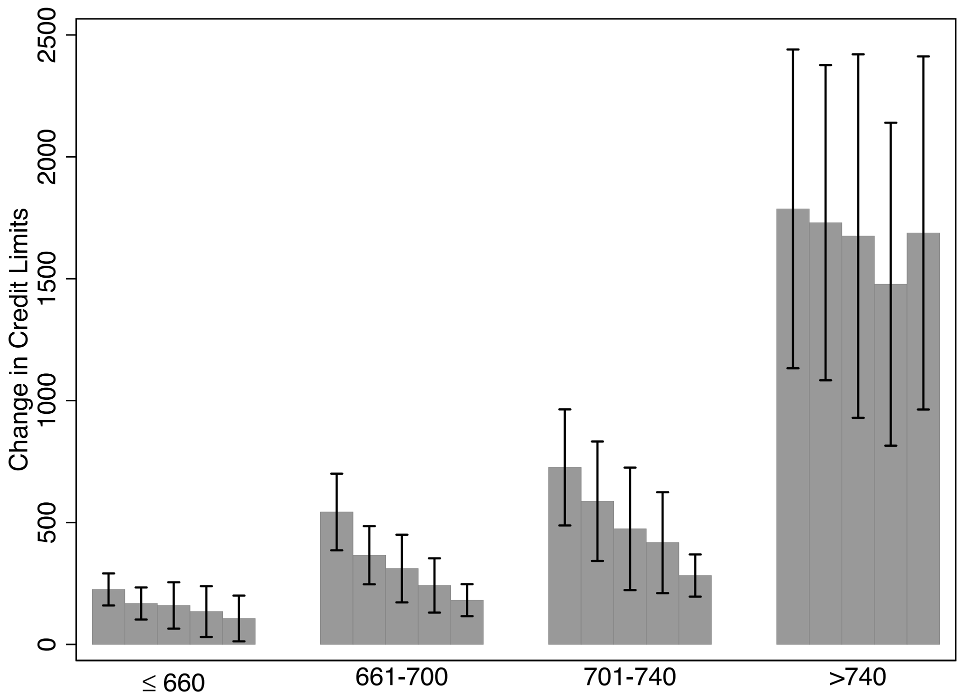

**Note:** Figure shows the effects of credit limits on the probability of default. Panel A shows effects on the cumulative probability of an account being more than 60 days past due (60+ DPD), Panel B on the probability of being more than 90 days past due (90+ DPD). Both panels show regression discontinuity estimates of the effect of a $1,000 increase in credit limits for different FICO groups and different time horizons after account origination. The corresponding estimates are show in Table 7.

**Figure 8:** Effect of $1,000 Credit Limit Increase on Marginal Profits and Components




**Note:** Figure shows the effect of a $1,000 increase in credit limits on marginal profits and its components by FICO score group. Panels A shows the effect on marginal cumulative costs over the first 48 months of since origination. Panels B-E show the effects on marginal cumulative chargeoffs, marginal cumulative income, marginal cumulative fee revenue, and marginal cumulative profits, similarly calculated over the first 48 months since origination. Vertical bars show 95% confidence intervals, constructed by bootstrapping across quasi-experiments. FICO score groups are determined by FICO score at account origination. The corresponding estimates are show in Table 6.

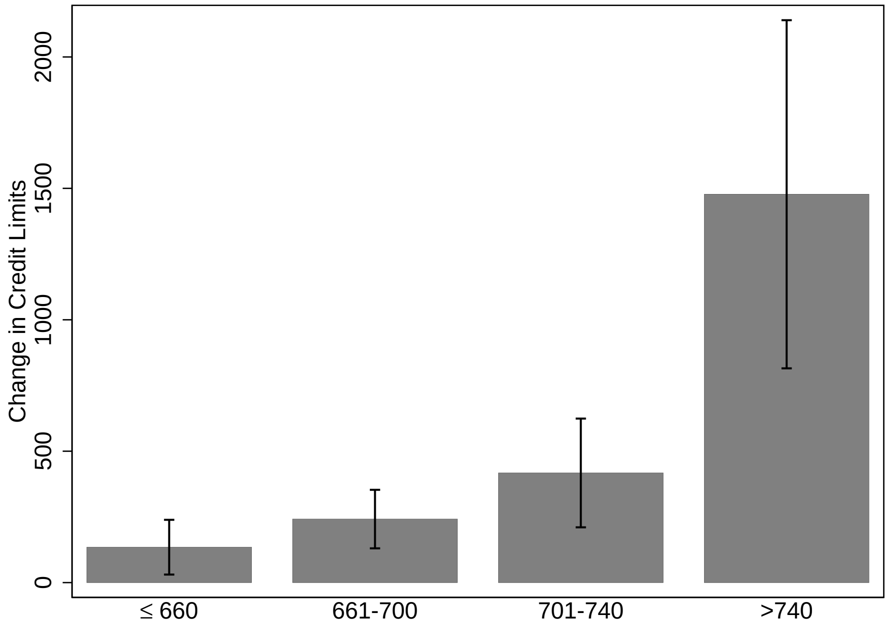
**Figure 9:** Marginal Propensity to Lend (MPL)



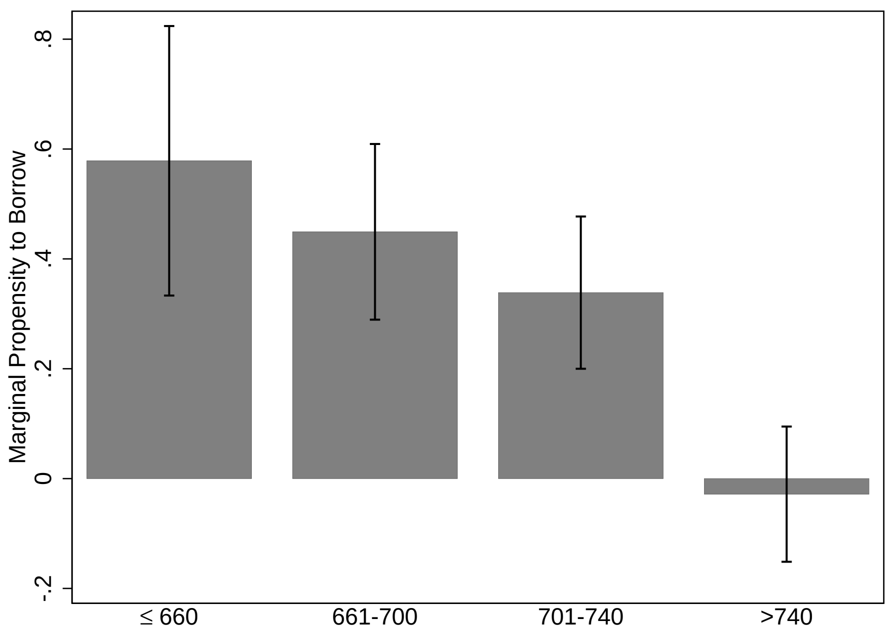

**Note:** Figure shows the implied effect of a 1 percentage point reduction in the cost of borrowing on credit limits by FICO score group. Estimates produced using equation 8. For each FICO score group, we show the implied increase in credit limit when measuring both the slope of marginal profit and marginal borrowing over the first 12, 24, 36, 48, and 60 months following origination. Within each Vertical bars show 95% confidence intervals, constructed by bootstrapping across quasi-experiments. FICO score groups are determined by FICO score at account origination. The corresponding estimates are shown in Table 8.

**Figure 10:** Correlation between MPL and MPB

(A) MPL




(B) MPB Across All Cards: At 12 Months




**Note:** Panel A shows the implied effect of a 1 percentage point reduction in the cost of borrowing on credit limits by FICO score group. The effects are calculated using the marginal profits estimates shown in Figure 8 and Table 6. Panel B shows the effect of a $1 increase in credit limits on borrowing across all cards by FICO group. The corresponding estimates are shown in Table 8. Vertical bars show 95% confidence intervals, constructed by bootstrapping across quasi-experiments. FICO score groups are determined by FICO score at account origination.

**Table 1:** Summary Statistics of Experimental Sample, At Origination

| | Average | S.D | | Average | S.D |
|---|---|---|---|---|---|
| **Credit Limit on Treated Card ($)** | | | **ADB Across All Credit Card Accounts ($)** | | |
| *Pooled* | 5,182 | 2,040 | *Pooled* | 9,447 | 3,599 |
| *≤660* | 2,490 | 677 | *≤660* | 5,074 | 2,252 |
| *661-700* | 4,281 | 1,066 | *661-700* | 10,289 | 2,904 |
| *701-740* | 4,813 | 1,593 | *701-740* | 10,888 | 3,278 |
| *>740* | 6,885 | 1,578 | *>740* | 9,699 | 3,295 |
| **APR on Treated Card (%)** | | | **Credit Limit Across All Credit Card Accounts ($)** | | |
| *Pooled* | 15.70 | 3.90 | *Pooled* | 32,574 | 14,847 |
| *≤660* | 20.60 | 5.36 | *≤660* | 11,783 | 5,267 |
| *661-700* | 14.79 | 3.76 | *661-700* | 26,748 | 7,310 |
| *701-740* | 15.35 | 3.09 | *701-740* | 32,458 | 8,647 |
| *>740* | 14.88 | 2.50 | *>740* | 44,618 | 12,711 |
| **Number of Credit Card Accounts** | | | **Number Times 90+ DPD In Last 24 Months** | | |
| *Pooled* | 10.83 | 2.98 | *Pooled* | 0.18 | 0.31 |
| *≤660* | 6.90 | 1.29 | *≤660* | 0.92 | 0.32 |
| *661-700* | 10.29 | 1.67 | *661-700* | 0.44 | 0.17 |
| *701-740* | 11.14 | 2.33 | *701-740* | 0.29 | 0.11 |
| *>740* | 12.54 | 2.93 | *>740* | 0.13 | 0.08 |
| **Age Oldest Account (Months)** | | | **Number Accounts Currently 90+DPD** | | |
| *Pooled* | 189.3 | 29.6 | *Pooled* | 0.03 | 0.04 |
| *≤660* | 158.9 | 26.9 | *≤660* | 0.10 | 0.05 |
| *661-700* | 181.3 | 19.4 | *661-700* | 0.03 | 0.02 |
| *701-740* | 185.8 | 24.2 | *701-740* | 0.02 | 0.02 |
| *>740* | 208.9 | 25.5 | *>740* | 0.01 | 0.01 |

**Note:** Table shows summary statistics and regression discontinuity estimates for the accounts in our experimental sample at the time the account was originated. These values are calculated using data at the quasi-experiment level. For each quasi-experiment, we calculate the mean value for a given variable across all of the accounts within 5 FICO score points of the cutoff. We then show the means and standard deviations of these values across our 812 quasi-experiments.

**Table 2:** Summary Statistics of Experimental Sample, Post Origination

| | FICO Score Group | | | | | FICO Score Group | | | | | FICO Score Group | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ≤660 | 661-700 | 701-740 | >740 | | ≤660 | 661-700 | 701-740 | >740 | | ≤660 | 661-700 | 701-740 | >740 |
| **Credit Limit ($)** | | | | | **Cumulative Costs ($)** | | | | | **Cumulative Revenue ($)** | | | | |
| *After 12 Months* | 2,547 | 4,316 | 4,957 | 6,905 | *After 12 Months* | 114 | 170 | 166 | 141 | *After 12 Months* | 226 | 190 | 178 | 167 |
| *After 24 Months* | 2,362 | 4,238 | 4,931 | 6,826 | *After 24 Months* | 267 | 446 | 422 | 293 | *After 24 Months* | 463 | 498 | 431 | 332 |
| *After 36 Months* | 2,291 | 4,537 | 5,012 | 6,560 | *After 36 Months* | 437 | 705 | 624 | 373 | *After 36 Months* | 724 | 791 | 645 | 426 |
| *After 48 Months* | 2,286 | 4,424 | 4,936 | 6,222 | *After 48 Months* | 551 | 836 | 793 | 449 | *After 48 Months* | 915 | 970 | 849 | 522 |
| *After 60 Months* | 2,312 | 4,316 | 4,564 | 6,041 | *After 60 Months* | 716 | 947 | 878 | 510 | *After 60 Months* | 1,154 | 1,123 | 943 | 596 |
| **ADB ($)** | | | | | **Cumulative Chargeoffs ($)** | | | | | **Cumulative Interest Charge Revenue ($)** | | | | |
| *After 12 Months* | 1,171 | 2,172 | 2,175 | 1,970 | *After 12 Months* | 45 | 65 | 60 | 36 | *After 12 Months* | 110 | 59 | 51 | 41 |
| *After 24 Months* | 1,024 | 1,794 | 1,700 | 1,416 | *After 24 Months* | 169 | 255 | 239 | 123 | *After 24 Months* | 295 | 290 | 238 | 154 |
| *After 36 Months* | 1,127 | 1,724 | 1,461 | 1,213 | *After 36 Months* | 289 | 442 | 390 | 183 | *After 36 Months* | 475 | 518 | 409 | 234 |
| *After 48 Months* | 1,045 | 1,502 | 1,243 | 947 | *After 48 Months* | 374 | 549 | 514 | 243 | *After 48 Months* | 609 | 667 | 569 | 319 |
| *After 60 Months* | 1,041 | 1,459 | 1,090 | 988 | *After 60 Months* | 483 | 627 | 586 | 288 | *After 60 Months* | 765 | 790 | 643 | 389 |
| **ADB Across All Cards ($)** | | | | | **Cumulative Prob 60+ DPD ($)** | | | | | **Cumulative Fee Revenue ($)** | | | | |
| *After 12 Months* | 5,768 | 10,727 | 11,374 | 10,331 | *After 12 Months* | 6.4% | 4.1% | 3.6% | 1.7% | *After 12 Months* | 68 | 79 | 78 | 70 |
| *After 24 Months* | 5,621 | 10,679 | 11,256 | 10,372 | *After 24 Months* | 12.2% | 9.3% | 8.2% | 3.8% | *After 24 Months* | 119 | 129 | 119 | 95 |
| *After 36 Months* | 6,180 | 10,600 | 11,632 | 10,829 | *After 36 Months* | 15.2% | 12.1% | 10.8% | 5.2% | *After 36 Months* | 184 | 174 | 153 | 108 |
| *After 48 Months* | 6,460 | 10,275 | 11,575 | 10,689 | *After 48 Months* | 16.5% | 13.5% | 12.1% | 5.9% | *After 48 Months* | 233 | 202 | 184 | 115 |
| *After 60 Months* | 7,595 | 10,376 | 11,858 | 11,623 | *After 60 Months* | 17.2% | 14.2% | 12.8% | 6.2% | *After 60 Months* | 364 | 306 | 211 | 75 |
| **Cumulative Purchase Volume ($)** | | | | | **Cumulative Cost of Funds ($)** | | | | | **Cumulative Profits ($)** | | | | |
| *After 12 Months* | 2,437 | 2,574 | 2,476 | 2,807 | *After 12 Months* | 14 | 16 | 16 | 15 | *After 12 Months* | 112 | 20 | 13 | 27 |
| *After 24 Months* | 2,403 | 3,926 | 3,718 | 4,134 | *After 24 Months* | 23 | 29 | 28 | 26 | *After 24 Months* | 196 | 55 | 10 | 42 |
| *After 36 Months* | 3,209 | 4,947 | 4,150 | 4,238 | *After 36 Months* | 29 | 37 | 36 | 32 | *After 36 Months* | 287 | 93 | 25 | 58 |
| *After 48 Months* | 3,629 | 5,060 | 4,845 | 4,415 | *After 48 Months* | 32 | 42 | 41 | 36 | *After 48 Months* | 363 | 133 | 57 | 73 |
| *After 60 Months* | 4,601 | 5,477 | 5,015 | 4,854 | *After 60 Months* | 33 | 45 | 43 | 38 | *After 60 Months* | 438 | 176 | 66 | 86 |

**Note:** Table shows average values for our key dependent and explanatory variables within 5 FICO score points of our quasi-experiments. Averages are presented separately by the FICO score of the quasi-experiment. Since later quasi-experiments are observed for shorter periods of time only, the set of experiments contributing to the averages across different horizons is not constant.

**Table 3:** Validity of Research Design

| | Average | Median | Standard Devation | Baseline |
|---|---|---|---|---|
| *Credit Limit* | 1,429 | 1,218 | 795 | 5,182 |
| *APR (%)* | -0.037 | -0.014 | 0.511 | 15.702 |
| *Months to Rate Change* | -0.053 | 0.009 | 1.233 | 14.432 |
| *Number of Credit Card Accounts* | 0.068 | 0.032 | 0.706 | 10.832 |
| *Total Credit Limit - All Accounts* | 142 | 35 | 2,701 | 32,574 |
| *Age Oldest Account (Months)* | 0.928 | 0.361 | 10.933 | 189.268 |
| *Number Times 90+ DPD - Last 24 Months* | 0.010 | 0.002 | 0.114 | 0.184 |
| *Number Accounts 90+ DPD - At Origination* | 0.001 | 0.002 | 0.017 | 0.030 |
| *Number Accounts 90+DPD - Ever* | 0.005 | 0.003 | 0.100 | 0.268 |
| *Number of Accounts Originated* | 22.00 | 5.82 | 73.74 | 843.82 |

**Note:** Table shows the reduced form estimates of the RD, corresponding to the numerator of equation 3. This captures the "jump" of the variables at the FICO score cutoff. All variables are measured at account origination, allowing us to inspect the validity of the research design. We present the average, median, and standard deviation of this jump across our 812 quasi-experiments. We also present the average of value of the variable at the cutoff, allowing us to judge the economic significance of any differences.

**Table 4:** Persistence of Credit Limit Effect

| | Months After Account Origination | | | | |
|---|---|---|---|---|---|
| | **12** | **24** | **36** | **48** | **60** |
| *FICO* | | | | | |
| *≤660* | 0.92 | 0.88 | 0.86 | 0.82 | 0.83 |
| | (0.01) | (0.02) | (0.04) | (0.05) | (0.09) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *661-700* | 0.93 | 0.86 | 0.78 | 0.68 | 0.64 |
| | (0.01) | (0.02) | (0.02) | (0.04) | (0.07) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *701-740* | 0.95 | 0.90 | 0.82 | 0.73 | 0.66 |
| | (0.01) | (0.01) | (0.02) | (0.04) | (0.06) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *>740* | 0.94 | 0.89 | 0.82 | 0.78 | 0.78 |
| | (0.01) | (0.02) | (0.03) | (0.04) | (0.08) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |

**Note:** Table shows regression discontinuity estimates of the effect of $1 increase in initial credit limits on credit limits at different time horizons after account origination. Columns show effects at different time horizons after account origination. Rows show effects for different FICO score groups, defined at account origination. Standard errors, constructed by bootstrapping over quasi-experiments, are shown in parenthesis. The corresponding *p*-values are shown in square brackets.

**Table 5:** Marginal Propensity to Borrow

| | Months After Account Origination | | | | |
|---|---|---|---|---|---|
| | 12 | 24 | 36 | 48 | 60 |
| **Panel A: Average Daily Balance** | | | | | |
| *FICO* | | | | | |
| *≤660* | 0.55 | 0.49 | 0.51 | 0.46 | 0.37 |
| | (0.03) | (0.03) | (0.03) | (0.05) | (0.04) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *661-700* | 0.45 | 0.37 | 0.31 | 0.25 | 0.18 |
| | (0.01) | (0.01) | (0.02) | (0.02) | (0.03) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *701-740* | 0.42 | 0.30 | 0.25 | 0.19 | 0.13 |
| | (0.01) | (0.02) | (0.02) | (0.02) | (0.03) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *>740* | 0.22 | 0.14 | 0.11 | 0.12 | 0.11 |
| | (0.01) | (0.01) | (0.02) | (0.02) | (0.03) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| **Panel B: Average Daily Balance Across All Cards** | | | | | |
| *FICO* | | | | | |
| *≤660* | 0.58 | 0.51 | 0.99 | 0.93 | 1.18 |
| | (0.13) | (0.17) | (0.26) | (0.47) | (0.67) |
| | [0.00] | [0.00] | [0.00] | [0.05] | [0.08] |
| *661-700* | 0.45 | 0.38 | 0.46 | 0.58 | 0.39 |
| | (0.08) | (0.09) | (0.12) | (0.19) | (0.29) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.18] |
| *701-740* | 0.34 | 0.23 | 0.26 | 0.33 | 0.44 |
| | (0.07) | (0.08) | (0.10) | (0.15) | (0.41) |
| | [0.00] | [0.00] | [0.01] | [0.03] | [0.28] |
| *>740* | -0.03 | -0.05 | -0.13 | 0.07 | 0.27 |
| | (0.06) | (0.09) | (0.14) | (0.16) | (0.37) |
| | [0.65] | [0.55] | [0.33] | [0.68] | [0.47] |
| **Panel C: Cumulative Purchase Volume** | | | | | |
| *FICO* | | | | | |
| *≤660* | 0.54 | 0.74 | 0.89 | 1.03 | 1.16 |
| | (0.05) | (0.08) | (0.11) | (0.16) | (0.19) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *661-700* | 0.34 | 0.50 | 0.47 | 0.57 | 0.67 |
| | (0.02) | (0.04) | (0.05) | (0.07) | (0.10) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *701-740* | 0.33 | 0.44 | 0.47 | 0.57 | 0.65 |
| | (0.02) | (0.04) | (0.05) | (0.08) | (0.12) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| *>740* | 0.22 | 0.28 | 0.31 | 0.33 | 0.39 |
| | (0.02) | (0.03) | (0.04) | (0.06) | (0.09) |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |

**Note:** Table shows regression discontinuity estimates of the effect of $1 increase in credit limits on borrowing and spending. Panel A shows effects on average daily balances on the treated credit card. Panel B shows effects on average daily balances aggregated across all credit cards used by the account holder. Panel C shows effects on cumulative purchase volume on the treated credit card. Columns show effects at different time horizons after account origination. Within each panel, rows show effects for different FICO score groups, defined at account origination. Standard errors, constructed by bootstrapping over quasi-experiments, are shown in parenthesis. The corresponding $p$-values are shown in square brackets.

**Table 6:** Effect of a $1,000 Credit Limit Increase on Marginal Profits and Components

| | Costs | | Chargeoffs | | Revenue | | Fees | | Profits | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Marginal Effect | Effect of $1K Increase on Marginal Effect | Marginal Effect | Effect of $1K Increase on Marginal Effect | Marginal Effect | Effect of $1K Increase on Marginal Effect | Marginal Effect | Effect of $1K Increase on Marginal Effect | Marginal Effect | Effect of $1K Increase on Marginal Effect |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| FICO | | | | | | | | | | |
| ≤660 | 0.283 | 0.073 | 0.209 | 0.078 | 0.232 | -0.125 | 0.025 | -0.098 | 0.003 | -0.122 |
| | (0.019) | (0.021) | (0.020) | (0.016) | (0.027) | (0.036) | (0.008) | (0.015) | (0.028) | (0.032) |
| | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.001] | [0.000] | [0.925] | [0.000] |
| 661-700 | 0.180 | 0.004 | 0.134 | 0.017 | 0.140 | -0.043 | 0.012 | -0.023 | -0.035 | -0.044 |
| | (0.011) | (0.004) | (0.011) | (0.003) | (0.008) | (0.005) | (0.003) | (0.002) | (0.012) | (0.005) |
| | [0.000] | [0.340] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.005] | [0.000] |
| 701-740 | 0.157 | 0.014 | 0.113 | 0.018 | 0.125 | -0.008 | 0.017 | -0.006 | -0.027 | -0.023 |
| | (0.010) | (0.004) | (0.010) | (0.004) | (0.007) | (0.003) | (0.002) | (0.001) | (0.010) | (0.003) |
| | [0.000] | [0.001] | [0.000] | [0.000] | [0.000] | [0.006] | [0.000] | [0.000] | [0.008] | [0.000] |
| >740 | 0.054 | -0.014 | 0.034 | -0.006 | 0.048 | -0.014 | 0.007 | -0.002 | -0.006 | -0.003 |
| | (0.009) | (0.003) | (0.009) | (0.002) | (0.006) | (0.002) | (0.001) | (0.001) | (0.009) | (0.003) |
| | [0.000] | [0.000] | [0.000] | [0.010] | [0.000] | [0.000] | [0.000] | [0.057] | [0.546] | [0.219] |

**Note:** Table shows the effect of a $1,000 increase in credit limits on marginal profits and its components. In particular, we show effects for costs, chargeoffs (which are an important component of costs), income, fee revenue (which is an important component of income) and profits (which is defined as income minus costs). We measure these variables over a time horizon of 48 months after account origination. For each measure, the left column shows the RD estimate of the marginal effect of a $1 increase in credit limits at the prevailing equilibrium level, and the right column shows the impact of $1,000 increase in credit limits on this marginal effect. Rows show effects for different FICO score groups, defined at account origination. Standard errors, constructed by bootstrapping over quasi-experiments, are shown in parenthesis. The corresponding $p$-values are shown in square brackets.

**Table 7:** Probability of Default

| | Months After Account Origination | | | | |
|---|---|---|---|---|---|
| | 12 | 24 | 36 | 48 | 60 |
| **Panel A: 60+ Days Past Due (%)** | | | | | |
| *FICO* | | | | | |
| *≤660* | 0.13 | 0.50 | 0.74 | 1.18 | 1.33 |
| | (0.26) | (0.33) | (0.37) | (0.36) | (0.35) |
| | [0.63] | [0.13] | [0.05] | [0.00] | [0.00] |
| *661-700* | 0.25 | 0.53 | 0.70 | 0.88 | 0.90 |
| | (0.14) | (0.18) | (0.21) | (0.22) | (0.22) |
| | [0.08] | [0.00] | [0.00] | [0.00] | [0.00] |
| *701-740* | 0.27 | 0.55 | 0.64 | 0.70 | 0.70 |
| | (0.13) | (0.16) | (0.17) | (0.17) | (0.16) |
| | [0.04] | [0.00] | [0.00] | [0.00] | [0.00] |
| *>740* | -0.17 | -0.11 | -0.14 | -0.25 | -0.27 |
| | (0.08) | (0.13) | (0.14) | (0.15) | (0.14) |
| | [0.03] | [0.40] | [0.34] | [0.09] | [0.06] |
| **Panel B: 90+ Days Past Due (%)** | | | | | |
| *FICO* | | | | | |
| *≤660* | 0.17 | 0.27 | 0.78 | 1.15 | 1.10 |
| | (0.20) | (0.32) | (0.32) | (0.33) | (0.33) |
| | [0.38] | [0.40] | [0.01] | [0.00] | [0.00] |
| *661-700* | 0.20 | 0.41 | 0.74 | 0.84 | 0.83 |
| | (0.11) | (0.17) | (0.18) | (0.17) | (0.19) |
| | [0.08] | [0.02] | [0.00] | [0.00] | [0.00] |
| *701-740* | 0.26 | 0.64 | 0.73 | 0.76 | 0.78 |
| | (0.11) | (0.16) | (0.16) | (0.16) | (0.16) |
| | [0.01] | [0.00] | [0.00] | [0.00] | [0.00] |
| *>740* | -0.04 | -0.01 | -0.01 | -0.11 | -0.13 |
| | (0.07) | (0.11) | (0.14) | (0.14) | (0.13) |
| | [0.55] | [0.89] | [0.96] | [0.44] | [0.32] |

**Note:** Table shows regression discontinuity estimates of the effect of a increase in credit limits on the probability of default. Panel A shows the effects of a $1,000 increase in credit limits on the probability that the account is at least 60 days past due (60+ DPD), Panel B on the probability that the account is at least 90 days past due (90+ DPD). Columns show effects at different time horizons after account origination. Within each panel, rows show effects for different FICO score groups, defined at account origination. Standard errors, constructed by bootstrapping over quasi-experiments, are shown in parenthesis. The corresponding $p$-values are shown in square brackets.

**Table 8:** Marginal Propensity to Lend × Marginal Propensity to Borrow

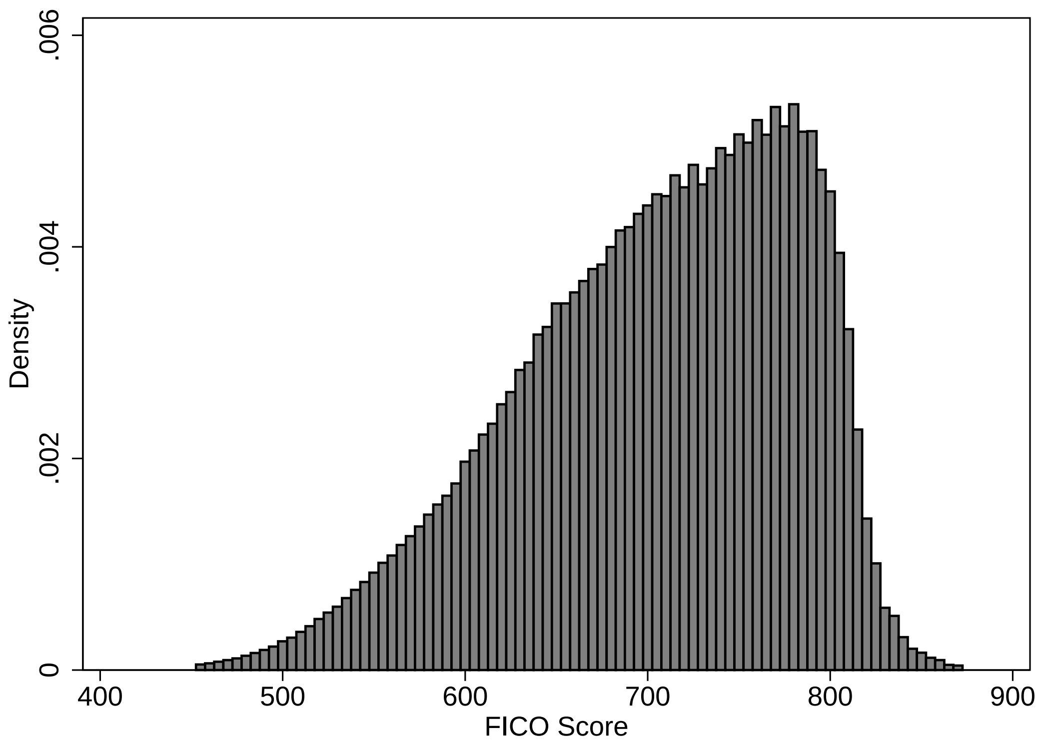
| | MPL | MPB Across All Cards | | | | |
|---|---|---|---|---|---|---|
| | | 12 Months | 24 Months | 36 Months | 48 Months | 60 Months |
| FICO | | | | | | |
| ≤660 | 134.74 | 0.58 | 0.51 | 0.99 | 0.93 | 1.18 |
| | (53.15) | 0.13 | 0.17 | 0.26 | 0.47 | 0.67 |
| | [0.01] | [0.00] | [0.00] | [0.00] | [0.05] | [0.08] |
| 661-700 | 241.75 | 0.45 | 0.38 | 0.46 | 0.58 | 0.39 |
| | (56.80) | 0.08 | 0.09 | 0.12 | 0.19 | 0.29 |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] | [0.18] |
| 701-740 | 417.21 | 0.34 | 0.23 | 0.26 | 0.33 | 0.44 |
| | (105.56) | 0.07 | 0.08 | 0.10 | 0.15 | 0.41 |
| | [0.00] | [0.00] | [0.00] | [0.01] | [0.03] | [0.28] |
| >740 | 1477.62 | -0.03 | -0.05 | -0.13 | 0.07 | 0.27 |
| | (337.94) | 0.06 | 0.09 | 0.14 | 0.16 | 0.37 |
| | [0.00] | [0.65] | [0.55] | [0.33] | [0.68] | [0.47] |
| Weighted Average | 703.84 | 0.29 | 0.23 | 0.35 | 0.44 | 0.58 |

| | MPL X MPB | | | | |
|---|---|---|---|---|---|
| | 12 Months | 24 Months | 36 Months | 48 Months | 60 Months |
| Without Accounting for Correlation | 202.26 | 160.50 | 248.00 | 310.90 | 406.54 |
| Accounting for Correlation | 50.15 | 23.72 | 2.43 | 120.23 | 243.42 |
| Accounting for Correlation + Lower Bound | 65.72 | 51.82 | 75.99 | 120.23 | 243.42 |

**Note:** Table shows the implied effects of a 1 percentage point reduction in the cost of borrowing on lending and borrowing. The first column of the top panel shows the effect of this reduction in the cost of borrowing on credit limits by FICO score, defined at account origination - it is constructed using marginal cumulative MPB and the slope of marginal cumulative profits over the first 48 months post horizon; Figure 9 shows that these patterns persist when calculating the MPL using different horizons. The remaining columns reproduce the MPB estimates for all account from Table 5 at different time horizons after account origination. Standard errors, constructed by bootstrapping over quasi-experiments, are shown in parenthesis. The corresponding $p$-values are shown in square brackets. The bottom panel shows the implied stimulative effect at these same time horizons. The estimates that do not account for correlation are calculated as weighted average MPL × weighted average MPB. The estimates that account for this correlation are constructed by first calculating MPL × MPB for each FICO score group and then taking the weighted average. In the last row we set the (statistically insignificant) negative coefficient for MPB for high FICO score borrowers to zero. Weighted averages are produced by weighting each group by the share of credit card holders with that FICO score in our data (see Section 1.4 and Appendix Figure A1).

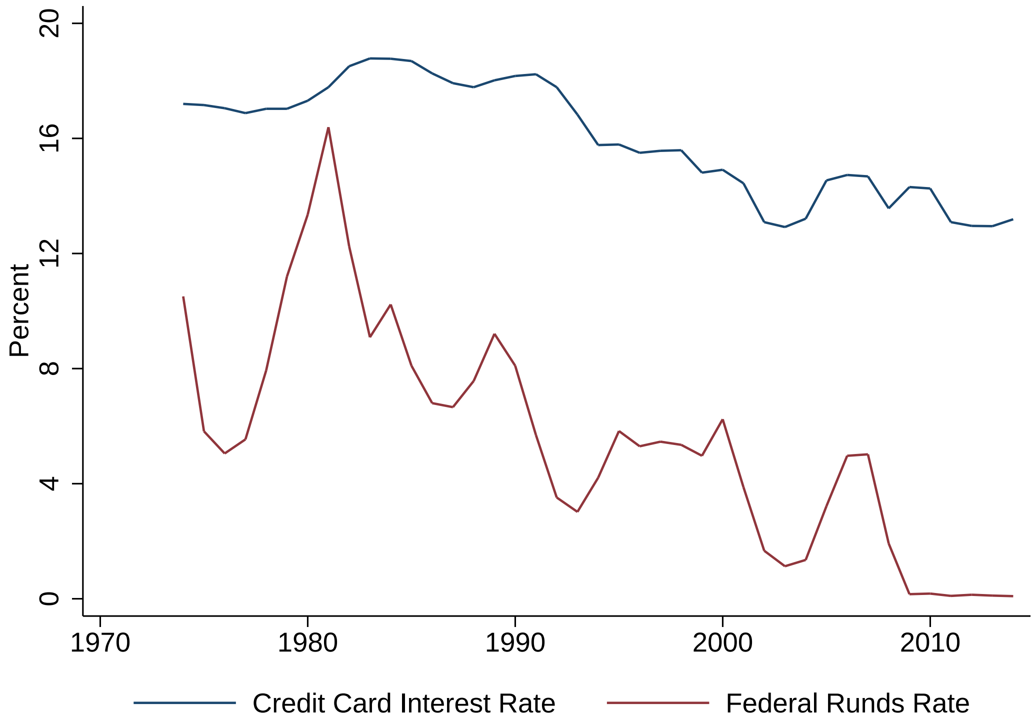
# APPENDIX

**Figure A1:** FICO Score, Population Distribution




**Note:** Figure shows the distribution over FICO scores of all credit cards issued by the banks in our sample, averaged over the period January 2008 to December 2013.

**Figure A2:** Credit Card Interest Rates vs. Federal Funds Rate




**Note:** Figure shows credit card interest rates and the Federal Funds Rate between 1974 and 2014. Before 1994, credit card interest rates were those reported in the Federal Reserve's "Quarterly Report of Interest Rates on Selected Direct Installment Loans." From 1994 onwards, credit card interest rates are from the Federal Reserve's "Quarterly Report of Credit Card Interest Rates for those credit card holders incurring interest charges."