The Marginal Propensity to Consume Over the Business Cycle*

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Abstract

This paper estimates how the marginal propensity to consume out of liquidity (MPC) varies over the business cycle. Ten years after an individual declares Chapter 7 bankruptcy, the record of the bankruptcy is removed from her credit report, generating an immediate and persistent increase in credit score. We study the effects of "bankruptcy flag" removal using a sample of over 160,000 bankruptcy filers whose flags were removed between 2004 and 2011. We document that in the year following flag removal, credit card limits increase by $780 and credit card balances increase by roughly $290, implying an MPC of 0.37. Using cohorts of flag removals over the last business cycle, we find that the MPC is 20 to 30 percent higher during the Great Recession compared to surrounding years. The MPC also increased during the 2001 recession, and is positively correlated with the local unemployment rate. We find no evidence that this counter-cyclical variation in the MPC is accounted for by changes in prices, the composition of borrowers, or credit supply over the business cycle. Taken together, these results are consistent with models where liquidity constraints bind more frequently when macro-economic conditions are poor.

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1. Introduction

Households exhibit a high marginal propensity to consume (MPC) out of transitory income shocks.[1] For instance, when households receive hundreds of dollars in tax rebates, they quickly spend nearly two-thirds of the money (Johnson, Parker, and Souleles 2006, Parker et al. 2013). Additionally, several studies have documented that many households exhibit a high "MPC out of liquidity." That is, households increase their borrowing on credit cards in response to increased credit limits, even when they are far from their limits ex ante (Gross and Souleles 2002, Agarwal et al. 2015, Aydin 2016). Both of these findings pose challenges to the canonical Permanent Income Hypothesis, leading to a large and active literature developing and testing alternative models of household behavior. To rationalize the empirical findings, recent models emphasize adjustment costs, illiquid assets, and liquidity constraints (Johnson, Parker, and Souleles 2006; Telyukova 2013; Kaplan and Violante 2014).

These types of frictions suggest that the MPC may evolve with aggregate economic conditions. For example, if liquidity constraints are more likely to bind during recessions, then the MPC may rise. By contrast, if many households are "wealthy hand-to-mouth," holding little liquid wealth but much illiquid wealth, then the MPC may be higher during mild recessions but lower during severe recessions (Kaplan and Violante, 2014). Direct evidence of how the MPC varies with aggregate economic conditions can therefore help distinguish between alternative models of household behavior. Additionally, these estimates are useful for designing stimulus policies aimed at increasing aggregate consumption through expansions of consumer credit.

To our knowledge, there exists little empirical evidence regarding how the MPC varies over the business cycle.[2] In this paper, we estimate variation in the MPC out of liquidity between 2004 and 2011, covering years before, during, and after the Great Recession. We exploit sharp increases in credit limits generated by credit reporting rules in order to identify the MPC. The Fair Credit Reporting Act (FCRA) requires that the record or "flag" of a Chapter 7 bankruptcy

[1] See Parker 1999; Hsieh 2003; Stephens 2003; Kueng 2015; Gelman et al. 2015; and Baker and Yannelis 2016 for recent estimates of the marginal propensity to consume.

[2] Johnson, Parker, and Souleles (2006) speculate that the MPC may be larger during recessions. Jappelli and Pistaferri (2014) note that it is not "obvious how to extrapolate the distribution of the MPC estimated during a given year to other periods." Parker (2011) describes the substantial practical difficulties with estimating how the MPC varies across recessions and expansions.

be removed ten years after the bankruptcy is adjudicated.[3] Because bankruptcy flags are an input into credit-scoring models, former bankruptcy filers experience a discontinuous increase in credit scores when their flags are removed.

We study a sample of over 160,000 bankruptcy filers in the Consumer Financial Protection Bureau Consumer Credit Panel (CCP), a dataset that contains a 1-in-48 random sample of all consumers with credit records in the U.S. As a first stage, we estimate that bankruptcy flag removal increases consumer credit scores by roughly 15 points, from an average of 616 to 631. We find that this increase in credit scores results in a substantial increase in borrowing. The rate at which consumers open new credit accounts ("trade lines," in industry parlance) increases sharply starting at the flag removal date, and persists at a permanently higher level for at least five years. In the first year after flag removal, consumers borrow an additional \$290 on new credit cards, take out \$473 in new mortgages, and take out \$99 in new auto loans for each 10-point increase in their credit scores. Credit limits on new credit cards increase by \$778 per 10-point change in credit score, implying an MPC out of liquidity of 0.37. This result is broadly similar to the few previous estimates of the MPC out of liquidity for subprime borrowers.[4]

Throughout the paper, we interpret the ratio of the increase in borrowing over the increase in credit limits on new credit cards as an estimate of the MPC out of liquidity. This interpretation assumes that the increase in credit scores affects borrowing only through credit limits, and that the resulting increase in borrowing represents an increase in consumption. Although our estimates should technically be termed a "marginal propensity to borrow" (MPB), we follow Gross and Souleles (2002) in describing our measure as an MPC. We develop a theoretical model that clarifies the conditions under which the MPC is equal to the MPB, and argue that these conditions are likely good approximations to reality in the population of low-credit-score consumers we study.

Our key contribution is to estimate how the MPC out of liquidity evolves over the business cycle. To do so, we split the sample into cohorts whose flags were removed in each year

[3] FCRA 15 U.S.C. § 1681c. The record of a Chapter 13 bankruptcy is removed 7 years after adjudication. In this paper, we focus on Chapter 7 bankruptcy flags, since over two-thirds of bankruptcies are Chapter 7 and the 7-year rule for Chapter 13 bankruptcies coincides with the time when other delinquencies are removed from consumers' records.

[4] Agarwal et al. (2015) estimate an MPC of 0.55 for consumers with credit score under 660 and 0.45 for those with credit scores between 661 and 700 in the first year after origination.

from 2004 through 2011, and estimate the change in credit card limits and balances for each cohort. Based on this approach, the MPC out of liquidity increased from 0.34 in 2004 to a peak of 0.46 in 2008, followed by a drop back to 0.38 by 2011. These results are consistent with liquidity constraints being significantly more likely to bind during recessions than in prior or subsequent years. Consistent with previous studies, we find that the MPC varies little with income, is negatively correlated with credit scores, and is positively correlated with utilization and balances. As additional support for the interpretation that the MPC is higher when macro-economic conditions are poor, we find that the MPC also increased during the 2001 recession, and is positively correlated with the local unemployment rate.

We carry out several additional analyses to assess threats to the validity of our results. As described above, our approach assumes the exclusion restriction that flag removal affects borrowing only through its effect on credit limits. This assumption would be violated if flag removal also changed interest rates, and the increase in borrowing was driven instead by the change in prices. We develop a simple model to formalize this intuition and to derive a formula for the size of the bias due to changes in prices. Using an estimate of the price elasticity from Gross and Souleles (2002) and our own estimate of the derivative of price with respect to credit score from the Mintel Comperemedia database, we find that the bias in the MPC from price changes is roughly one percentage point, and varies little over the business cycle. Thus, while the price of credit is almost certainly affected by flag removal, in practice this bias has little impact on our results.

A second threat is that the change in borrowing after flag removal reflects changes in demand for credit, not changes in supply as manifest by higher credit limits. This relates to the question of whether consumers anticipate flag removal, which affects the interpretation of our estimates relative to theory. First, we reason that if consumers anticipate flag removal, then we should see a drop in credit activity in the months prior to flag removal, as consumers wait for better terms when their credit scores rise. We find no such evidence, and instead see a smooth trend of borrowing activity followed by an immediate jump at the time of flag removal. Second, we note that in order for our results to be driven by anticipatory effects, consumers must know and react to the timing of flag removal within a one-year window. We fielded an online survey to a sample of 187 past bankruptcy filers to assess this possibility. Based on the self-reported date of their most recent bankruptcy filing, only 9.2 percent of recent Chapter 7 filers correctly reported the year that their bankruptcy flags would be removed, only slightly better than chance. We conclude from these two pieces of analysis that our results are unlikely to be driven by anticipatory behavior, and interpret the estimated MPCs as resulting from an unexpected, permanent increase in borrowing limits.

We next examine two alternative explanations for the changes in the MPC over the business cycle: 1) that they are an artifact of changes in credit supply and 2) that they are driven by compositional differences across cohorts. The first explanation is premised on the observation that the effect of flag removal on credit limits (i.e. the denominator of the MPC) changes over the course of our sample period, reflecting variations in supply. However, while the effect on credit limits decreases from 2004-2009 and then remains flat afterward, the MPC follows and inverse-u pattern. Formalizing this intuition, we conduct an exercise that "partials out" the effect of the changing first-stage in credit limits from the MPC, and find that our main result is unaffected. To address potential compositional differences across cohorts which could drive heterogeneity in the MPC, we conduct a reweighting exercise following DiNardo, Fortin, and Lemieux (1996). In particular, we re-weight the sample in each year to match the 2008 flag removal cohort along a vector of observable characteristics including credit score and balances. Again, this exercise has little effect on our main findings.

Finally, we analyze additional heterogeneity in the MPC and measure the long-run effects of flag removal. Consistent with previous studies, we find that the MPC varies little with income, is negatively correlated with credit scores, and is positively correlated with utilization and balances. To study the longer-run effects of flag removal, we extend our main results out to five years following bankruptcy flag removal. We find that the increase in credit scores persists – virtually unchanged – for at least five years following bankruptcy flag removal. Similarly, the flow of new credit is permanently higher at the five-year horizon. These results must be interpreted cautiously since the event-study econometric approach extrapolates based on pre-existing linear trends, which may be less reliable in the long run. Still, the longer-run effects support our interpretation that bankruptcy flag removal causes a persistent increase in consumer credit scores, which in turn increases the availability of consumer credit for at least several years.

Interestingly, we find no evidence that the increase in credit usage following flag removal causes an increase in delinquencies, collections inquiries, or collections trades.

This paper's empirical strategy is similar to recent work that has studied the removal of negative information on consumer credit reports in the U.S. and Sweden (Musto 2004; Elul and Gottardi 2015; Bos, Breza, and Liberman 2015; Cohen-Cole, Herkenhoff, and Phillips 2016; Dobbie et al 2016), though, to our knowledge, no previous studies have exploited flag removal to estimate the MPC out of liquidity and how it varies over the business cycle. The paper is also related to the macroeconomic literature on the effects of credit on consumption. When recessions are caused by financial crises, the sharp drop in bank lending and consumer credit can exacerbate and prolong the economic downturn (Bernanke and Gertler 1989; Kiyotaki and Moore 1997; Eggertsson and Krugman 2012, Guerrieri and Lorenzoni 2015). Consistent with these models, Ludvigson (1999) estimates the effect of consumer credit on aggregate consumption and finds a strong relationship in the macroeconomic time series.

Few studies, however, have been able to identify and quantify the effects of credit supply shocks on consumption using detailed microeconomic data.[5] Most closely related to our paper are works by Gross and Souleles (2002), Agarwal et al. (2015), and Aydin (2016), who study the MPC out of liquidity by exploiting sharp variation in credit card limits. The pooled MPC out of liquidity that we measure is similar to that in Agarwal et al. (2015), the only study that presents estimates for subprime customers in the U.S. This paper is distinguished from the prior literature by our focus on variation in the MPC over the business cycle. Our paper complements recent model-based estimates of how the MPC varies over the business cycle (Carroll et al. 2015; Kaplan and Violante 2014).

Our estimates are likely to be informative about the MPC out of liquidity for the population of subprime borrowers with relatively low credit scores. This group exhibits a high MPC and is likely to be particularly responsive to expansionary policy, so it is an important subgroup to study. Future calibrations can use our estimates to extrapolate from bankruptcy filers to other groups, and to explore macroeconomic models that allow for general equilibrium responses. As

[5] Exceptions include work by Bhutta and Keys (2016) and Mian, Rao, and Sufi (2013).

described by Parker (2011), macroeconomic models that are inconsistent with the microeconomic estimates of "state dependence" in this paper are unlikely to provide accurate evaluations of stimulus policy.

The remainder of the paper proceeds as follows. The subsequent section provides background on the institutional setting and credit bureau data we analyze. Section 3 presents our empirical approach and the basic assumptions that underlie it. Section 4 describes the main results. Section 5 concludes.

2. Background on Bankruptcy Flags and Credit Bureau Data

This study uses data from the Consumer Financial Protection Bureau Consumer Credit Panel (CCP). The CCP is a longitudinal, nationally representative panel of de-identified credit records from a major consumer credit reporting agency. The full dataset includes snapshots in September of 2001, 2002, and 2003, and the end of each calendar quarter from June 2004 through June 2014. In each snapshot, the CCP includes the complete credit record for each sampled consumer including public records (e.g. bankruptcies, civil judgments, and tax liens), credit inquiries, trade lines, and credit score.[6]

We exploit rules imposed by the Fair Credit Reporting Act (FCRA) governing how long bankruptcies can remain on consumers' credit records. According to 15 U.S.C. § 1681c, "Cases under title 11 [United States Code] or under the Bankruptcy Act that, from the date of entry of the order for relief or the date of adjudication, as the case may be, antedate the report by more than 10 years." While this rule imposes a ten-year limit on reporting for all consumer bankruptcies, consumer credit bureaus voluntarily remove the flags for Chapter 13 bankruptcies after seven years. Because the FCRA also imposes a seven-year limit on many other types of records that often occur around the time of bankruptcy filing – including civil judgments, collections, and credit delinquencies – the removal of Chapter 13 flags is confounded by other changes in consumers' credit reports. Thus, we restrict our study to Chapter 7 bankruptcies alone.[7]

[6] See Avery et al. (2003) for more information on consumer credit records.
[7] Because bankruptcy flags are removed based on bankruptcy chapter choice and filing date alone, our identification strategy is not subject to ex-post selection bias based on consumers' payment behavior or other outcomes subsequent to filing. However, our results only apply to the subset of consumers who file for Chapter 7, whose characteristics are different from those of the general population.

The public-records portion of the CCP includes the filing date and chapter of each bankruptcy filed by the consumers in the sample. To create our analysis sample, we collected the complete credit records from each snapshot of every consumer whose record included a Chapter 7 bankruptcy at any time. To account for the possibility that a given consumer has multiple bankruptcies on their credit record during the sample period, we define the "index bankruptcy" as the first observed bankruptcy for each consumer. While we do not observe the date of bankruptcy adjudication, which typically occurs shortly after filing, flags are almost always removed between 117 and 118 months after the filing date, slightly earlier than the ten years required by the Fair Credit Reporting Act.[8] We define the date of bankruptcy flag removal as 117 months after the filing date for each bankruptcy. We define our sample (the "bankruptcy flag sample") as all consumers in the CCP whose index bankruptcy was a Chapter 7 filing, and whose flag for the index bankruptcy was removed between 2004 and 2011.[9]

Table 1 presents summary statistics for the paper's main sample and for comparison, a one-percent random sample of consumers in the CCP.[10] For the bankruptcy flag sample, we present summary statistics for the quarter before their flag is removed. The average consumer in the flag sample has 1.3 total bankruptcies observed on their credit records at any point between 2001–2014, which includes bankruptcy filings between 1991–2014 for Chapter 7 and 1994–2014 for Chapter 13. Consumers in this sample have an average credit score of 616, 4.8 open accounts, $76,000 in balances, and $85,000 in credit limits and original principal on open accounts in the quarter before flag removal. As compared to the overall CCP data, consumers in the flag sample have credit scores that are 80 points lower, 14 percent lower credit limits and principal, and similar levels of overall balances.

The last panel of Table 1 presents sample statistics on credit inquiries, collections trades, and delinquencies. The average consumer has 0.5 credit inquiries in the quarter prior to bankruptcy flag removal. Credit inquiries reported in our dataset are a subset of formal applications for credit made by consumers, which generate "hard pulls" of credit reports. While these post-

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[8] This timing is consistent with Musto (2004), who finds the flag removals occur between 9.5 and 10 years after the discharge date, which is typically on the same day or shortly after the filing date.

[9] Since this sample represents bankruptcy filings between 1994 and 2001, it is unaffected by compositional changes in the filing population caused by the Bankruptcy Abuse and Consumer Protection Act, which occurred in 2005.

[10] While the majority of U.S. adults have credit bureau records, the CCP sample differs from the general U.S. population in that younger consumers, minorities, and lower-income consumers are less likely to have credit records. See Brevoort et al. (2015) for more details.

bankruptcy consumers have relatively little debt in collections accounts, 4 percent of their open accounts are 90 or more days delinquent. By contrast, randomly selected borrowers have fewer inquiries, less debt in collections, and fewer delinquencies.

As a whole, consumers in the bankruptcy flag sample have significantly lower credit scores and higher delinquency rates than in the CCP. However, their overall credit profiles are remarkably similar. One key dimension of difference is that the credit card utilization in the quarter before flag removal is higher than utilization among consumers in the general CCP sample. Dividing credit card balances by limits, utilization after flag removal is 46 percent on average, compared with 20 percent in the CCP sample. By this measure, consumers in the bankruptcy flag sample are more likely to be credit constrained than the general population, but few of them are at their credit limit.

3. Empirical Approach

As documented below, credit scores increase sharply by roughly 15 points from a mean of 616 once a bankruptcy flag is removed from a consumer's record.[11] Our goal is to study this event and to use it to estimate the causal effect of an increase in credit supply on consumer credit outcomes. This section describes our empirical approach for doing so. Section 3.1 describes our event study framework, which follows the approach taken by Dobkin et al. (2018) to study the effects of hospitalization on credit usage. Section 3.2 presents a simple theoretical framework to understand how the marginal propensity to consume relates to the marginal propensity to borrow. Section 3.3 discusses the identifying assumptions required by the event study framework, focusing on the potentially confounding effects of changes in prices. Section 3.4 explains one the details of a key measurement issue, the focus on new cards rather than all open credit cards.

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[11] This is an average effect for the bankruptcy flag sample, which includes consumers who experienced no change in their credit scores after flag removal. Although flags for the index bankruptcy are almost always removed within a few months of the date we define for bankruptcy flag removal, the existence of any public record on a consumer's record is treated as a discrete outcome in commonly used credit score models. Thus, consumers who have tax liens, subsequent bankruptcies, or other public records on their credit reports experience no change in credit score after flag removal for the index bankruptcy. Because of this, we present our main estimates in terms of the effects of 10-point changes in credit scores instead of the raw effects of flag removal, which can be affected by compositional differences in the fraction of consumers with other public records on their credit reports.

3.1 Event-Study Regressions

We first take a non-parametric, graphical approach. For each outcome y_{it} exhibited by bankruptcy filer i at calendar time t, we denote the months since bankruptcy flag removal as r_{it}. We estimate the following non-parametric event-study regression:

$$y_{it} = \gamma_t + \gamma_c + \sum_{\tau=-24}^{24} \delta_\tau \cdot I\{r_{it} = \tau\} + \epsilon_{it}.$$

Here, γ_t represents fixed effects for calendar time and γ_c represents fixed effects for each flag-removal cohort based on the year and month in which their flag was removed.[12] We include indicator functions for each of the 24 months before and after flag removal. We then plot estimates of δ_τ, the change in the outcome of interest over event time. This event-study approach describes the change in outcomes before and after flag removal with few parametric assumptions. Intuitively, the regression compares outcomes for consumers who just had their flag removed to outcomes for consumers who have yet to have their flags removed while differencing out the common effect of calendar time and level shifts across cohorts.

For most of our key outcomes, calendar time t denotes calendar month. We observe the "flow" of new credit at a monthly level, since the data include the exact calendar date that each new account is opened. For these outcomes, our approach above is straightforward, and we can include both calendar month and event month dummies in the regression. However, for a few outcomes, such as the existence of a bankruptcy flag, credit score, and the current balance and credit limit on open accounts, we only observe the data at a quarterly level since these outcomes are based on a "snapshot in time" of current credit circumstances, and our data only include snapshots on a quarterly basis.

Nonetheless, even for the "snapshot" variables, we are able to estimate event time coefficients at monthly rather than quarterly intervals. The reason is the following. We can divide our sample into three cohorts consisting of consumers who lose their bankruptcy flags during

[12] Note that we face collinearity between time since bankruptcy, calendar time, and bankruptcy filing cohort. Intuitively, as a filing cohort proceeds through time, both time since bankruptcy and calendar time increase at the same rate. This is a standard age-time-cohort problem in event-study research designs (Borusyak and Jaravel, 2016). We address this with an empirical model that assumes that pre-existing trends in event time can be well-approximated by a linear time trend. This is similar to the identifying assumption in Dobkin et al. (2018).

the first, second, and third month of each calendar quarter. We observe the "snapshot" outcomes for each cohort in March, June, September, and December of each year. Even though each cohort is only observed four times per year, we can track every month of "event time," since those with flag removals in the first month of the quarter are observed in months -4, -1, 2, …; those with flag removals in the second month are observed in months -5, -2, 1, …; etc. This is a similar setup to that of Dobkin et al. (2018), where the credit report data are observed once each year, but the authors have precise dates of hospital admissions that allow them to measure event time in months before and after hospital admission. In summary, for the "snapshot in time" variables, our specification includes calendar quarter and event month fixed indicators.

Given this setup, we impose the normalization that the event time indicator variables representing 24, 23, and 22 months prior to flag removal are equal to each other and that the indicator variable representing 1 month prior to flag removal is equal to zero. These restrictions are necessary to address the fundamental under-identification problem in event-study designs, as discussed formally by Borusyuk and Jaravel (2016). Because each month relative to flag removal is observed for only one of the three month-of-quarter cohorts described above, we have to normalize three distinct event-time indicator variables and pool them to estimate the rest of the event-study coefficients.[13] As in Dobkin et al. (2018), we have experimented with many alternative normalizations, and they have all led to similar results.

A drawback to this overall approach is that it does not control for trends that depend on the time elapsed since bankruptcy. Bankruptcy represents a dramatic event in the financial lives of consumers during which the majority of their debt is absolved, causing a sharp and immediate decrease in their credit scores. Over time, post-bankruptcy consumers gradually accumulate new credit, and their financial health improves (Han, Keys, and Li 2013; Jagtiani and Li 2014). These dynamics cause overall credit usage to exhibit trends prior to bankruptcy flag removal, and we document below that the trends are roughly linear for most outcome variables. Since the timing of flag removal occurs at the same time relative to bankruptcy filing for all consumers, the non-parametric event study cannot account for such trends. To account for pre-

[13] We also impose this restriction for both the "flow" and "snapshot in time" variables for consistency, even though it is not strictly necessarily for the flow variables since we observe each cohort in every month. Our results are unchanged if we omit this restriction for the flow variables.

trends, we augment the approach above by using a parametric event-study regression that controls for a linear pre-existing time trend.

The parametric event-study regression we estimate is the following:

$$y_{it} = \gamma_t + \gamma_c + \alpha \cdot r_{it} + \sum_{\tau=0}^{24} \delta_\tau \cdot I\{r_{it} = \tau\} + \epsilon_{it}.$$

There are two differences between this regression and the more flexible specification above. First, this specification includes the term $\alpha \cdot r_{it}$, which captures the pre-flag-removal trend in outcomes. Second, we only estimate the lagged effect of flag removal $(\tau > 0)$. The coefficients of interest are the effects of flag removal at different horizons: δ_τ. Those estimates describe the change in consumers' outcomes relative to what one would predict given their pre-flag-removal trend.

In the absence of pre-existing time trends, this parametric approach leads to identical estimates as the non-parametric specification above. But in the presence of pre-trends, this specification can recover the effect of flag removal relative to what one would expect if the pre-trends were to continue. Thus, the parametric approach explicitly captures the comparison we seek to make: the difference between consumers' post-flag-removal outcomes and the counterfactual outcomes we would expect if their flags hadn't been removed, given their pre-flag-removal trajectories.

We scale these reduced-form estimates of the effect of bankruptcy flag removal by the first-stage effect of bankruptcy flag removal on credit scores. This scaling makes it easier to interpret the results as the effect of a ten-point change in credit scores, which has a more direct practical meaning than the removal of a bankruptcy flag itself. To achieve that scaling, we jointly estimate the first-stage effect of bankruptcy flag removal on credit scores and the reduced-form effect of flag removal on the outcome of interest using seemingly unrelated regression (SUR). We then compute the ratios of the reduced-form and first-stage coefficients at various months after flag removal. The SUR framework allows us to easily compute standard errors for these scaled estimates using the delta method. The tables that follow present estimates that describe the change in credit outcomes per 10-point increase in credit scores.

A final detail is that many of the outcomes we study are flows rather than stocks, and we seek to measure the cumulative effect of flag removal on these variables over different horizons. For instance, we estimate the number of new accounts opened in the first 6 months due to flag removal as the sum of the first six event-study estimates: $\hat{\delta}_1 + \hat{\delta}_2 + \hat{\delta}_3 + \hat{\delta}_4 + \hat{\delta}_5 + \hat{\delta}_6$. To calculate the MPC out of liquidity, we divide the effect of flag removal on new credit card balances by its effect on new credit card limits. Formally, for horizon r relative to flag removal, we define

$$MPC(r) \equiv \frac{\sum_{j=1}^{r} \hat{\delta}_j^{balances}}{\sum_{j=1}^{r} \hat{\delta}_j^{limits}}.$$

We jointly estimate the effects on balances and limits using SUR, and calculate the associated standard errors (clustered by flag-removal cohort) using the delta method.

To measure the MPC out of liquidity across the business cycle, we estimate the following regression:

$$y_{it} = \gamma_t + \gamma_c + \sum_{j=2004}^{2011} I\{J_i = j\} \cdot \left[\alpha_j \cdot r_{it} + \sum_{\tau=0}^{24} \beta_{j,\tau} \cdot I\{r_{it} = \tau\}\right] + \epsilon_{it}.$$

Here, we denote the year that consumer i had their flag removed as J_i. This approach allows us to estimate *p*-values associated with a test of the null hypothesis that consumers exhibit the same MPC out of liquidity each calendar year. That is, we test whether $J_{2004} = J_{2005} = \cdots = J_{2011}$.

3.2 MPC versus MPB

Throughout the paper, we refer to the ratio of the effect of flag removal on credit card balances and credit limits as the marginal propensity to consume out of liquidity (MPC), following the terminology used by Gross and Souleles (2002). In that paper and much subsequent work, it is often not made clear what assumptions are needed for the marginal propensity to borrow out of an exogenous shock to credit limits (MPB) to be a valid proxy for the MPC. In this section, we develop a simple economic model to derive the conditions under which these two objects are the same, and when one is an upper or lower bound for the other. We then conclude with a discussion for why we believe the two objects are likely to be similar in our empirical setting when viewed through the lens of our model.

We assume that an individual has exogenous income y and chooses a borrowing amount b subject to an exogenous credit limit φ. The individual chooses liquid checking account balances m, which are used to pay for consumption c_1. The remaining liquid balances at the end of the period are defined as $n = m - c_1$. The remaining resources not including checking account balances $(y + b - m)$ are used to pay for consumption c_2. Both types of consumption occur in the same period. One interpretation of this setup is that there are certain expenses, such as rent for an apartment, that must be paid in cash out of a checking account, while remaining expenses can be either paid in cash or charged to a credit card. The future utility cost of credit-card borrowing is represented by the function $U(b; r, \varphi)$, where r is the interest rate on credit-card borrowing, and the future utility benefit of maintaining liquid checking account balances is given by $V(n; r, \varphi)$. Consumer utility is given by $u(c_1, c_2) + \beta\big(U(b; r, \varphi) + V(n; r, \varphi)\big)$, where β is the discount rate. Given this setup, the consumer's optimization problem is the following:

$$\max_{c_1, c_2, b, m, n} u(c_1, c_2) + \beta\big(U(b; r, \varphi) + V(n; r, \varphi)\big) \quad s.t.$$

$$c_1 \leq m - n, c_2 \leq y + b - m, b \leq \varphi$$

We use stars to denote the optimal solution to the above program, and we assume an interior solution throughout this section. We define the MPB as $db^*/d\varphi$ and the MPC as $dc^*/d\varphi$, where $c^* = c_1^* + c_2^*$.[14] It is straightforward to show from the budget constraint that $dc^*/d\varphi = db^*/d\varphi$ if and only if $dn^*/d\varphi = 0$. This corresponds to one of two scenarios. The first scenario is when $V(n; r, \varphi) = 0$. In this case the consumer has no demand for maintaining liquid balances between periods, so $n = 0$ and thus $c_1 = m$. As a result, $c_1^* + c_2^* = y + b^*$ at the optimum, and we immediately conclude that $db^*/d\varphi$ must equal $dc^*/d\varphi$. The second scenario is $V(n; r, \varphi) \neq 0$ but $dn^*/d\varphi = 0$. Given the assumption of an interior solution, this

[14] We note that the MPC out of liquidity need not be equal to MPC out of permanent (or transitory) income. The Online Appendix of Guerrieri and Lorenzoni (2017) derives the relationship between these two concepts in a simple dynamic model.

scenario requires that $\partial V/\partial\varphi < 0$. This is a "knife-edge" case, where the direct effect and precautionary-savings effect described below exactly offset each other. In this case, MPC = MPB, even with an endogenous demand for liquid savings.[15]

We next describe conditions under which the MPB is either an upper or lower bound for the MPC. If $\partial V/\partial\varphi = 0$ (but $V \neq 0$ and $\partial V/\partial n > 0$), then the first-order conditions of the optimal solution imply that an increase in φ will increase c_1, c_2, and n all together. Thus, $dc^*/d\varphi < db^*/d\varphi$ since $\frac{dc^*}{d\varphi} + \frac{dn^*}{d\varphi} = db^*/d\varphi$ and $\frac{dn^*}{d\varphi} > 0$. As a result, in this case, we will have MPC < MPB, with the magnitude of the gap equal to $\frac{dn^*}{d\varphi}$. Intuitively, an increase in the borrowing limit spurs consumers to increase both types of consumption as well as liquid checking account balances. In this case, the MPB is an upper bound on the true MPC because some of the increased borrowing is used to increase liquid checking account balances that can be drawn down in the future (rather than spent on consumption today).

Alternatively, it might be the case that demand for liquid savings falls with φ (so that $\partial V/\partial\varphi < 0$). This could arise in a situation where part of demand for n is a precautionary savings motive, and an increase in φ reduces demand for precautionary savings. In this case, it's possible that this precautionary savings effect is sufficiently strong so that $\frac{dn^*}{d\varphi}$ is negative, and so now the MPB is a lower bound instead of an upper bound (i.e., MPB < MPC). Intuitively, demand for self-insurance falls with availability of credit card borrowing, and consumers react to increase in credit limits by reducing liquid savings buffer stock and the actual increase in consumption in the current period is greater than the increase in borrowing.

How does this model apply to our empirical setting? Since our focus is on low-credit-score consumers with low average liquid savings, we do not expect liquid savings to be very responsive to exogenous changes in the credit limit. For example, nearly half of U.S. consumers report having less than $400 in excess liquid savings (Fed 2016). Thus, we expect that n^* is both low on average and not very responsive to changes in credit limits in our sample, meaning that

[15] All of these results hold regardless of the interest rate *r*, but we assume that the interest rate is held constant since we focus on an exogenous change in credit limits. If the interest rate changes at the same time as the change in credit limit, then this will cause bias in both the MPB and MPC. This is a conceptually distinct issue in interpreting the reduced-form results, and is discussed below in Section 3.3.

the MPB should be a good approximation of the MPC. We maintain this assumption throughout our analysis. Future work on data that contains both credit card borrowing and liquid savings may be able to assess the validity of this assumption directly, but we think it is a reasonable approximation for our sample population.

3.3 Flag Removal and Interest Rates

In order to interpret our results as estimates of the MPC out of liquidity, we must assume that the change in credit scores induced by bankruptcy flag removal affects borrowing only through the change in credit limits. This exclusion restriction would be threatened if flag removal also affects interest rates, which is plausible given the increase in credit scores. In this section, we develop a simple econometric model that formalizes this concern, and describe a calibration exercise to estimate the potential size of the bias due to violations of the exclusion restriction.

To understand the bias due to interest rate changes more concretely, consider the following econometric model. An individual i borrows B_i in response to their credit limit (L_i) and the interest rate (r_i) they face:

$$B_i = \alpha + \beta L_i + \delta r_i + e_i.$$

Here, $\beta = \partial B/\partial L$ represents the marginal propensity to consume out of liquidity and $\delta = \partial B/\partial r$ represents the effect of changes in interest rates on borrowing.

To identify the MPC out of liquidity, we need to isolate variation in credit limits holding the interest rate constant. Consider an instrumental variable Z_i based on bankruptcy flag removal.[16] That instrument changes credit scores and thus increases credit limits. Flag removal may also decrease offered interest rates, which in turn affects borrowing. To explore the potential bias in the estimated MPC out of liquidity, consider the total effect of a change in credit score on borrowing:

$$\frac{dB}{dZ} = \frac{\partial B}{\partial L} \cdot \frac{dL}{dZ} + \frac{\partial B}{\partial r} \cdot \frac{dr}{dZ} = \beta \cdot \frac{dL}{dZ} + \delta \cdot \frac{dr}{dZ}.$$

[16] It is possible that lenders may react to the removal of the bankruptcy flag itself in addition to the change in credit scores. This does not affect the validity of the natural experiment, however, since we only rely on bankruptcy flag removal as an instrument for a change in credit limits. If bankruptcy flags enter into credit pricing functions independently of credit scores, then a similar logic as that described in this section applies to the calibration of the potential bias due to price effects.

The ratio $\frac{dB}{dZ} / \frac{dL}{dZ}$ will be an unbiased estimate of the MPC out of liquidity (β) if either Z has no effect on the interest rate $(\frac{dr}{dZ} = 0)$ or there is no response of borrowing to changes in borrowing costs ($\delta = 0$). If neither of these are the case, then the estimated MPC out of liquidity will be biased:

$$\widehat{\frac{dB}{dZ}} \Bigg/ \widehat{\frac{dL}{dZ}} = \beta + \delta \cdot \frac{dr}{dZ} \Bigg/ \frac{dL}{dZ}.$$

In this context, it is straightforward to sign the bias term. Consumers borrow more in response to lower interest rates ($\delta < 0$), flag removal lowers the offered interest rates ($dr/dz < 0$), and flag removal raises offered credit limits ($dL/dZ > 0$). As a result, the ratio above will be an over-estimate of the true MPC.

How large is this bias? Unfortunately, we are not able to address this concern in the CCP data because it does not include interest rates. Since we do not possess a panel dataset that includes both interest rates and bankruptcy flags, we instead estimate the change in interest rates per unit change in credit scores in a sample of credit card mail offers collected by Mintel Comperemedia. Mintel is a market research firm that maintains a sample of several thousand nationally representative respondents, and collect the details of all credit card mail offers from a sample of respondents in monthly cross sections. The mail offers are merged to credit bureau records that include the credit scores of each individual who receives credit card offers. See Han, Keys, and Li (2013) and Ru and Schoar (2016) for more details on the Mintel data.

The Mintel dataset is particularly relevant because we focus on new credit cards for estimating the MPC in our main results, as we describe in detail in the next section. Since many new credit cards include low introductory-rate offers, we focus on the weighted average of the introductory rate and the regular purchase interest rate. We weight these two interest rates by the fraction of months during the first year that the introductory rate applies, to match our focus on the MPC on new credit cards during the first year after origination.[17]

[17] Although the weighted-average APR is lower than the regular APR, the slope of the relationship between credit scores and interest rate is similar if we use the regular purchase APR alone.

For our analysis, we start with the universe of 921,198 credit card acquisition offers mailed to consumers between 2004 and 2011 in the Mintel sample. We then estimate the slope of the relationship between interest rates and credit scores in the range between 600 and 700, which covers the credit scores of most consumers in the flag-removal sample. Appendix Figure A1 presents the results of this analysis. For the pooled sample, a 10-point increase in credit scores is correlated with a 41-basis-point drop in the weighted-average APR (standard error 0.007). This correlation is similar to those reported in previous studies (Agarwal et al. 2015, Han et al. 2015). That number and associated confidence interval suggest that, on average, the 15-point rise in credit scores generated by bankruptcy flag removal will lower offered interest rates by 60 to 64 basis points.

Gross and Souleles (2002) estimate the effect of changes to the interest rate on a borrower's utilization (their credit card debt divided by their limit).[18] Their estimate is -0.016 (standard error of 0.001), implying that a 10-basis-point decrease in the interest rate would increase utilization by 0.16 percentage points. We multiply this estimate by the change in the interest rate caused by flag removal, described above.[19] That exercise suggests that the drop in interest rates would lead to a long-run (6- to 12-month) increase in the debt-to-limit ratio of roughly 0.95 to 1 percentage point. This suggests a small bias relative to the pooled MPC of roughly 0.37.

Moreover, this bias likely varies relatively little over the business cycle. As shown in Panel B of Appendix Figure A1, a 10-point increase in credit scores is correlated with changes in interest rates that range from 15 to 55 basis points during our sample period. These estimates imply a bias in the pooled MPC ranging from 0.2 to 0.8 percentage points over the business cycle. This is a small share of the change in the MPC from peak to trough of 10 to 15 percentage points. We thus conclude that endogenous interest rates are unlikely to account for a meaningful share of the variation in the MPC over the business cycle.

3.4 New Cards versus Overall Borrowing

Throughout the paper, we focus on new credit cards rather than on all credit cards for estimating the MPC. We do so because our research design requires a high degree of precision

[18] See Table III, row 2, of Gross and Souleles (2002).

[19] The product of this estimate from Gross and Souleles (2002) and the likely change in interest rates approximates the bias term described above, $\delta \cdot \frac{dr}{dZ} / \frac{dL}{dZ}$.

in measuring the timing of credit activity relative to bankruptcy flag removal. The CCP includes the exact date that each new credit card is originated, allowing us to measure the rate of new account activity relative to the date of flag removal with very little error. In particular, we measure the balance and credit limit on each new card one year after origination, and compare these variables for cards originated before and after flag removal.[20] This procedure allows us to leverage the precise data on the timing of new account originations relative to the date of flag removal.

Unlike the date of account opening, the timing of account activity is measured more noisily for existing credit cards. Reporting lags vary widely and idiosyncratically across accounts and creditors. As a result, the credit profile reported in any given quarter actually reflects past activity over a range of time periods, depending on the reporting lag for each account.[21] To be concrete, consider the following simplified example. Consider a consumer with two open accounts as of January 2010. Lender A reports the consumer's accounts on a monthly basis, so the balance and credit limit in credit bureau data would be current as of January 2010. Lender B, however, only makes sporadic reports to the credit bureau, so that account's balance and credit limit reflects the consumer's borrowing status from six months earlier, as of June 2009. Thus, while the credit limits are likely to remain relatively stable, the consumer's balances across both accounts as of January 2010 would reflect a mix of current and lagged information.

As a further complication, lenders often fail to report the closure of accounts. It is often impossible to distinguish reporting lags from account closures, and balances and credit limits may remain on a consumer's report and artificially inflate their credit activity even though the accounts have already been closed as of the data snapshot date. By focusing on new accounts, which are unlikely to be closed within twelve months of origination, we minimize measurement errors due to both reporting lags and failures to report account closures.

To make sure that our focus on new accounts does not affect our overall conclusions, we have estimated the MPC both for new accounts and open accounts, and we describe those

[20] We emphasize that each new account appears in the MPC calculation only once. We observe balances for each credit card 12 months after account origination, and link those balances to the account's contemporaneous credit limit.

[21] Brevoort, Grodzicki, and Hackman (2017) use the same dataset we do, and write that "often times there are significant lags between when debts are acquired and when they are reported to the NCRCs, though the delay does not affect the reported trade line's opening date."

estimates in Section 4.2 below. The estimates for overall borrowing are substantially noisier and somewhat attenuated relative to those for new borrowing, as expected from the aforementioned measurement error. While the 6-month MPC using new accounts in our pooled sample is 0.37 with a standard error of 0.01, the same estimate using open accounts is 0.24, with a much larger standard error of 0.09. Moreover, the estimates based on open accounts are more sensitive to alternative specifications and controls than the paper's main estimates. That said, the MPC we estimate using new credit cards is reassuringly similar in magnitude to that for all open accounts, consistent with prior evidence that expansions of new credit are fully passed through to total borrowing for subprime consumers (Agarwal et al. 2015).

4. Effects of Bankruptcy Flag Removal

This section presents our main empirical estimates. We first study the effect of bankruptcy flag removal on credit scores. We then estimate how the change in credit scores affects new borrowing, the MPC out of liquidity, and delinquency. Next, we describe how the MPC out of liquidity changes over the business cycle—we find that the estimated MPC rises during recessions. We then discuss heterogeneity in the MPC. In particular, we find that the estimated MPC is larger in regions with higher unemployment rates. The section concludes by discussing threats to validity and the long-run effects of flag removal.

4.1. Effect of Bankruptcy Flag Removal on Credit Scores

Figure 1 describes the effect of bankruptcy flag removal on credit scores. The first panel plots event-study coefficients when the existence of a bankruptcy flag is the dependent variable. The circular markers in the figure plot the means of the outcome of interest once flag-removal-cohort fixed effects and calendar-year-month fixed effects have been removed. The solid line in the figure plots the results of an OLS regression based solely on the pre-period event-study estimates. Reassuringly, the figure suggests a nearly deterministic relationship between the time since bankruptcy filing and the removal of the bankruptcy flag. The likelihood of having a bankruptcy flag on record decreases by precisely one between 116 and 118 months after bankruptcy filing.

The second panel of Figure 1 describes the effect of flag removal on credit scores.[22] There is a sudden, 15-point increase in credit scores that occurs instantaneously the month that the bankruptcy flag is removed, consistent with the fact that the bankruptcy flag is a direct input into credit scoring models.[23] Table 2 provides the numbers behind this figure. The table presents the estimated effects of bankruptcy flag removal on credit scores for the entire sample and also for flag removals in selected years. We present estimates of the effect over two different time horizons. The first row of estimates calculates the effect of bankruptcy flag removal by comparing the average credit score 6 months after flag removal to the predicted credit score based on the pre-flag-removal time trend. The second row of estimates calculates the effect in the same way, but 12 months after bankruptcy flag removal.

Overall, the table suggests an average 15-to-16-point increase in credit scores after flag removal. The effect is remarkably similar across time periods. For instance, we observe a 15.5-point increase in credit scores 6 months after flag removal for the pooled sample. The 12-month effect increases to 16.4 points for those who have their bankruptcy flags removed in 2011. The increase in credit scores after flag removal is statistically significant, with associated *p*-values well below one percent.

4.2. Effect of Flag Removal on Borrowing and Credit Limits

We next test how the change in credit scores affects the supply and usage of new credit. Figure 2 presents the effect of bankruptcy flag removal on outcomes that summarize the amount of new credit consumers receive as a result of flag removal. The figure depicts the average number, balances, and principal and credit limits on new accounts opened each month. Panel A shows a

[22] In some of the figures, the outcomes appear to follow three-month cycles. Those cycles are an artifact of the data construction and normalization. "Snapshot" outcomes such as credit score and number of open accounts on the credit record are only observed once per quarter, though the event-study specification involves point estimates for each month. The figures thus effectively overlay three separate cohorts of consumers depending on whether they filed for bankruptcy in the first, second, or third month of the quarter. Because some outcomes follow pre-trends and we normalize the first three coefficients of the event study to be equal, the normalization generates a slight offset across these three effective cohorts. This normalization has very little impact on the results.

[23] While a positive trend in credit scores is visible in the figure before and after flag removal, we are cautious about its interpretation. This specification does not allow us to separately identify the pre-trend, a full set of event-time indicator functions, a full set of calendar time dummies, and flag-removal-cohort fixed effects (Borusyak and Jaravel, 2016). We choose the specification with flag-removal-cohort fixed effects in order to most precisely estimate the MPC by year, but at the expense of not being able to interpret the slopes of the pre-trends in our outcome variables.

sudden and striking increase in the number of new accounts opened per month after flag removal. The rate of new account opening increases by about 0.03 per month, with increases of about $300 and $400 per month in the balances, principal, and limits on these new accounts.[24]

Table 3 presents the numbers behind these figures and also presents the analogous estimates for disaggregated product categories. The table presents IV estimates of the change in credit on new accounts per 10-point increase in credit scores. To measure the cumulative impact of flag removal on borrowing, we integrate the effects over new account openings during the first 6 and 12 months after flag removal.[25] In column 1, the table shows that for each 10-point change in credit score after flag removal, consumers opened 0.13 new accounts in the first 6 months and took on $489 in balances and received $927 in principal and limits on these new accounts. All in all, these results suggest a very clear increase in both credit supply and usage once bankruptcy flags are removed and credit scores rise.

We next probe how borrowing on different types of credit products responds to changes in credit score. Figure 3 shows the effects on new credit card accounts. It suggests that a large share of the increase in new accounts in Figure 2 is driven by credit cards. As shown in column 2 of Table 3, consumers take out 0.099 additional credit card accounts per 10-point change in credit score in the 6 months after flag removal, which comprises three quarters of the increase in all new credit accounts over the same period. Out of $411 in additional credit limits on these new credit cards, consumers take out $152 in additional balances. Those two estimates imply a marginal propensity to consume out of liquidity of 37 percent. Below, we calculate the MPC more formally and estimate how it changes across the business cycle.

Figure 4 presents results for two other types of credit: mortgages and auto loans. The figure suggests clear increases in both number of accounts and loan principal on new accounts for these types of loans, consistent with the results for credit cards and overall credit. The third

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24 In these summary measures, we include all types of credit accounts on consumer credit reports, including mortgages, auto loans, credit cards, and student loans. For open-ended revolving credit products such as credit cards and home equity lines of credit (HELOCs) we measure the total amount of credit extended by credit limits, and for closed-end products (e.g. mortgage and auto loans), we measure it by the principal amount of the loan.

25 By "integration" we mean that the estimates in Table 3 involve the summation of coefficients over either 6 months or 12 months. So, for instance, the estimated 6-month effect of flag removal on the balances on new accounts is the *sum* of the first 6 coefficients from the event-study specification when the total balance on new accounts opened in each month is the dependent variable divided by the estimated change in credit scores at 6 months.

and fourth columns of Table 3 present IV estimates for these products. Panel A suggests that the number of new mortgage and new auto accounts increase by much less than new credit card accounts, which is unsurprising given the size of these loans and the relative infrequency of large asset purchases. However, the small increase in new accounts leads to a statistically significant increase in new balances and new borrowing (Panels B and C). In the first 6 months after flag removal, consumers take out $155 in new mortgage principal and $40 in new auto loans per 10-point increase in credit scores.

A remaining question is whether this increase in borrowing simply represents re-financing of past loans or whether it represents novel borrowing. To answer that question, we apply the same research design to open credit card accounts instead of new accounts. If flag removal simply led to a shift in balances from existing cards to newly opened credit cards, then we would observe no change in open balances. Consistent with the idea that expansions in new credit pass through to overall borrowing for subprime consumers (Agarwal et al 2015), we find that the MPC using overall borrowing is similar to the MPC using new credit. However, the former is estimated with more error, as we expected given measurement issues discussed above. We estimate an MPC out of liquidity of 0.241 (standard error 0.086) using balances and credit limits on open accounts in the first six months after flag removal, and an MPC of 0.285 (standard error 0.054) in the first twelve months after flag removal, which are similar in magnitude but attenuated relative to our main results. These standard errors are much larger than those we report below using new credit cards, and would not allow us to rule out economically significant variation over the business cycle.

4.3. The Marginal Propensity to Consume Over the Business Cycle

We next estimate the MPC out of liquidity. Table 4 presents the estimated MPC for credit cards for the entire sample and for flag removals that occurred in each year. Panel A presents the estimated MPC while panels B and C present the components of the MPC: the change in credit card limits and credit card balances respectively.[26] Overall, we estimate an MPC of 0.37, suggesting that consumers borrow 37 percent of the increased credit card limits offered to them

[26] The MPC out of liquidity is defined as the change in balances divided by the change in limits.

once their bankruptcy flags are removed. That estimate is similar to previous estimates for subprime borrowers (Agarwal et al. 2015).

The remaining columns of Table 4 present the estimated MPC for each flag removal cohort. In addition, Figure 5 presents the estimates for all years graphically to assess the overall pattern of MPC estimates over time. Both the figure and Table 4 suggest a clear inverse-U-shaped pattern during the sample period. The estimated MPC based on the first six months after flag removal remained fairly constant between 0.33 and 0.35 from 2004 to 2006. The MPC then rose, ranging from 0.41 to 0.46 in the three subsequent years, peaking in 2008 during the depths of the Great Recession. In the two final years of the sample, the MPC declined back to 0.35 to 0.38, close to pre-recession levels. While our earlier results show that consumers take up significant amounts of new credit between 6 and 12 months after flag removal, both the estimated MPC and the pattern over the business cycle are remarkably consistent across these two different measurement periods.

Panels B and C of the table and graph decompose the change in MPC into changes in credit limits and changes in borrowing. The results show that, in contrast to the inverse-U-shaped pattern in the MPC, the change in credit limits following flag removal decreased dramatically between 2004 and 2011. This pattern suggests a substantial contraction in the supply of unsecured credit for subprime consumers which failed to recover after the recession.[27] If the increase in MPC between 2004 and 2008 were simply a mechanical effect of the decline in credit supply, we would expect the MPC to continue to increase or at least remain elevated from 2008 to 2011. Instead, we find that the MPC declined after the Great Recession, suggesting that these results reflect a change in the credit constraints faced by consumers instead of purely the mechanical effect of changes in credit supply. We investigate this more formally in a robustness analysis, below.

[27] While all types of consumer credit contracted after the financial crisis, different markets have seen various degrees of recovery. As of 2013, near the end of our sample period, mail offers and originations for subprime credit cards were still substantially below pre-crisis levels. That could be due to a combination of deteriorations in consumer credit quality, shocks to bank balance sheets, tightened regulation and capital requirements, and changes in consumer demand. See NY Fed Household Debt and Credit Report (2016), Agarwal et al (2015), and Han, Keys, and Li (2014).

4.4. Heterogeneity in the MPC

We next study how the estimated MPC out of liquidity varies across different cuts of the sample. Table 5 presents estimates of the MPC stratified by consumers' ex-ante credit score, median household income based on census tract, and ex-ante credit card utilization. The first panel suggests a monotonic pattern by credit score: those with higher credit scores before flag removal exhibit a lower MPC out of liquidity. That pattern is consistent with the work of Agarwal et al. (2015). The second panel suggests no clear pattern between the MPC and household income, consistent with Gross and Souleles (2002). The bottom panel of Table 5 suggests that those with higher pre-flag-removal utilization exhibit a higher MPC out of liquidity. Surprisingly, those with relatively low utilization before flag removal also exhibit an MPC that is statistically distinguishable from zero. Both of those patterns are consistent with the work of Gross and Souleles (2002) and Aydin (2016). Appendix Table A1 presents stratifications for an alternative measure of utilization based on credit line (limit minus balance), and finds similar results.

Table 6 presents stratifications based on the unemployment rate in consumers' county of residence. We stratify counties by two measures of the income shock experienced during the Great Recession: counties' peak unemployment rate and average unemployment rate during the period between 2004 and 2011, matching the years of our main sample. This stratification is meant to test whether harsher macro-economic conditions lead to higher MPCs not only in the time series, but also in the cross section. The results suggest that a higher local unemployment rate is indeed associated with a higher MPC out of liquidity. Both panels present estimates of the MPC that are monotonically increasing in the county unemployment shock. For instance, the 6-month MPC estimate for counties that experienced an annual unemployment rate higher than 10.9 percent is 0.42, whereas the corresponding estimate for counties with peak unemployment below 8.7 percent is only 0.35.

Table 6 thus provides further evidence that the MPC out of liquidity is state dependent. Table 4 presents evidence of state dependence *over time*: the MPC rises during recessions. Table 6 presents complementary evidence *across space*: a higher MPC in counties with larger unemployment shocks. The similarity across these approaches suggests genuine state dependence in the MPC out of liquidity, likely stemming from more-binding liquidity constraints when economic conditions are poor. Any alternative explanations would have to account for the patterns in the MPC both over time and across space.

4.5. Robustness Analysis and Threats to Validity

We next probe whether the analysis above credibly isolates the change in the MPC out of liquidity in response to poor macro-economic conditions. In particular, we test three alternative interpretations of the results: (1) that the changing MPC over time is driven by the changing magnitude of the effect of flag removal on credit limits, (2) that the changing MPC is driven by compositional differences across flag-removal cohorts, and (3) that the results are unique to the Great Recession.

We first address whether the pattern in the MPC over time is caused by changes in the effect of flag removal on credit limits, which is decreasing over time. This would complicate our interpretation if consumers borrow differently out of small credit limit increases than large ones.[28] To investigate this possibility, we pursue the following empirical strategy, designed to "partial out" changes in the credit limits from the MPC.

First, we obtain an estimate of the MPC each year and also an estimate for each year of the increase in credit limits after flag removal. We then regress the MPC each year on the change in credit limits we observe that year. The residuals of that regression represent the MPC we would observe each year once we have "partialled out" the effect of changes in credit limits on the estimated MPC. If these residuals still show a higher MPC during the recession than during surrounding years, then this suggests that our results are not driven simply by the changing effect of flag removal on credit limits over time.

Concretely, suppose that, using the same notation as above, we estimate the 12-month MPC for each year *t*: $MPC_t = \frac{\sum_{j=1}^{12} \hat{\delta}_{j,t}^{balances}}{\sum_{j=1}^{12} \hat{\delta}_{j,t}^{limits}}$. We also estimate the effect of flag removal on credit limits alone, the denominator of the MPC: $Lim_t = \sum_{j=1}^{12} \hat{\delta}_{j,t}^{limits}$. We run a simple, OLS regression of the former on the latter: $MPC_t = \alpha_0 + \beta \cdot Lim_t + \epsilon_t$. That regression, based on

[28] For example, the model of Kaplan and Violante (2014) predicts a non-monotonic relationship between increases in credit limits and the MPC, with the total effect depending on the fraction of wealthy versus poor hand-to-mouth consumers.

only as many observations as years in the data, allows us to account for the change in the MPC over the business cycle that is driven solely by changes in limits.

Appendix Figure A2 plots those residuals, which have been scaled to represent the counterfactual MPC that would have been observed each year if flag removal had the same effect on credit limits each year. The figure still suggests an inverse-U-shaped pattern, with the observed MPC peaking during the Great Recession. This suggests that differential effects of flag removal on credit limits over time are unlikely to account for the cyclical variation in the MPC. In essence, this exercise formalizes the intuition that while the effect of flag removal on credit limits is monotonically decreasing over time, the MPC follows an inverse-U-shaped pattern, so the former is unlikely to mechanically explain the latter.

A second key concern with the analysis above is that the composition of consumers having their bankruptcy flags removed may change over time. For example, Table 5 reports results that show higher MPCs for individuals with lower credit scores. Therefore, any cyclical variation in average credit scores of bankruptcy filers could potentially account for the time series pattern in the MPC. In other words, our estimates could confound changes in the underlying demographics of consumers filing for bankruptcy with changes in the MPC driven by the business cycle, holding the composition of consumers constant.

The most direct way to address this concern would be to show how the characteristics of bankruptcy filers changed over time between 1994–2001, corresponding to the cohorts who lost bankruptcy flags in our main sample. Unfortunately, the CCP does not extend back before 2001, so we are unable to undertake this exercise. Instead, we control for the observable characteristics of consumers around the timing of flag removal (as opposed to bankruptcy filing).

In particular, we follow DiNardo, Fortin, and Lemieux (1996) to re-weight the sample each year to match a base year along a vector of observable characteristics. We combine consumers who had a flag removed in each year with those whose flags were removed in 2008, and then estimate a probit regression with the outcome of interest being an indicator function equal to one if the observation had a bankruptcy flag removed in 2008. The regression's independent variables are the credit score and balances on open credit card, mortgage, and auto accounts in the quarter before flag removal. For each observation i, we then calculate a predicted value, $\hat{p}_i$, from that regression, and following DiNardo, Fortin, and Lemieux (1996) we define a weight, w_i, as

$$w_i \equiv \frac{\widehat{p_i}}{1-\widehat{p_i}} \cdot \frac{P(\tau_i = 2008)}{P(\tau_i \neq 2008)}.$$

We then re-estimate the MPC by year using these weights as sample weights. This allows us to account for changes in demographics across years based on observables. Appendix Figure A3 presents estimates of the MPC by year after re-weighting and suggests a roughly similar pattern as Figure 5. The similarity between these figures suggests that composition effects due to changes in observable characteristics are not able to account for the counter-cyclical variation. These results also provide suggestive evidence regarding the mechanism behind the estimated variation in the MPC over the business cycle. By holding constant mortgage balances, credit scores, and other financial characteristics, Appendix Figure A3 suggests that the deterioration of household balance sheets during the recession may play a relatively less-important role than aggregate macroeconomic conditions in accounting for the changing MPC. As a result, the estimates may generalize to other recessions, not just recessions following financial crisis.

A final concern is that the results above are focused, by necessity, on the Great Recession of 2008 and 2009, and that they may not apply to other recessions. Since the CCP data include account opening dates several years prior to the first observation year in 2001, we are able to expand our approach to study the 2001 recession. A challenge, however, is that the 2001 recession occurred sufficiently early in the sample such that studying it requires us to compress the pre-flag-removal window for estimating the MPC prior to 2004. For this reason, we view these expanded results as complementary rather than a substitute for our main results focusing on 2004-2011. Nonetheless, the fact that our approach can be used in future research to estimate MPCs beyond our current sample period is a feature of our approach.

Figure 6 presents this analysis. The first panel presents estimates of the MPC out of liquidity extending back to 2001. The remaining panels describe the effect of flag removal on balances and limits separately. We find these patterns reassuring for two reasons. First, they show that the MPC rises not only during the Great Recession years, but also during the recession of 2001. Second, the effect of flag removal on credit limits follows a pattern consistent with a subprime credit card boom during the 2000s followed by a contraction and limited recovery during and after the Great Recession. This pattern does not match the clear counter-cyclical path of the MPC, which bolsters our conclusion from the "partialling-out" exercise described above.

4.6. Do Consumers Anticipate Flag Removal?

Because credit scores increase mechanically when bankruptcy flags are removed, consumers are more likely to obtain credit and receive better terms after flag removal than before. Thus, forward-looking consumers would avoid applying for credit in the months just prior to flag removal, resulting in a "missing mass" of new accounts and inquiries in these months. However, because the existence and effects of bankruptcy flags are relatively obscure features of the credit reporting system, consumers may not anticipate or even be aware of impending flag removal when making financial decisions.

Understanding whether consumers anticipate flag removal is important for assessing the validity of our research design – in particular, whether the effects of flag removal could be attributed to demand effects instead of a shift in credit supply. It is also important for interpreting the results, since the effects of anticipated and unanticipated credit expansions should differ in theory. Throughout our analysis, we interpret flag removal as generating an exogenous, unanticipated increase in credit supply. This section provides evidence for that interpretation.

Consistent with a lack of anticipatory behavior, we find no evidence of missing mass in any of our event-study figures. By contrast, there exist smooth and steady trends in the pre-period, with clear and sharp "on impact" effects starting in the month of flag removal. None of the main figures show evidence that a consumers react to the approaching flag removal.

To investigate the roles of demand and supply in more detail, we examine the rate of credit inquiries per month around flag removal. Credit inquiries are reported in our dataset whenever a lender obtains a consumer's credit report for the purposes of screening a new credit application (Avery et al 2003).[29] While most traditional lenders require credit checks in order to obtain credit, not all lenders report each inquiry to all credit bureaus. Mortgage inquiries are typically reported to all three major credit bureaus, but auto and credit card inquiries may only

[29] "Soft" inquiries, made by consumers checking their own credit files, lenders pre-screening consumers for mail advertisements, credit monitoring of existing consumers, and other activities unrelated to credit demand, are excluded from our dataset.

be reported to one or two credit bureaus. Thus, while our dataset is likely to under-estimate the total number of credit applications consumers make, we believe it can accurately capture relative changes in the rate of credit application for a given set of consumers over time.

The first column of Figure 7 presents our main specification when inquiries per month are the outcomes of interest, and Panel A of Table 7 presents the associated point estimates. We find no statistically significant changes in mortgage and auto inquiries resulting from flag removal, consistent with it being unanticipated. The rate of credit card inquiries does increase significantly, albeit less than the increase in new accounts. Because many credit card applications result from direct mail and other forms of marketing by issuers, which in turn are targeted based on consumer credit scores, credit card inquiries are likely to confound supply and demand for credit (Han, Keys, and Li, 2013).

To further disentangle the role of more-frequent credit applications (demand effects) versus higher approval rates (supply effects) for each application, we examine the number of new accounts per inquiry as a proxy for lenders' approval rate. These results are presented in the second column of Figure 7 and in Panel B of Table 7. As noted above, our inquiry data under-estimate the true number of applications, so the average number of new accounts per inquiry may be greater than one. While the proxy cannot be used to calculate the actual approval rate, it is likely to capture changes in the approval rate as long as reporting of inquiries does not systematically change based on the timing of flag removal.

We find that the rate of new accounts per inquiry increases for all credit types following flag removal. In particular, the results suggest that the approval rate for credit cards increases even conditional on the increase in credit card inquiries. Using the pre-flag-removal mean rate of inquiries as a benchmark, the estimates from Panel B of Table 7 suggest that over 90 percent of the increase in all new accounts and over two thirds of the increase in new credit card accounts can be explained by an increase in approval rates as opposed to an increase in inquiries.[30]

[30] We can estimate the effect of the increase in approval rates by multiplying the increase in accounts per inquiry in Panel B by the pre-removal mean inquiries per quarter from Panel A, and integrating over the relevant horizon. For example, the effect of the increase in approval rates on new account openings for all account types over the first six months following flag removal is 0.12 = 0.126 account / inquiry × 0.475 inquiries / quarter × 2 quarters. Comparing this to the estimate of 0.13 from Panel A, column 1 of Table 3 suggests that the change in approval rates can account for 91 percent of the increase in new accounts for all account types.

Taken as a whole, these results support the interpretation that flag removal is a shock to credit supply rather than demand.

To more directly assess the likelihood that consumers anticipate flag removal, we surveyed 187 Americans who had declared bankruptcy at least once and asked them a series of questions designed to assess their understanding of bankruptcy flags. Table 8 describes the respondents and their responses.

Panel A describes the demographics of the sample, and Panel B presents one simple approach to assessing how much consumers understand about the nature of bankruptcy flags on credit reports. We asked all of the survey respondents whether they believe that bankruptcy flags exist at all, for 1–6 years, 7–10 years, more than 10 years, or forever. The correct answer here, regardless of chapter, is 7–10 years, and 40 percent of respondents made that choice. If respondents were choosing randomly from the available options, 16.6 percent would have chosen that option. Thus, while former bankruptcy filers seem to have some knowledge about bankruptcy flags, only a minority correctly report the timing of flag removal within a broad window. In order for our results to be explained by anticipatory behavior, consumers would need to know, not only that bankruptcy flags exist for between 7–10 years but they would need to be able to pinpoint the timing of their own flag removal within one year.

We fielded additional survey questions designed to investigate the precision of consumer knowledge about their own bankruptcy flags. Panel C of Table 8 describes one way of doing so, using responses to several of our survey questions. We focus here only on respondents who report that their most recent bankruptcy was a Chapter 7 filing. We divide the sample into whether or not their credit record still includes a bankruptcy flag, which we infer based on the self-reported year of their most recent bankruptcy. We then measure the share of respondents who correctly report whether a flag exists on their credit report. For those who declared Chapter 7 bankruptcy within the past 10 years, 71 percent of respondents correctly report that they have a bankruptcy flag. For those who declared Chapter 7 bankruptcy more than 10 years ago, 44 percent correctly report that no flag exists.

Finally, Panel D describes the most precise evidence for consumer knowledge about bankruptcy flag removal. For former Chapter 7 filers who still have a bankruptcy flag on their record, we asked how much longer the flag would remain on their record, and calculate the implied length of time between bankruptcy filing and flag removal, which we show in the paper to be about 9.5 years. Figure 8 presents a histogram of all responses, which shows a great deal of variation. Only 15 percent of the respondents report that bankruptcy flags remain on credit records for 9–11 years, and only 9 percent answer with precisely 10 years. By comparison, 8 percent would have chosen 10 years by chance among all of the available answers.

All of this suggests that relatively few bankruptcy filers know exactly when their flags will be removed. In fact, the last panel of Table 8 suggests that fewer than 10 percent of former filers know the year in which their flag will be removed, let alone the month. In order for demand effects to drive our results, consumers would not only have to know this information but also act on it strategically. These survey results thus suggest that anticipatory behavior leading up to flag removal is unlikely. They add further support to our assumption that demand effects in anticipation of flag removal are unlikely to be driving the results.

4.7 The Longer-Run Effects of Flag Removal

The results described above show that consumers increase their borrowing as a result of bankruptcy flag removal. A remaining question is how this increase in leverage affects delinquency rates and overall financial health. Of course, the consumers in this sample have a history of bankruptcy, so their overall credit risk is high.[31] But it is unclear, a priori, whether an increase in credit would improve or harm their financial health. If consumers are still affected by the factors that initially drove them into bankruptcy (e.g., due to persistence in economic shocks or persistence in their own behavior), then additional debt may lower overall financial health, and we would observe an increase in delinquencies and a reversion of credit scores toward pre-flag-removal levels. However, if new credit helps alleviate credit constraints without increasing financial distress, then the removal of bankruptcy flags could lead to greater consumption smoothing, asset building, and credit building.

We assess the impacts of flag removal on delinquency and financial health in two ways. First, we apply the same empirical framework as above, but with measures of delinquency and collections activity as the outcomes of interest. Second, we extend the framework to study long-

six months after flag removal.

[31] From Table 1, 7 percent of new accounts reported within one year of opening are 90+ days delinquent, and 4 percent of all open accounts are 90+ days delinquent as of the quarter before flag removal. These delinquency rates are significantly higher than those in the random CCP sample, and their credit scores are significantly lower.

run trends in delinquency, borrowing, and credit scores. Figure 9 presents the first of these approaches. The figure presents results for four key measures of delinquency and collections: the delinquency rate on new loans one year after origination, the delinquency rate on all open loans, collection inquiries, and new collections balances.[32] As a whole, the figure rules out an increase in delinquency after flag removal. In fact, the only pattern apparent in the figure is a short-run *decrease* in delinquencies on new accounts in Panel A. These results suggest that consumers are less likely to become delinquent on new debt taken out after flag removal, with little effect on delinquency for existing debts or bill payments.

Next, we analyze the longer-run effects of bankruptcy flag removal. Figure 10 presents four main summary measures of each consumer's credit record 60 months after bankruptcy flag removal, extending our main results by three years. The figure suggests that the initial increase in credit scores after flag removal is highly persistent and does not revert back to pre-flag-removal levels. Since credit scores are a summary measure of delinquency and credit activity, this finding is consistent with the interpretation that financial health remains stable after flag removal. Panel B examines the delinquency rate for open accounts, and suggests a small decrease in delinquencies over the longer run. The increase in the flow of new credit card accounts, balances and limits persists for at least five years after flag removal. Table 9 summarizes these and other credit outcomes over the longer run. The data suggest that instead of reverting back to pre-flag-removal levels, credit scores remain persistently higher once bankruptcy flags are removed.

Our findings that credit scores remain persistently higher and delinquencies remain unchanged contrast with those of Musto (2004), which could be due to a number of factors. Our larger sample and longer time period allow us greater precision in our estimates and allow us to track consumers for a longer period of time after flag removal. But the differences in our results could also be driven by significant changes in the characteristics of bankruptcy filers, the nature of the consumer credit market, the nature of credit scoring and credit supply models, and in the overall macro-economy between the different time periods we study. Our finding that delinquencies are unaffected by the increase in credit supply following flag removal also contrasts

[32] In all of our analysis, we consider a loan delinquent if there have been 90 or more days since the contractually obligated payment was made.

with Agarwal et al (2015), who find that subprime consumers assigned to receive higher credit limits based on discontinuities in issuer line assignment rules are more likely to default. While their study examines the effects of a one-time increase in credit limit, flag removal leads to a persistent increase in access to credit. Our results thus suggest the presence of positive externalities among creditors who lend to consumers after flag removal.

5. Conclusions

A likely explanation for the enduring interest in estimating the marginal propensity to consume out of liquidity is that the MPC plays an important role in macroeconomic stabilization policy. Policies that try to boost household demand through government transfers, subsidized loans, temporary tax cuts, or income-tax rebates are more effective if they are targeted towards households with a high MPC.

In this paper, we estimate a relatively high MPC out of liquidity for former bankruptcy filers. Using a large panel dataset, we also show that the MPC out of liquidity in this sample is higher during the Great Recession. The counter-cyclical variation is both statistically and economically significant, with the MPC decreasing by roughly 20–30 percent between 2008 and 2011 as aggregate economic conditions improved. The 12-month MPC we estimate decreased from 0.48 in 2008 to 0.36 in 2011. By comparison, this variation in the MPC over the business cycle is similar in magnitude to the difference between the "wealthy hand-to-mouth" agents and non-hand-to-mouth agents studied by Kaplan et al. (2014).

We present the following simple calibration exercise designed to assess the implications of our results for stimulus policy. Consider a hypothetical economic policy that provides $1,000 in additional credit limits to all American consumers with credit scores under 700. We take to this scenario the 2006 estimate of the MPC out of liquidity, 0.34, and first assume that that estimate applies to all years. In other words, we apply an empirical estimate of the MPC from a typical year to project the effect of stimulus during a recession. The fifth column of Table 10 presents the change in aggregate consumption one would expect, given that assumption, for each year between 2007 and 2009.

By contrast, the sixth column of Table 10 describes the change in aggregate consumption one would expect based on our estimate of the MPC for the actual year in question. The difference between the two estimates is large: $14 billion for 2008, a 40-percent difference. This calculation is stylized, of course, but it illustrates how accounting for the "state dependence" of the MPC can alter the amount of consumer credit needed to achieve a given consumption target. Ignoring that state dependence may cause policymakers to overestimate the appropriate stimulus needed. We view these results as complementary to recent work that emphasizes heterogeneity in the MPC across the population (Jappelli and Pistaferri 2014; Mian, Rao, and Sufi 2013).

Beyond policy guidance, these results provide empirical moments that can help distinguish recent macroeconomic models of household finance. Models featuring costly adjustment of illiquid assets point out that the population MPC can be *lower* in severe recessions relative to mild recessions (Kaplan and Violante 2014). Assuming that the Great Recession can be categorized as a severe recession, our evidence contradicts that prediction. This conclusion comes with the important caveat that our results are only identified on a sample with relatively low credit scores, and, as a result, our results may be specific to this population. Nevertheless, our tentative conclusion is that even during the Great Recession, the MPC out of liquidity was unusually large relative to typical economic times.

There are several important limitations of our results. First, our results are based on former bankruptcy filers. We interpret the results as informative about the MPC out of liquidity for consumers with relatively low credit scores, but this is an assumption that should be confirmed more directly in future work. Whether these results generalize to the broader population is an open question. Second, consistent with the past literature, we interpret our results as reflecting the propensity to consume out of liquidity. However, we do not observe consumption directly. It would be useful to confirm in other data sets how the estimated MPC out of liquidity actually maps onto changes in consumption. Lastly, we interpret our results as reflecting an unanticipated change in liquidity. Whether the results are similar for anticipated changes in consumer credit is not clear.

An important limitation of this paper's analysis is that it focuses solely on credit-report outcomes for individuals whose bankruptcy flags are removed. In reality, some of the effects of flag removal may spill over onto other members of the individual's household. As is the case with most recent research on credit market outcomes, we are unable to match individuals within a household. Future work may be able to make these linkages, and it would be interesting to see how individual-level estimates differ from household-level estimates. Similar to the work of Cesarini et al. (2017), it may be possible to use MPC estimates for different members of the household to distinguish between different models of household bargaining.

Overall, our results are broadly consistent with the conjecture of Johnson, Parker, and Souleles (2006) that liquidity constraints become more important as aggregate conditions deteriorate, which raises the MPC among subprime consumers. Our results also confirm the conjecture by Jappelli and Pistaferri (2014) that one should be concerned that MPC estimates in severe recessions may be significantly different than MPC estimates in "normal" economic times. Future work ought to continue to investigate the role of aggregate economic conditions on the MPC, especially for low-credit-score consumers who may be especially responsive to economic stimulus.

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Table 1. Summary Statistics

This table presents summary statistics for Chapter 7 bankruptcy filers whose flags are removed between 2004 and 2011 alongside sample statistics for a one-percent random sample of the CCP data. For the bankruptcy flag sample, the table summarizes characterstics in the quarter preceding flag removal.

	Mean for bankruptcy flag sample	Mean for a 1-percent sample of the CCP
Total number of bankruptcies	1.3	0.1
Chapter 7	1.2	0.1
Chapter 13	0.1	0.0
Summary credit characteristics		
Credit score	616	696
# of open accounts	4.8	5.3
Balances on open accounts	$76,348	$72,823
Credit card balance	$3,720	$4,142
Mortgage balance	$56,575	$53,918
Auto balance	$6,656	$4,068
Other credit balance	$9,397	$10,696
Principal and limits on open accounts	$85,457	$98,861
Credit card limits	$8,170	$20,732
Mortgage principal	$55,688	$55,151
Auto principal	$9,835	$6,304
Other prinicpal and limits	$11,451	$16,358
Inquiries and delinquency		
# credit inquiries per quarter	0.5	0.3
# collections inquiries per quarter	0.04	0.02
Balance on collections trades	$31	$10
Delinquency rate on new trades	0.07	0.04
Delinquency rate on open trades	0.04	0.02

Table 2. Effect of Bankruptcy Flag Removal on Credit Scores (First Stage)

This table presents the effect of bankruptcy flag removal on credit scores 6 months and 12 months after bankruptcy flag removal. Each column summarizes a separate regression with credit score as the outcome of interest. The underlying regressions include a linear trend in the number of months until flag removal, indicator variables for the 24 months after flag removal, a fixed effect for year-month flag removal cohort, and a fixed effect for each calendar quarter. Standard errors are clustered on flag-removal-month cohorts and associated *p* -values are in brackets.

	(1) All	(2) 2004	(3) 2005	(4) 2006	(5) 2007	(6) 2008	(7) 2009	(8) 2010	(9) 2011
6-month	15.455	15.285	15.376	15.829	17.119	16.567	16.844	16.814	16.540
effect	(0.513)	(1.621)	(1.991)	(1.198)	(1.586)	(2.019)	(1.439)	(2.051)	(2.048)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
12-month	16.426	17.163	15.520	18.030	18.696	18.741	18.839	18.850	19.161
effect	(0.562)	(2.119)	(1.941)	(1.887)	(2.190)	(2.725)	(1.834)	(2.186)	(2.576)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

Table 3. Effect of Bankruptcy Flag Removal on New Accounts

Each point estimate represents the change in each outcome per 10-point change in credit score. This is calculated by dividing the reduced-form effect of flag removal on the outcome by the effect of flag removal on credit score. The effects are estimated jointly by Seemingly Unrelated Regression, and the standard errors in parentheses are clustered on bankruptcy-flag cohort and calculated using the delta method; associated *p*-values in brackets. The underlying regressions include a linear trend in the number of months until flag removal, indicator variables for the 24 months after flag removal, a fixed effect for year-month flag removal cohort, and a fixed effect for each calendar month.

	(1) All	(2) Cards	(3) Mortgage	(4) Auto	(5) Other
			A. Number of new accounts		
6-month effect	0.132	0.099	0.002	0.003	0.028
	(0.010)	(0.008)	(0.001)	(0.001)	(0.003)
	[0.000]	[0.000]	[0.017]	[0.004]	[0.000]
12-month effect	0.252	0.181	0.007	0.007	0.056
	(0.019)	(0.014)	(0.002)	(0.002)	(0.006)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Pre-removal mean stock	4.789	2.830	0.385	0.491	1.083
			B. Balances on new accounts		
6-month effect	489	152	155	40	141
	(140)	(14)	(122)	(16)	(44)
	[0.000]	[0.000]	[0.204]	[0.014]	[0.001]
12-month effect	1140	290	473	99	276
	(258)	(25)	(231)	(30)	(72)
	[0.000]	[0.000]	[0.041]	[0.001]	[0.000]
Pre-removal mean	71,397	3,233	52,978	6,282	8,904
			C. Principal and limits on new accounts		
6-month effect	927	411	195	53	269
	(170)	(34)	(135)	(20)	(77)
	[0.000]	[0.000]	[0.146]	[0.008]	[0.000]
12-month effect	2000	778	609	132	487
	(315)	(63)	(262)	(36)	(127)
	[0.000]	[0.000]	[0.020]	[0.000]	[0.000]
Pre-removal mean	81,061	7,667	53,030	9,302	10,782

Table 4. Estimated Marginal Propensity to Consume

Panel A presents an estimate of the MPC for each year, with the numerator the effect of flag removal on credit-card balances and the denominator the effect of flag removal on credit-card limits. Standard errors in parentheses are clustered on bankruptcy-flag cohort and calculated using the delta method; associated *p*-values in brackets. For panels B and C, each point estimate represents the change in each outcome per 10-point change in credit score, instrumented by bankruptcy flag removal. The underlying regressions include a linear trend in the number of months until flag removal, indicator variables for the 24 months after flag removal, a fixed effect for year-month flag removal cohort, and a fixed effect for each calendar month. The *p*-values in the final column are based on a test of equality across all years.

	(1) All	(2) 2004	(3) 2005	(4) 2006	(5) 2007	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) *p*-value
					A. Marginal propensity to consume					
6-month effect	0.371	0.335	0.332	0.348	0.445	0.461	0.410	0.352	0.383	0.021
	(0.011)	(0.040)	(0.029)	(0.029)	(0.022)	(0.052)	(0.072)	(0.050)	(0.062)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	
12-month effect	0.373	0.320	0.355	0.343	0.463	0.480	0.454	0.376	0.362	0.013
	(0.011)	(0.032)	(0.028)	(0.033)	(0.028)	(0.054)	(0.076)	(0.048)	(0.067)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	
					B. Credit card balances					
6-month effect	152.363	233.202	267.159	209.076	233.305	106.515	49.552	49.734	49.153	0.000
	(13.755)	(55.646)	(56.532)	(30.729)	(32.037)	(34.127)	(14.764)	(12.297)	(11.092)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]	[0.001]	[0.000]	[0.000]	
12-month effect	289.975	442.039	557.679	365.989	397.920	173.702	106.443	95.180	80.242	0.000
	(24.731)	(95.958)	(112.214)	(55.326)	(58.732)	(58.929)	(31.530)	(21.468)	(19.830)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]	[0.001]	[0.000]	[0.000]	
					C. Credit card limits					
6-month effect	410.820	695.784	805.773	601.296	523.751	231.190	120.848	141.222	128.396	0.000
	(33.977)	(121.815)	(121.680)	(70.855)	(74.041)	(60.831)	(28.584)	(24.820)	(17.506)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	
12-month effect	778.102	1379.660	1572.879	1067.508	859.622	361.621	234.565	253.384	221.809	0.000
	(63.283)	(231.989)	(235.145)	(132.483)	(133.811)	(104.653)	(56.531)	(42.812)	(33.031)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	

Table 5. MPC Stratified by Credit Score, Income, and Utilization

This table presents estimates of the MPC out of liquidity for groups of consumers stratified by whether they have low, medium, or high levels of the given outcome in the month before bankruptcy flag removal. See notes to Table 4 for how MPC is calculated. Credit score groups: less than or equal to 660, 661–700, and greater than 700. Mean credit score for the 3 groups in Panel A are: 523, 615, and 670. Mean income for the 3 groups in Panel B are: $44,556, $55,975, and $73,153. Mean utilization for the 3 groups in Panel C are: 0.07, 0.54, and 1.04.

	(1) Low	(2) Medium	(3) High
		A. Stratified by Credit Score	
6-month effect	0.393	0.370	0.285
	(0.014)	(0.022)	(0.063)
	[0.000]	[0.000]	[0.000]
12-month effect	0.407	0.354	0.255
	(0.013)	(0.021)	(0.050)
	[0.000]	[0.000]	[0.000]
		B. Stratified by Median Tract Income	
6-month effect	0.376	0.372	0.387
	(0.026)	(0.019)	(0.018)
	[0.000]	[0.000]	[0.000]
12-month effect	0.373	0.380	0.385
	(0.025)	(0.018)	(0.018)
	[0.000]	[0.000]	[0.000]
		C. Stratified by Utilization	
6-month effect	0.282	0.414	0.479
	(0.021)	(0.022)	(0.022)
	[0.000]	[0.000]	[0.000]
12-month effect	0.274	0.425	0.491
	(0.019)	(0.025)	(0.021)
	[0.000]	[0.000]	[0.000]

Table 6. MPC Stratified by Local Economic Conditions

This table presents estimates of the estimated MPC for groups of consumers stratified by the unemployment rate in their counties of residence. Panel A presents results when counties are stratified by their peak unemployment between 2004 and 2011; Panel B presents results when counties are stratified by their average unemployment over the entire sample. See notes to Table 4 for how MPC is calculated. Mean peak unemployment for the three groups of counties in Panel A are 7.4 percent, 9.8 percent, and 12.7 percent, respectively. Mean average unemployment for the three groups of counties in Panel B are 5.3 percent, 6.7 percent, and 8.7 percent, respectively.

	(1) Low	(2) Medium	(3) High
	A. Counties Stratified by Peak Unemployment		
6-month effect	0.353	0.367	0.421
	(0.021)	(0.023)	(0.024)
	[0.000]	[0.000]	[0.000]
12-month effect	0.366	0.381	0.397
	(0.020)	(0.023)	(0.020)
	[0.000]	[0.000]	[0.000]
	B. Counties Stratified by Average Unemployment		
6-month effect	0.348	0.384	0.410
	(0.023)	(0.020)	(0.024)
	[0.000]	[0.000]	[0.000]
12-month effect	0.359	0.391	0.395
	(0.021)	(0.018)	(0.022)
	[0.000]	[0.000]	[0.000]

Table 7. Effect of Bankruptcy Flag Removal on Inquiries and Trades Per Inquiry

Each point estimate represents the change in each outcome per 10-point change in credit score. This is calculated by dividing the reduced-form effect of flag removal on the outcome by the effect of flag removal on credit score. The effects are estimated jointly by Seemingly Unrelated Regression, and the standard errors in parentheses are clustered on bankruptcy-flag cohort and calculated using the delta method; associated *p* -values in brackets. The underlying regressions include a linear trend in the number of months until flag removal, indicator variables for the 24 months after flag removal, a fixed effect for year-month flag removal cohort, and a fixed effect for each calendar month.

	(1) All	(2) Cards	(3) Mortgage	(4) Auto	(5) Other
			A. Number of inquiries		
6-month effect	0.033	0.021	0.000	0.001	0.004
	(0.008)	(0.002)	(0.003)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.958]	[0.561]	[0.004]
12-month effect	0.067	0.037	0.002	0.002	0.007
	(0.022)	(0.004)	(0.005)	(0.003)	(0.002)
	[0.002]	[0.000]	[0.711]	[0.580]	[0.002]
Pre-removal mean per quarter	0.475	0.186	0.151	0.061	0.077
			B. Trades per inquiry		
6-month effect	0.126	0.184	0.010	0.030	0.246
	(0.012)	(0.020)	(0.007)	(0.020)	(0.042)
	[0.000]	[0.000]	[0.182]	[0.135]	[0.000]
12-month effect	0.095	0.139	0.025	0.021	0.151
	(0.011)	(0.018)	(0.008)	(0.021)	(0.037)
	[0.000]	[0.000]	[0.001]	[0.323]	[0.000]
Pre-removal mean	0.920	1.184	0.234	0.880	1.705

Table 8. Survey Evidence on Whether Consumers Anticipate Flag Removal

This table presents results of an online survey fielded to 187 individuals who had declared bankruptcy in the past. Panel A describes the demographics of the respondents. Panel B reports responses to the question "How long do you think lenders know about your past bankruptcy filing(s)?" For Panels C and D, we infer the existence of bankruptcy flags on a respondent's credit record using self-reported information about the year and chapter of their most recent bankruptcy filing. We then compare the inferred existence of their bankruptcy flag to their responses to "Is there a record of your most recent bankruptcy on your credit report today?" and "How many more years will your most recent bankruptcy remain on your credit report?" See text for details.

Sample	Question	*N*	Mean
	A. Demographics		
All surveyed respondents	Female	187	61.5%
	At least an associate degree	187	46.5%
	Non-white	187	27.3%
	Employed	187	70.1%
	B. Understanding of Bankruptcy Flags in General		
All surveyed respondents	Correctly report that bankruptcy flags exist for 7–10 years	187	39.6%
Those who filed Chapter 7		92	44.6%
Those who filed Chapter 13		71	38.0%
	C. Existence of Most Recent Bankruptcy Flag, Chapter 7 Only		
Respondent's credit report still has a bankruptcy flag	Reports that flag exists	65	70.8%
	Reports that no flag exists	65	13.8%
	Does not know	65	15.4%
Respondent's credit report no longer has a bankruptcy flag	Reports that flag exists	27	18.5%
	Reports that no flag exists	27	44.4%
	Does not know	27	37.0%
	D. Exact Years Left on Most Recent Bankruptcy Flag, Chapter 7 Only		
Respondent's credit report still has a bankruptcy flag	Reports correct number of years remaining on flag plus or minus 1	65	15.4%
	Reports exactly correct number of years remaining on flag	65	9.2%

Table 9. Long-Run Effects of Bankruptcy Flag Removal

This table presents estimates of the effect of flag removal on the given outcomes in the long run. The underlying regressions are identical to those of Table 2 (for column 1) or Table 3 (for other columns), but with up to 60 months of post-bankruptcy-flag-removal data included. Standard errors in parentheses clustered on flag-removal cohort, associated *p*-values in brackets.

	(1) Credit Score	(2) Delinq Rate	(3) MPC	(4) Card Limits	(5) Card Balances	(6) Mortgage Principal	(7) Auto Principal
12-month effect	16.381	0.000	0.373	750	279	569	170
	(0.540)	(0.001)	(0.011)	(63)	(25)	(374)	(43)
	[0.000]	[0.624]	[0.000]	[0.000]	[0.000]	[0.128]	[0.000]
24-month effect	17.352	0.000	0.372	1243	462	1208	361
	(0.522)	(0.001)	(0.014)	(113)	(45)	(878)	(94)
	[0.000]	[0.730]	[0.000]	[0.000]	[0.000]	[0.169]	[0.000]
36-month effect	17.767	0.000	0.378	1654	625	1969	587
	(0.605)	(0.001)	(0.018)	(175)	(72)	(1573)	(168)
	[0.000]	[0.797]	[0.000]	[0.000]	[0.000]	[0.211]	[0.000]
48-month effect	17.823	0.000	0.387	2040	789	2811	814
	(0.669)	(0.001)	(0.023)	(241)	(102)	(2457)	(259)
	[0.000]	[0.739]	[0.000]	[0.000]	[0.000]	[0.253]	[0.002]
60-month effect	18.123	- 0.001	0.399	2370	945	3761	1081
	(0.749)	(0.001)	(0.028)	(333)	(144)	(3497)	(372)
	[0.000]	[0.354]	[0.000]	[0.000]	[0.000]	[0.282]	[0.004]
Pre-removal mean stock	616	0.040	-	8,182	3,685	55,555	9,809

Table 10. Policy Simulation

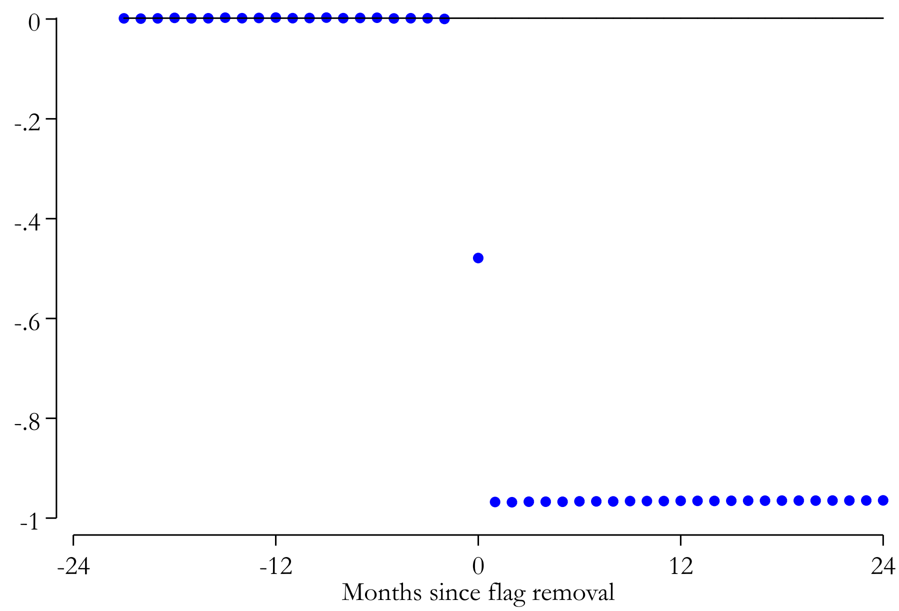
This table presents an illustrative simulation of a hypothetical stimulus policy that increases credit limits by $1,000 for all Americans with credit scores under 700. The predicted spending impacts are based on the MPC estimates from Table 4, Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	MPC for each year	Predicted spending after $1,000 increase in limits	Predicted spending based on 2006 MPC	Number of consumers with credit score under 700	Change in aggregate consumption based on 2006 MPC	Change in aggregate consumption based on time-varying MPC	Percent difference
2007	0.46	463	343	100,464,000	$34.44 bil	$46.50 bil	35.0
2008	0.48	480	343	101,976,000	$34.96 bil	$48.98 bil	40.1
2009	0.45	454	343	102,307,200	$35.08 bil	$46.43 bil	32.4

Figure 1. Direct Effect of Bankruptcy Flag Removal

The circular markers in the figure plot the estimated effects of event time, controlling for calendar year-quarter and flag-removal cohort. Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.

A. Bankruptcy Flag on Record



B. Credit Score

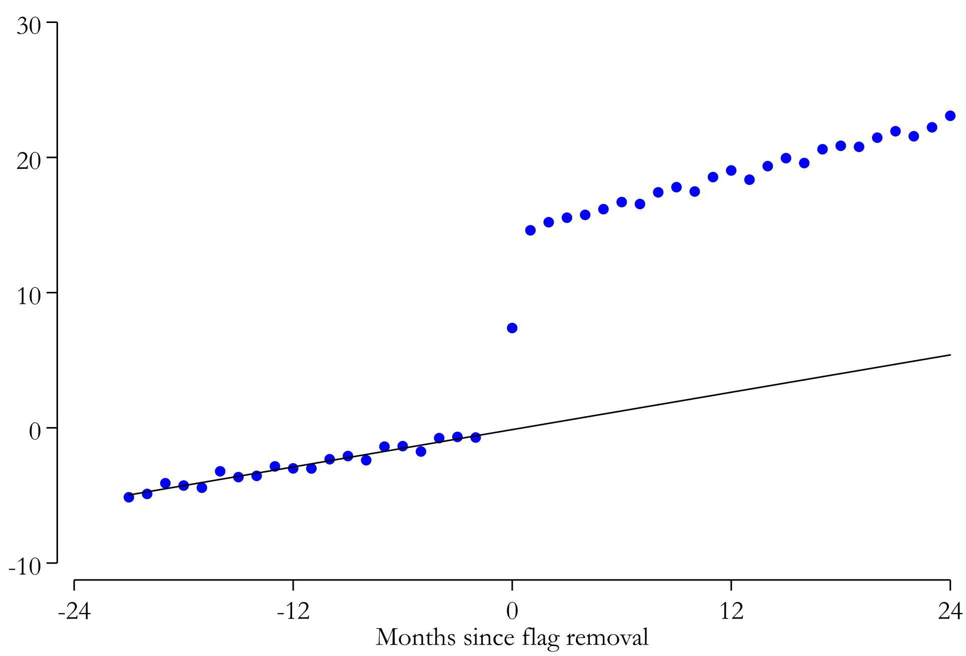
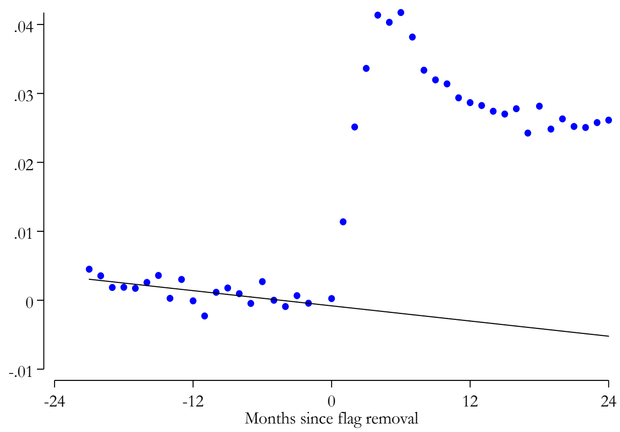


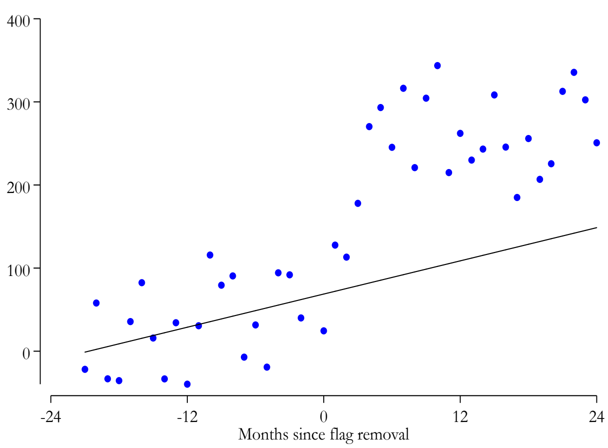
Figure 2. Effect of Bankruptcy Flag Removal on Summary Outcomes

The circular markers in the figure plot the estimated effects of event time, controlling for calendar year-month and flag-removal cohort. Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.

A. Number of New Accounts



B. Balances on New Accounts



C. Principal and Limits on New Accounts

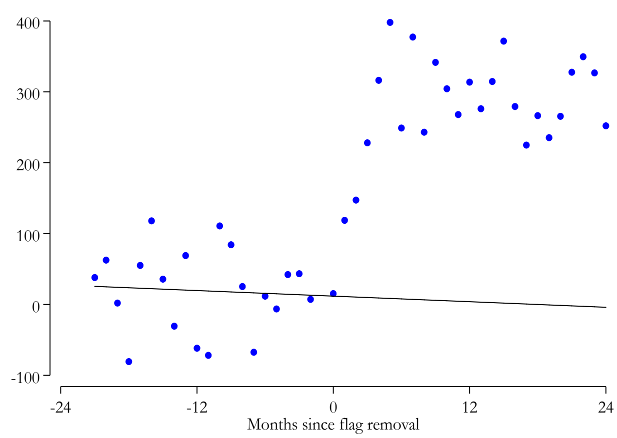


Figure 3. Effect of Bankruptcy Flag Removal on Credit Cards

The circular markers in the figure plot the estimated effects of event time, controlling for calendar year-month and flag-removal cohort. Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.

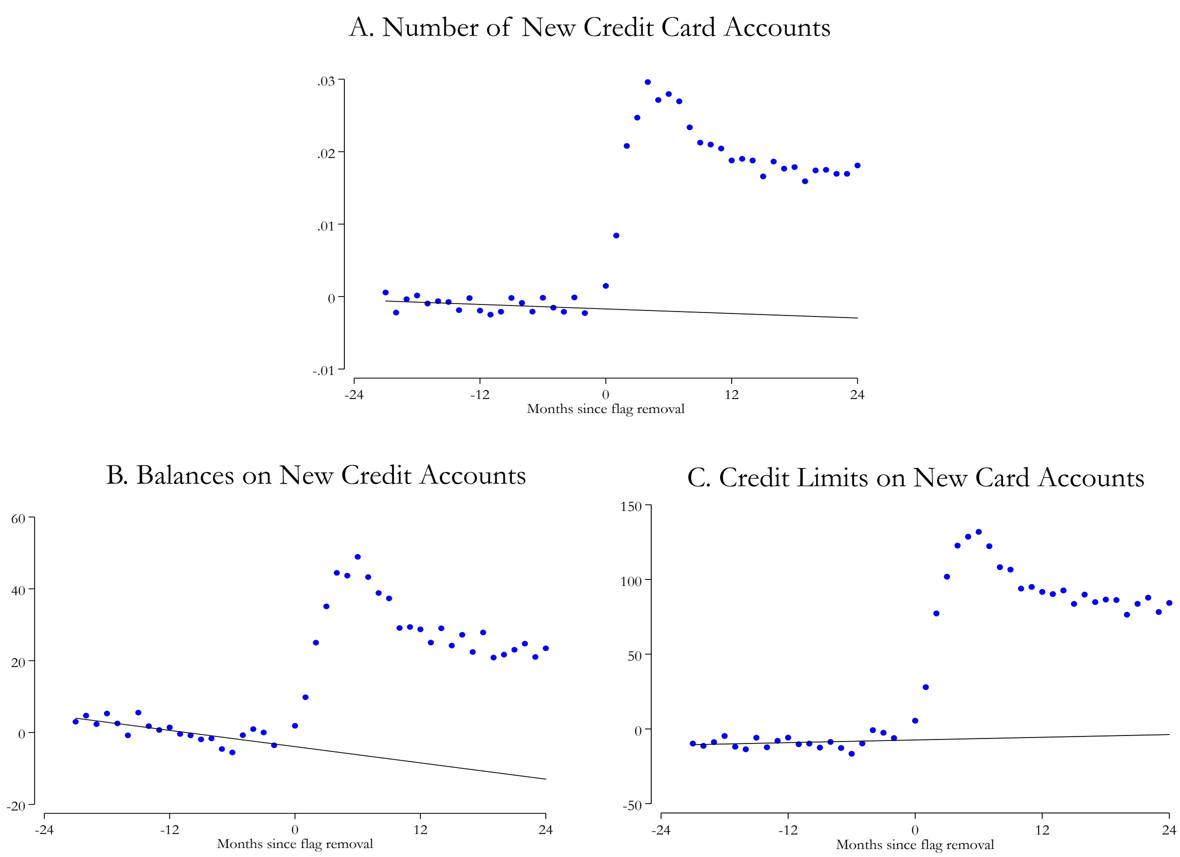
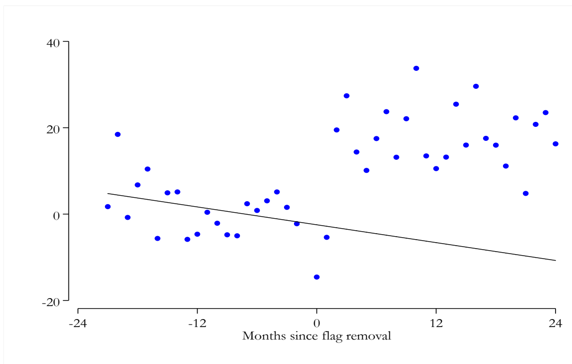


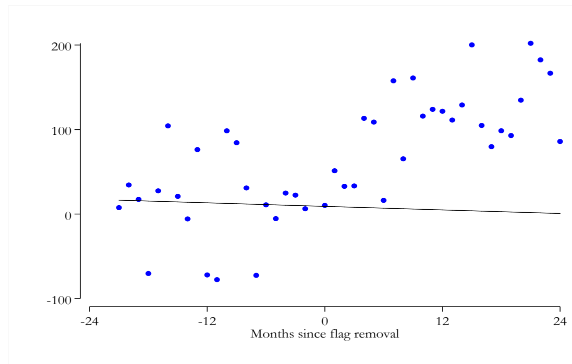
Figure 4. Effect of Bankruptcy Flag Removal on Auto Loans and Mortgages

The circular markers in the figure plot the estimated effects of event time, controlling for calendar year-month and flag-removal cohort. Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.

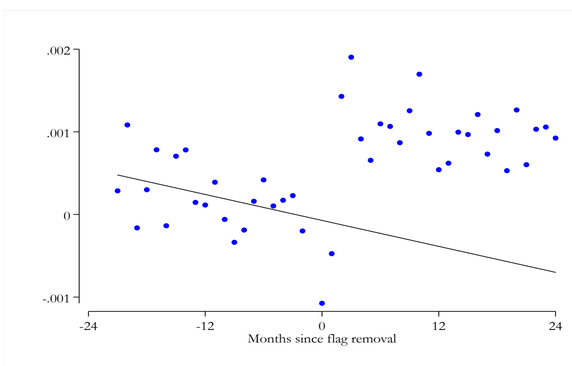
A. Principal on New Auto Accounts



B. Principal on New Mortgage Accounts



C. Number of New Auto Accounts



D. Number of New Mortgage Accounts

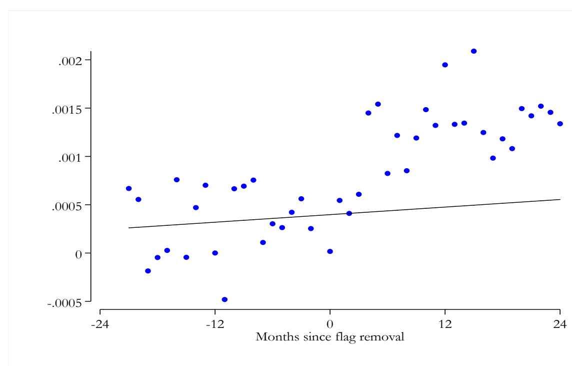
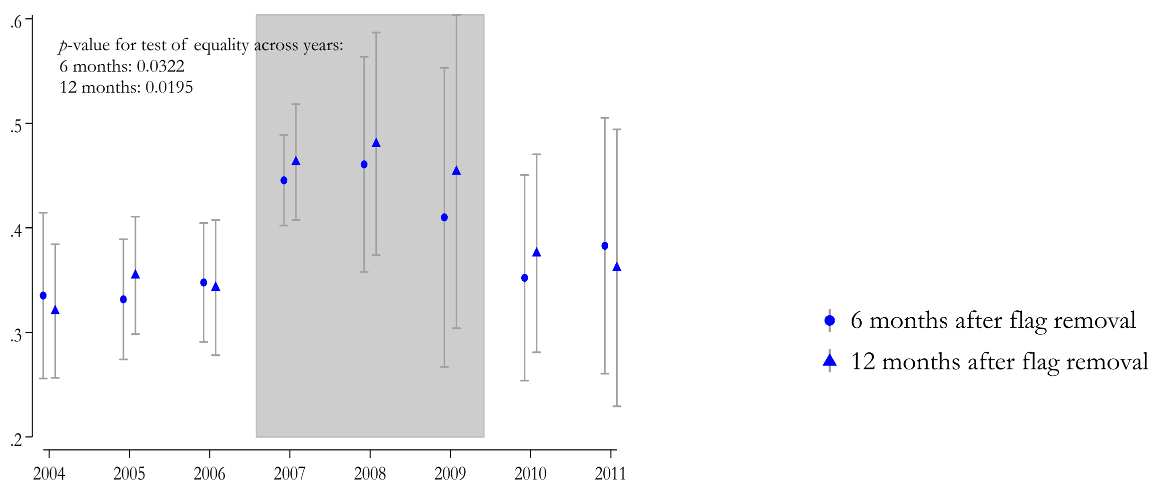


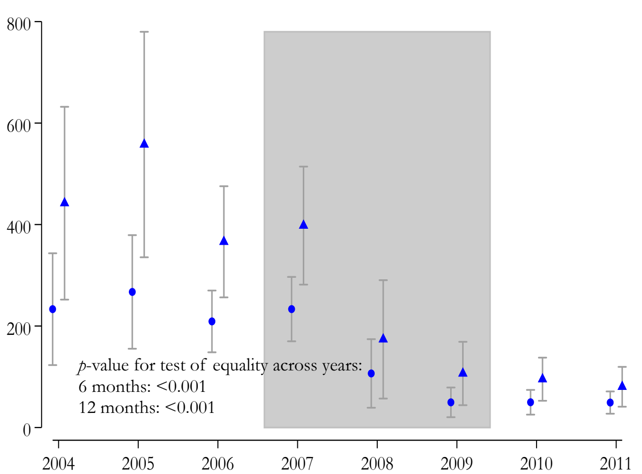
Figure 5. Estimated Marginal Propensity to Consume Over Time

This figure plots the estimated marginal propensity to consume by year and also the numerator and denominator of the estimated marginal propensity to consume by year. The shaded region indicates the Great Recession.

A. Marginal Propensity to Consume



B. Credit Card Balances



C. Credit Card Limits

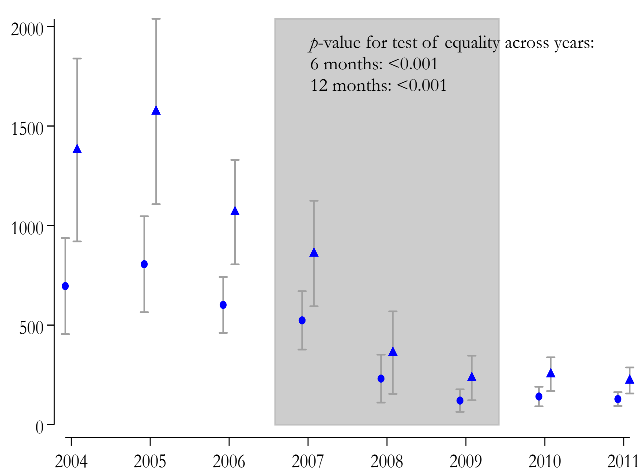


Figure 6. Marginal Propensity to Consume for Extended Time Series

This figure is based on an alternative sample that includes flag-removal cohorts from 2001 through 2013. These estimates are based on a two-year pre and post window for each cohort, or the widest window possible in the dataset. The figure plots the estimated marginal propensity to consume by year and also the numerator and denominator of the estimated marginal propensity to consume by year. The shaded regions indicate recessions.

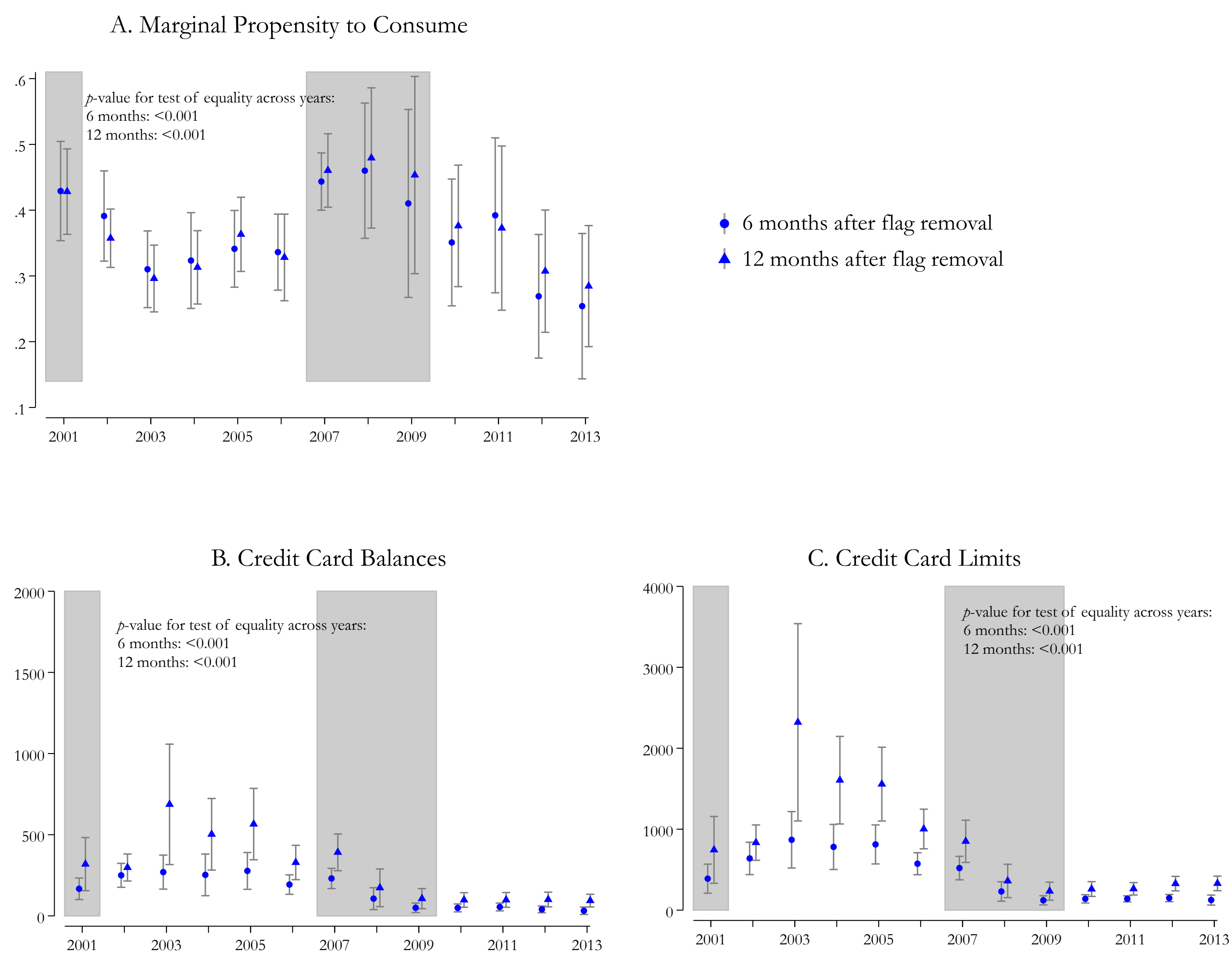


Figure 7. Effect of Bankruptcy Flag Removal on Inquiries and New Trades per Inquiry

The circular markers in the figure plot the estimated effects of event time, controlling for calendar year-month and flag-removal cohort. Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.

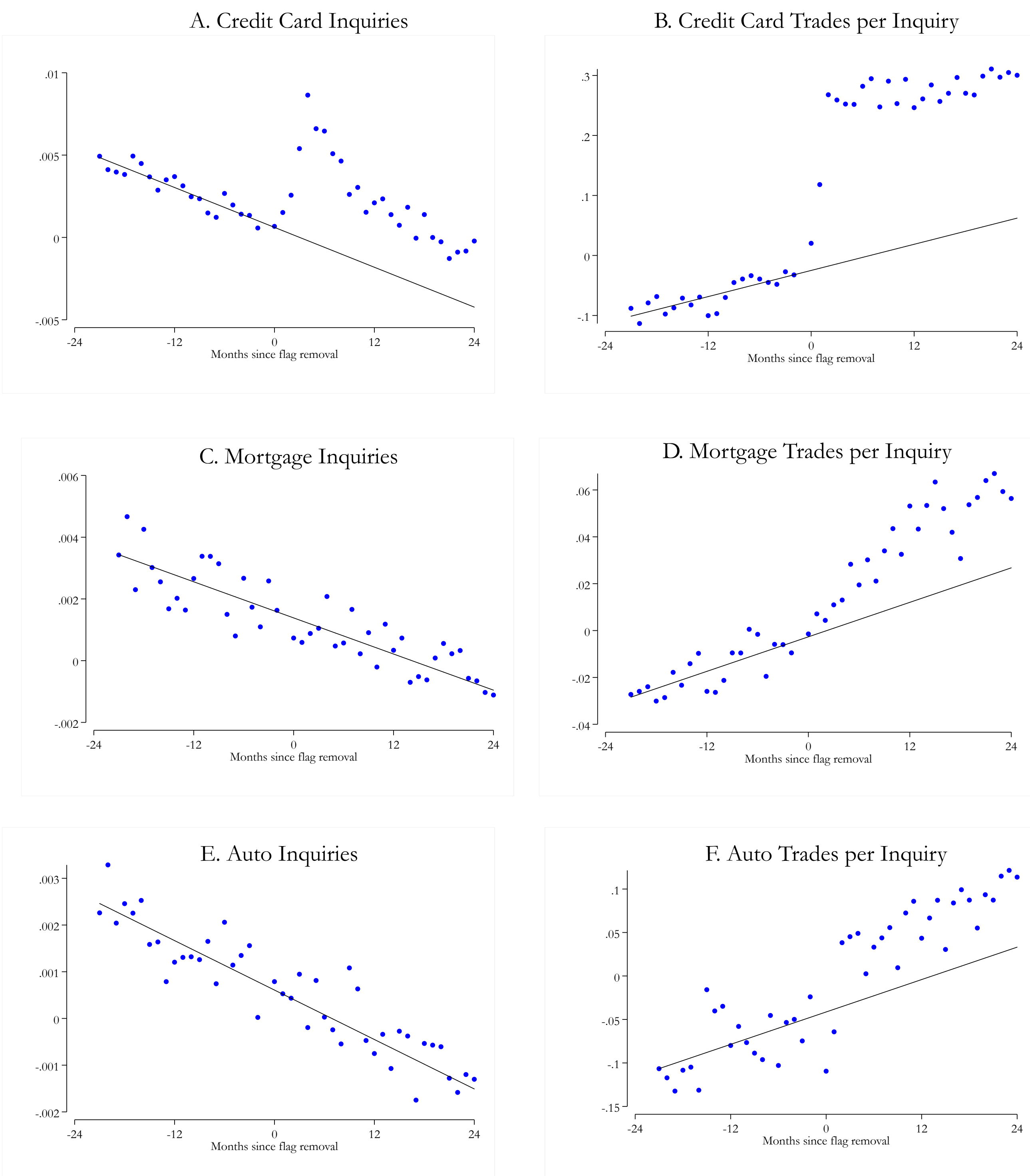


Figure 8. Survey Estimates: Consumer Knowledge of the Bankruptcy Flag Duration

This figure summarizes the survey responses of 65 consumers who declared Chapter 7 bankruptcy within the past 10 years. The survey asks respondents how many more years the flag will remain on their records, and this figure plots the implied duration of bankruptcy flags based on these responses.

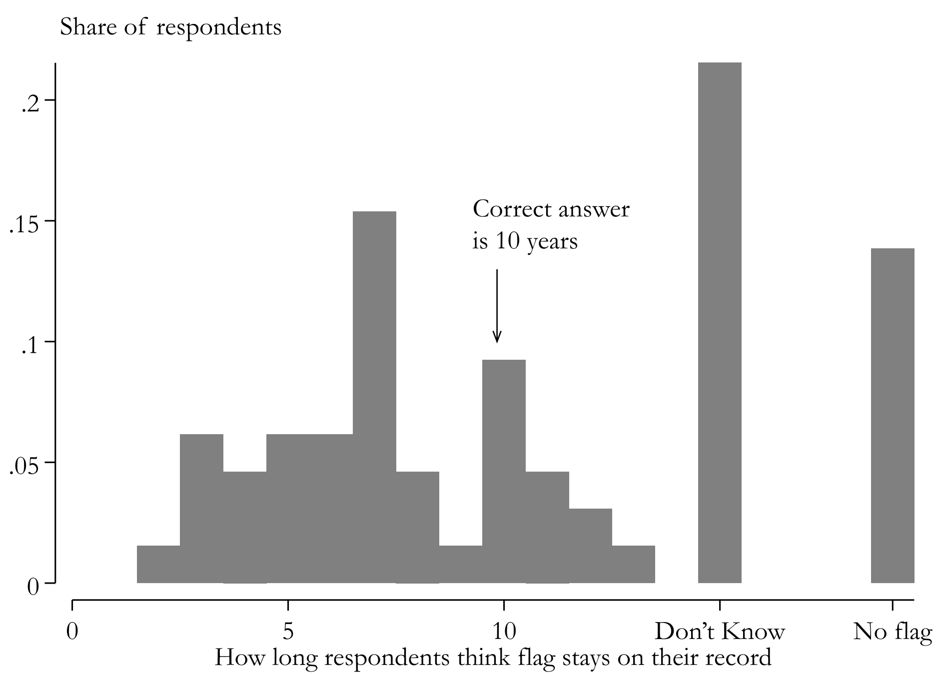


Figure 9. Effect of Bankruptcy Flag Removal on Delinquency

The circular markers in the figure plot the estimated effects of event time, controlling for calendar year-month and flag-removal cohort. Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.

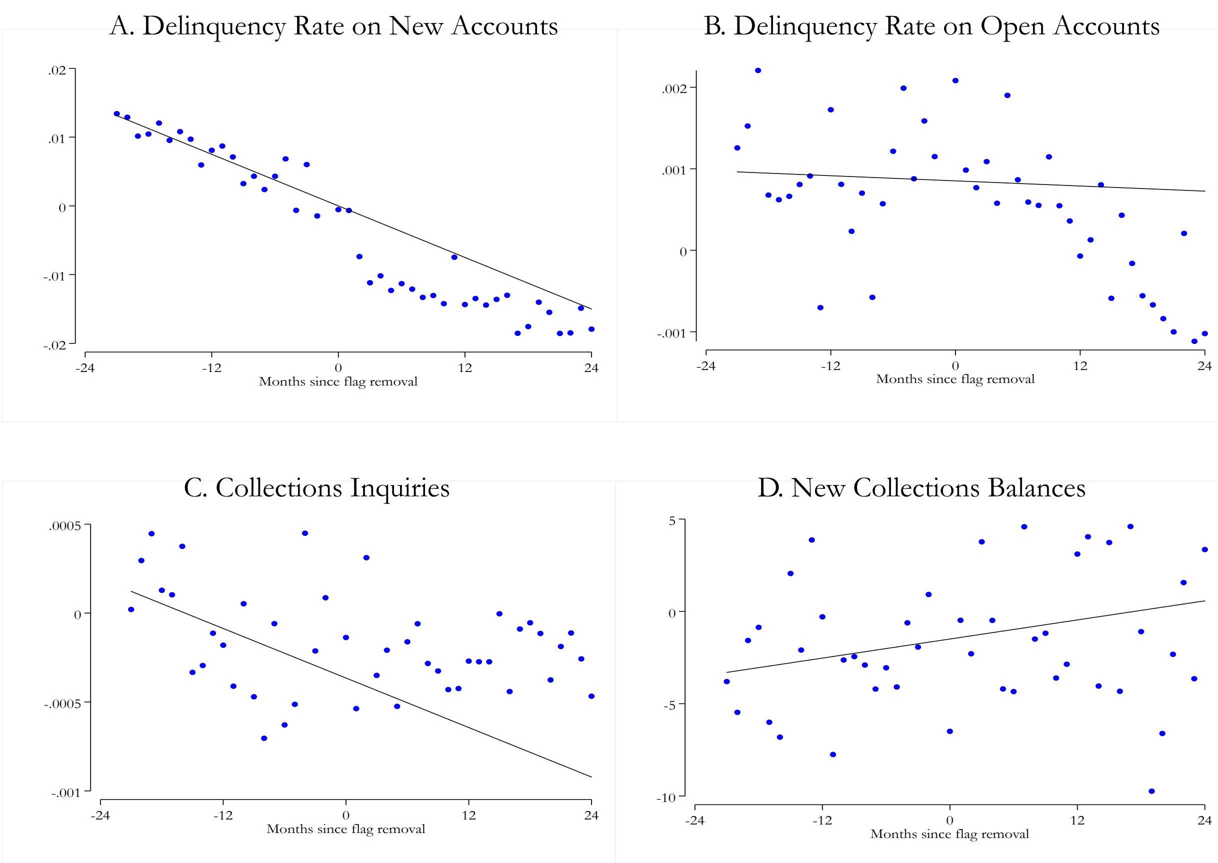


Figure 10. Long-Run Changes in Outcomes

The circular markers in the figure plot the estimated effects of event time, controlling for flag-removal cohort and either calendar year-quarter (panels A and B) or calendar year-month (panels C and D). Time periods -24, -23, and -22 are restricted to have same point estimate; time period -1 is omitted. The solid line is an OLS regression line fit to all pre-period event-study estimates.