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ABSTRACT

This paper estimates how the marginal propensity to consume (MPC) varies over the business cycle by exploiting exogenous variation in credit card borrowing limits. 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 out of liquidity" of 0.37. We find a significantly higher MPC during the Great Recession, with an average MPC roughly 20–30 percent larger between 2007 and 2009 compared to surrounding years. We find no evidence that the counter-cyclical variation in the average MPC is accounted for by compositional changes or by changes over time in the supply of credit following bankruptcy flag removal. These results are consistent with models where liquidity constraints bind more frequently during recessions.

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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, which in turn have led 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 – adjustment costs, illiquid assets, and liquidity constraints – 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 out of liquidity 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, estimates of the variation in the MPC over the business cycle can be useful for designing stimulus policies aimed at increasing aggregate consumption through expansions of consumer credit.

To our knowledge, however, there exists little empirical evidence regarding how the MPC varies over the business cycle.[2] Several studies calibrate structural models that incorporate variation in aggregate economic conditions and come to varying conclusions. For example, Kaplan and Violante (2014) calibrate a model that emphasizes the role of illiquid wealth

[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."

and find that the effects of stimulus may be smaller during more severe recessions that induce households to pay a transaction cost to liquidate their assets. By contrast, a calibration by Carroll et al. (2013) finds that the MPC may be roughly constant over time.[3]

In this paper, we provide direct evidence on how the MPC out of liquidity varies between 2004 and 2011, covering the 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 out of liquidity. The Fair Credit Reporting Act (FCRA) requires that the record or "flag" of a Chapter 7 bankruptcy be removed ten years after the bankruptcy is adjudicated.[4] 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 trades (i.e., new consumer credit accounts) 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, for each 10-point increase in their credit scores, consumers borrow an additional \$290 on new credit cards, take out \$473 in new mortgages, and take out \$99 in new auto loans. The limits on new credit cards increase by \$778 per 10-point change in credit score, leading to an MPC out of liquidity of 0.37.

The sample of former bankruptcy filers that underlies this estimate has lower credit scores than the general population, and the estimated MPC out of liquidity is broadly similar

[3] Interestingly, Carroll et al. (2013) take it as good news that their model implies the MPC may not vary over the business cycle, writing that "neither the mean value of the MPC nor the distribution changes much when the economy switches from one state to the other... The result is encouraging because it provides reason to hope that micro-economic empirical evidence about the MPC obtained during normal, non-recessionary times may still provide a good guide to the effects of stimulus for policymakers during the Great Recession."

[4] FCRA 15 U.S.C. § 1681c. The record of a Chapter 13 bankruptcy is removed 7 years later. 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.

to the few previous estimates of MPC out of liquidity for subprime borrowers.[5] Additionally, our findings confirm a conclusion supported by prior evidence: bankruptcy filers are not excluded from credit markets, but may be extended credit on less favorable terms (Fisher et al., 2004; Jagtiani and Li, 2014; Cohen-Cole et al., 2013; Han, Keys, and Li, 2015).

We next examine how the MPC out of liquidity evolved over the course of the Great Recession. To do so, we estimate the change in credit limits and credit card balances for borrowers whose flags were removed in each year from 2004 through 2011. The MPC out of liquidity increased from 0.34 in 2004 to a peak of 0.46 in 2008 followed by a drop to 0.38 by 2011. These results are consistent with liquidity constraints being significantly more likely to bind during the years of the Great Recession between 2007 and 2009 than in prior or subsequent years. Several exercises verify that this pattern is not driven by the changing selection of consumers subject to bankruptcy flag removal or to specific functional-form assumptions.

We carry out several additional analyses to assess heterogeneity in the MPC, to measure the long-run effects of flag removal, and to test whether consumers anticipate bankruptcy flag removal. First, we estimate the MPC separately by pre-flag-removal credit score, median income in the census tract, and credit card utilization.[6] Consistent with previous studies, we find little variation in the MPC by income (Gross and Souleles 2002; Johnson, Parker, and Souleles 2006). However, consumers with lower pre-flag-removal credit scores or higher pre-flag-removal credit utilization exhibit a higher MPC out of liquidity. That pattern is consistent with credit constraints being a driver of the substantial average MPCs we estimate.

We also study the longer-run effects of flag removal by extending our main results out to five years following bankruptcy flag removal. We find that the average increase in credit scores persists – virtually unchanged – for at least five years following bankruptcy flag removal. Similarly, we find strongly persistent effects on credit limits and credit card borrowing. These 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

[5] 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.

[6] The CPP lacks a direct measure of income, so we proxy for income with the median income of each individual's census tract at the time of flag removal.

usage following flag removal causes an increase in delinquencies, collections inquiries, or collections trades. This suggests that former bankruptcy filers are able to take on additional debt without increasing their risk of default.

Finally, we test whether consumers anticipate flag removal, and conclude that it is largely unanticipated. We observe no change in borrowing in the months before flag removal, which suggests that consumers do not postpone applying for credit in anticipation of better terms after flag removal. We find no evidence of intertemporal substitution in anticipation of flag removal. The persistence and lack of anticipatory effects simplifies the interpretation of our empirical results, allowing us to interpret the estimated MPCs as resulting from an unexpected, permanent increase in borrowing limits.

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 2011; Bos, Breza, and Liberman 2016; 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. 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 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.[7] Most closely related to our paper are works by Gross and Souleles (2002), Agarwal et al. (2015), and Aydin (2016), who all study the MPC out of liquidity by exploiting sharp variation in credit card limits. The overall MPC out of liquidity that we measure is similar to previous estimates that focus on subprime customers in the U.S., and this paper is distinguished by its focus on variation in the MPC over the business cycle.

Finally, our paper complements recent, model-based estimates of how the MPC varies over the business cycle (Carroll et al. 2013; Kaplan and Violante 2014). One advantage of

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

these recent studies is that they focus on the general population. By contrast, our estimates are based on former bankruptcy filers. That said, former bankruptcy filers make up nearly ten percent of the population (Stavins, 2000), and a much larger share of the subprime market. As we describe below, our estimates are likely relevant to the broader population of subprime borrowers with relatively low credit scores. As a result, future calibrations can use these estimate to extrapolate from bankruptcy filers to other groups.

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 describes the event-study framework we employ to evaluate the effects of bankruptcy flag removal. Section 4 describes the main results. Section 5 estimates the long-run effect of bankruptcy flag removal. Section 6 examines the implications of the results for monetary policy. Section 7 concludes.

2. Background on Bankruptcy Flags and the 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.[8]

We exploit rules imposed by the 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

[8] See Avery et al. (2003) for more information on consumer credit records.

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.

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. 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.[10]

Table 1 presents summary statistics for the paper's main sample and, to facilitate comparison, for a one-percent random sample of consumers in the CCP.[11] 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 trades, \$76,000 in balances, and \$85,000 in credit limits and original principal on open trades 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.

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[10] 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.

[11] 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.

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-bankruptcy consumers have relatively little debt in collections trades, 7 percent of their open trades 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. Nonetheless, few of them are close to their credit limit. We discuss below the extent to which estimates of the MPC in the bankruptcy flag sample are informative about the aggregate MPC.

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.[12] 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.

[12] 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 issue, we present our main IV 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.

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

$$y_{it} = \gamma_t + \gamma_c + \sum_{\tau \in T} \delta_\tau \cdot I\{r_{it} = \tau\} + \epsilon_{it}.$$

Here, γ_t represents fixed effects for calendar quarter and γ_c represents fixed effects for each flag-removal cohort based on the year in which their flag was removed. For the set of mutually exclusive and exhaustive lead and lag indicators, T, we include indicator functions for 24 months before flag removal and 24 months after flag removal.[13] We then plot estimates of δ_τ, the change in the outcome of interest over event time. Such an 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.

A drawback to this 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 (Han, Keys, and Li 2015; 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 flag removal occurs at the same time relative to bankruptcy filing for all consumers, and is not randomly assigned, the non-parametric event study cannot account for such trends. To account for pre-trends, we complement the approach above with a parametric event-study regression that controls for a linear pre-existing time trend.

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

[13] We pool the first three indicator variables in *T,* representing 24, 23, and 22 months prior to flag removal, thus assuming that outcomes during those three months are identical. That restriction is necessary to avoid multicollinearity and to identify an underlying data generating process (Borusyak and Jaravel, 2016). To ensure that that restriction is not pivotal, we have experimented with alternative normalizations, all of which have led to similar results.

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

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.[14] 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 an additional advantage of this second approach is that it 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.

Finally, 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.[15] To implement the instrumental-variables (IV) specification, we jointly estimate the effect of bankruptcy flag removal on the outcome of interest and also on credit scores using a seemingly-unrelated-regressions model. We then construct IV estimates as the ratio of the reduced-form effect of flag removal to the first-stage estimate on credit scores at various months after removal. This allows us to estimate dynamic effects of bankruptcy flag removal in a single empirical model. The tables that follow present IV estimates that describe the change in credit outcomes for a ten-point increase in credit scores.

[14] This approach is similar to that taken by Dobkin et al. (2016), who report both non-parametric event-study estimates and parametric estimates.

[15] We assume that the reduced-form effect of bankruptcy flag removal on borrowing comes entirely through its effect on credit scores, and thus use flag removal as an instrument for credit scores. We rely on the fact that the ten-year rule is an artifact of credit reporting regulations, and does not reflect an underlying discontinuous shift in consumers' circumstances which would cause them to borrow differently in the absence of the change in credit score. Our identifying assumptions are similar to those underlying a regression discontinuity design where the running variable is time relative to flag removal.

A final complication 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 effect of flag removal on the number of new trades opened in the 6 months after flag removal as the sum of the first six event-study estimates: $\hat{\delta}_1 + \hat{\delta}_2 + \hat{\delta}_3 + \hat{\delta}_4 + \hat{\delta}_5 + \hat{\delta}_6$. We apply this approach solely for outcomes that are based on the number of new trades opened in each month or the number of inquiries in each month.

To calculate the MPC out of liquidity, we divide the effect of flag removal on new credit card balances by its effect on 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 calculate the associated standard errors using the delta method.[16] 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 the variable 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.

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.

[16] These standard errors are conservative, in that we perform our analysis on aggregated cell means rather than the underlying, individual-level data, by calculating means for each bankruptcy flag removal cohort and calendar month.

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.[17] 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.[18] 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-

[17] In some of the figures, the outcomes appear to follow three-month cycles. Those cycles are an artifact of the data construction and normalization. "Stock" outcomes such as credit score and number of open trades 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.

[18] 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 quarter 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.

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 Bankruptcy Flag Removal on Borrowing

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 trades opened each month. Panel A shows a sudden and striking increase in the number of new trades opened per month after flag removal. The rate of new trade 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 trades.[19]

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 trades per ten-point increase in credit scores. To measure the cumulative impact of flag removal on borrowing, we integrate the effects over new trade openings during the first 6 and 12 months after flag removal.[20] In column 1, the table shows that for each 10-point change in credit score after flag removal, consumers opened 0.13 new trades in the first 6 months and took on $489 in balances and received $927 in principal and limits on these new trades. 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 respond to changes in credit score. Figure 3 shows the effects on new credit card trades. It suggests that a

[19] In these summary measures, we include all types of credit trades 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.

[20] 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 trades is the *sum* of the first 6 coefficients from the event-study specification when the total balance on new trades opened in each month is the dependent variable divided by the estimated change in credit scores at 6 months.

large share of the increase in new trades in Figure 2 is driven by credit cards. As shown in column 2 of Table 3, consumers take out 0.099 additional credit card trades 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 trades 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 trades and loan principal on new trades for these types of loans, consistent with the results for credit cards and overall credit. The third and fourth columns of Table 3 present IV estimates for these products. Panel A suggests that the number of new mortgage and new auto trades increase by much less than new credit card trades, which is unsurprising given the size of these loans and the relative infrequency of large asset purchases. However, the small increase in new trades 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 refinancing of past loans or whether it represents novel borrowing. To answer that question, we apply the same research design to open credit card trades instead of new trades. 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. By contrast, we estimate an MPC out of liquidity of 0.23 using balances and credit limits on open trades in the first six months after flag removal, and an MPC of 0.28 in the first twelve months after flag removal, which is similar to the magnitude for our main results.[21] As a result, we conclude that a large fraction of the balances

[21] We can measure the timing of new trade opening with precision because our data contain fields for the exact date of origination for each trade. However, this information is often reported with lags. The median reporting lag is 16 months for new trades. While we correct for reporting lags in the measurement of new trades by tracing each trade back to its origination date, we cannot do so for open trades because the open/closed status of each trade evolves dynamically and can only be measured for trades that have begun reporting. Thus, we expect the effects of flag removal on open trades to be significantly biased downward. We estimate that the effect of flag removal on open credit card balances to be about 40-60% of that for new trades in the first 6–12 months, which

accrued on new credit card trades following bankruptcy flag removal represents a net increase in credit card debt, as opposed to balance transfers from existing cards.

4.3. The Marginal Propensity to Consume Out of Liquidity 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.[22] Overall, we estimate an MPC of 0.37, suggesting that consumers borrow 37 percent of the increased credit card limits offered to them once their bankruptcy flags are removed. That estimate is similar to previous estimates for sub-prime 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 between 2004 and 2006. The MPC then rose significantly, 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, closer to pre-recession levels. While our earlier results show that consumers take up significant amounts of new credit between six and twelve 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

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is consistent with a large majority of new credit representing net increases in borrowing after accounting for reporting lags.

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

of unsecured credit for subprime consumers which failed to recover after the recession.[23] 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 2008–2011. 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 a robustness analysis, below.

4.4. Robustness Analysis and Threats to Validity

We next probe whether the analysis above credibly isolates the changing MPC out of liquidity over time. In particular, we test two alternative interpretations of the results: (1) that the changing MPC over time is driven partly by a non-linear response of borrowing to credit limits (so that the average MPC varies with the change in credit limit), and (2) that the changing MPC is driven partly by compositional differences across flag-removal cohorts.

We first address the functional-form assumption. Suppose that the MPC out of liquidity depends on the magnitude of the increase in credit limits, so that consumers borrow differently out of small increases in their credit limits than out of large increases.[24] That possibility would complicate our interpretation of the changing MPC out of liquidity over time, since we find evidence that the effect of bankruptcy flag removal on credit limits varies over time.

To investigate this possibility, we pursue the following empirical strategy, designed to "partial out" changes in the credit limits from the MPC. 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 observe each year once we have "partialled

[23] 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).

[24] 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.

out" the effect of changes in credit limits on the estimated MPC. Appendix Figure A1 plots those residuals. The figure still suggests an inverse-U-shaped pattern, with the observed MPC peaking during the Great Recession.

A second key concern with the analysis above is that the composition of consumers having their bankruptcy flags removed may also change over time. Overall, we observe higher MPCs for individuals with lower credit scores, which we report in Table 5.[25] Therefore, any cyclical variation in average credit scores could potentially account for some of the changes over time in the average MPC we calculate. As a result, our estimates could potentially confound changes in the demographics of underlying consumers experiencing flag removal with changes in the average MPC holding the composition of consumers constant.

To investigate that concern, we test whether the observable characteristics of flag-removal cohorts can explain the changing average MPC. 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 trades 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 these observable dimensions. Appendix Figure A2 presents the estimates of MPC by year after re-weighting and suggests a roughly similar pattern as in Figure 5.[27] The broad similarity between these figures suggests

[25] Consistent with the work of Gross and Souleles (2002) and Aydin (2016), the results in Table 5 also suggest a higher MPC for individuals with higher utilization.

[28] For this analysis, we start with the universe of 921,198 credit card acquisition offers sent by mail to consumers between 2002 and 2014 gathered by Mintel, a marketing research firm, and linked to the credit scores of those receiving these offers. The Mintel credit card data are described in more detail by Han, Keys, and Li (2013) and

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 average MPC over the business cycle. By holding constant mortgage balances, credit scores, and other financial characteristics, our results suggest that the deterioration of household balance sheets during the recession may play a relatively less important role than aggregate macroeconomic conditions in accounting for our results. As a result, our results may generalize to other recessions, not just recessions following financial crisis.

A final concern involves potential changes in borrowing costs that might occur at the same time as bankruptcy flag removal. In particular, the increase in credit scores upon flag removal may trigger a decrease in offered interest rates, which would confound our analysis of the MPC out of liquidity.

Unfortunately, we are not able to address this concern in the CCP data because it does not include interest rates. In a complementary dataset of credit card mail offers, we calculate that a 10-point increase in credit scores is associated with a 33-basis-point drop in the regular purchase APRs on new credit cards.[28] This association is similar to those reported in previous studies (Agarwal et al. 2015, Han et al. 2015). Published estimates of the elasticity of debt to the interest rate, in turn, suggest that the drop in interest rates would lead to a long-run increase of $57 in credit card borrowing.[29] As we discuss below, flag removal is associated with a one-time, persistent increase in credit score, which should be associated with a one-time increase in balances if it were driven purely by a price effect. However, we show that flag removal is instead associated with a permanent increase in the *flow* of new credit, which is more consistent with credit access rather than prices as the main driver of increased borrowing. As shown in Table 7, credit card borrowing increases by $462 ($945) as of 24 (60) months after

Ru and Schoar (2016). We calculate this association for credit cards issued to consumers with credit scores between 600 and 700.

[28] For this analysis, we start with the universe of 921,198 credit card acquisition offers sent by mail to consumers between 2002 and 2014 gathered by Mintel, a marketing research firm, and linked to the credit scores of those receiving these offers. The Mintel credit card data are described in more detail by Han, Keys, and Li (2013) and Ru and Schoar (2016). We calculate this association for credit cards issued to consumers with credit scores between 600 and 700.

[29] See Table III of Gross and Souleles (2002).

flag removal. These numbers suggest that at most 6–12 percent of the increase in borrowing we observe can be explained by a drop in interest rates after bankruptcy flag removal.

Moreover, this bias varies relatively little over the business cycle and so can explain little of the change in the estimated MPC over the business cycle. The association between a 10-point increase in credit scores and credit card interest rates varies from a low of 2.2 percent in 2007 to a high of 4.9 percent in 2012. That translates into an increase in credit card borrowing from $37 to $83, all of which is under 20 percent of the increase in borrowing we observe.

4.5. 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, perfectly forward-looking consumers would avoid applying for credit in the months just prior to flag removal, resulting in a "missing mass" of new trades 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.

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 consumers, on average, react to the approaching flag removal. Thus, we interpret the main estimates as capturing consumer responses to an *unanticipated* change in credit supply following removal of bankruptcy flag.

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).[31] 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

[31] "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 not related to credit demand, are not included in our dataset.

only 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 6 presents our main specification when inquiries per month are the outcomes of interest, and Panel A of Table 6 presents the associated point estimates. We find no statistically significant changes in mortgage and auto inquiries resulting from flag removal, consistent with flag removal being unanticipated. The rate of credit card inquiries does increase significantly, albeit less than the increase in new trades. Because many credit card applications result from direct mail and other forms of marketing by issuers, which in turn are targeted in part 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 versus higher approval rates for each application, we examine the number of new trades per inquiry as a proxy for lenders' approval rate. These results are presented in the second column of Figure 6 and in Panel B of Table 6. As noted above, our inquiry data under-estimate the true number of applications, so the average number of new trades 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 trades 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 6 suggest that over 70% of the increase in all new trades and about two thirds of the increase in new credit card trades can be explained by an increase in approval rates as opposed to an increase in inquiries.[33]

[33] We can estimate the effect of the increase in approval rates by multiplying the increase in trades 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 trade openings for all trade types over the first six months following flag removal is 0.12 = (0.126 trades / 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

These results support the interpretation that our main estimates are driven primarily by a change in credit supply rather than a change in borrower behavior. Furthermore, Appendix Figure A3 shows that credit card trades per inquiry exhibited a sharp decline from 2004 to 2006, but remained relatively constant from 2006 to 2011, providing further evidence that variation in credit supply is unlikely to explain the pattern in the MPC that we document.

5. 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. The consumers in this sample, of course, have a history of bankruptcy, and so their overall credit risk is high.[34] But it is unclear, a priori, whether an increase in their credit scores 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 increases in delinquencies and a reversion of credit scores toward pre-flag-removal levels. However, if new credit helps alleviate consumers' 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-run trends in delinquency, borrowing, and credit scores. Figure 7 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

rates can account for 91% of the increase in new trades for all trade types in the first six months after flag removal.

34 From Table 1, 7 percent of new trades reported within one year of opening are 90+ days delinquent, and 4 percent of all open trades 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.

all open loans, collection inquiries, and new collections balances.[35] 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 trades 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 8 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 trades, and suggests a small decrease in delinquencies over the longer run. The increase in the flow of new credit card trades, balances and limits persists for at least five years after flag removal. Table 7 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.

6. Implications for Stimulus Policy

This section presents a calibration exercise designed to assess the implications of the results for designing stimulus policy that relies on an expansion of consumer credit. In particular, we assess how variations in the MPC out of liquidity over the business cycle can alter the predicted effects of credit expansions. We consider a hypothetical economic policy that would provide \$1,000 in additional credit limits to all U.S. consumers with credit scores under 700. We estimate the total number of U.S. consumers with credit scores under 700 in each year from 2007 to 2009 using the CCP.

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. The fifth column of Table 8 presents the

[35] 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, as of the date of last reporting of a given trade line.

change in aggregate consumption one would expect, given that assumption, for the years 2007–2009. The sixth column of Table 8 describes, by contrast, the change in aggregate consumption one would expect based on each year's average MPC estimate. 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 average 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.

7. 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 high MPC out of liquidity for consumers with relatively low credit scores, consistent with previous work (Agarwal et al 2015). Using a large panel dataset, we show that the average MPC out of liquidity is counter-cyclical, with higher average MPCs during the Great Recession. The cyclical variation is both statistically and economically significant, with the average MPC decreasing by roughly 20–30 percent between 2008 and 2011 as aggregate economic conditions improved. By comparison, this difference in average MPCs 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 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). The results above also suggest substantial heterogeneity in the MPC across consumers, but – more importantly – heterogeneity across the business cycle. Our results therefore suggest that MPCs estimated during "normal" times may provide misleading guidance for policymakers assessing similar interventions during a recession.

Beyond policy guidance, we interpret our results as providing statistics for assessing recent macroeconomic models of household finance. Models featuring costly adjustment of illiquid assets point out that severe recessions can actually cause *lower* average MPCs as compared 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 average MPC out of liquidity was unusually large relative to typical economic times and likely larger than that during mild recessions.

There are several important limitations of our results. First, our results come from a specific sample of former bankruptcy filers. We interpret the results as informative about the MPC out of liquidity for a broad sample of 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 a 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 that the estimated MPC out liquidity actually reflects 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.

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 average MPC. 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 average MPC, especially for low-credit-score consumers who are often the focus of macroeconomic stabilization policy. Such consumers are thought to exhibit especially high MPCs, and, it seems, may exhibit even higher MPCs during severe recessions.

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Table 1. Summary Statistics

This table presents summary statistics for the analysis sample used in following regressions 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 bankruptcy 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 trades	4.8	5.3
Balances on open trades	$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 trades	$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 right-hand-side variables consist of a control for the number of months till flag removal; indicator variables for the 24 months after flag removal; a fixed effect for flag removal cohort; and a fixed effect for each calendar month. Standard errors are clustered on flag-removal-month cohorts and associated p-values are in brackets.

	(1) All	(2) 2005	(3) 2007	(4) 2009	(5) 2011
6-month effect	15.455	13.223	15.719	15.774	16.925
	(0.513)	(3.322)	(2.184)	(2.395)	(2.372)
	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]
12-month effect	16.426	11.240	16.693	16.796	18.903
	(0.562)	(3.451)	(2.655)	(3.242)	(3.007)
	[0.000]	[0.008]	[0.000]	[0.000]	[0.000]
Pre-removal mean	616	619	622	612	608

Table 3. Effect of Bankruptcy Flag Removal on New Trades

Each point estimate is a ratio of the effect of flag removal on the given outcome divided by the effect of flag removal on credit scores, with standard errors in parentheses clustered on bankruptcy-flag cohort and calculated via the delta method, and associated *p*-values in brackets. The underlying regressions include a control for the number of months till flag removal; indicator variables for the 24 months after flag removal; a fixed effect for flag removal cohort; and a fixed effect for each calendar month. One can interpret the point estimates as describing the change in the given outcome after flag removal per 10-point change in credit scores.

	(1) All	(2) Cards	(3) Mortgage	(4) Auto	(5) Other
			A. Number of new trades		
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 trades		
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 trades		
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

For panels B and C, each point estimate is a ratio of the effect of flag removal on the given outcome divided by the effect of flag removal on credit scores, with standard errors in parentheses clustered on bankruptcy-flag cohort and calculated via the delta method, and associated *p* -values in brackets. Panel A is based on the same structure, though with the numerator being the effect of flag removal on credit-card balances and the denominator being the effect of flag removal on credit-card limits. The underlying regressions include a control for the number of months till flag removal; indicator variables for the 24 months after flag removal; a fixed effect for 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	0.371	0.335	0.332	0.348	0.445	0.461	0.410	0.352	0.383	0.021
effect	(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	0.373	0.320	0.355	0.343	0.463	0.480	0.454	0.376	0.362	0.013
effect	(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	152.363	233.202	267.159	209.076	233.305	106.515	49.552	49.734	49.153	0.000
effect	(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	289.975	442.039	557.679	365.989	397.920	173.702	106.443	95.180	80.242	0.000
effect	(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	410.820	695.784	805.773	601.296	523.751	231.190	120.848	141.222	128.396	0.000
effect	(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	778.102	1379.660	1572.879	1067.508	859.622	361.621	234.565	253.384	221.809	0.000
effect	(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 and Income

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. Income groups: under $51,150, between $51,152 and $60,807, and greater than $60,807. Utilization groups: 0–36 percent, 36–88 percent, and greater than 88 percent.

	(1) Low	(2) Medium	(3) High
		A. Stratified by Credit Score	
6-month effect	0.395	0.423	0.307
	(0.030)	(0.022)	(0.057)
	[0.000]	[0.000]	[0.000]
12-month effect	0.406	0.397	0.294
	(0.024)	(0.018)	(0.056)
	[0.000]	[0.000]	[0.000]
		B. Stratified by Median Tract Income	
6-month effect	0.392	0.375	0.430
	(0.033)	(0.040)	(0.025)
	[0.000]	[0.000]	[0.000]
12-month effect	0.396	0.361	0.421
	(0.027)	(0.032)	(0.021)
	[0.000]	[0.000]	[0.000]
		C. Stratified by Utilization	
6-month effect	0.297	0.476	0.490
	(0.024)	(0.029)	(0.040)
	[0.000]	[0.000]	[0.000]
12-month effect	0.287	0.467	0.492
	(0.020)	(0.024)	(0.032)
	[0.000]	[0.000]	[0.000]

Table 6. Effect of Bankruptcy Flag Removal on Inquiries and Trades Per Inquiry

Each point estimate is a ratio of the effect of flag removal on the given outcome divided by the effect of flag removal on credit scores, with standard errors in parentheses clustered on bankruptcy-flag cohort and calculated via the delta method, and associated p-values in brackets. The underlying regressions include a control for the number of months till flag removal; indicator variables for the 24 months after flag removal; a fixed effect for flag removal cohort; and a fixed effect for each calendar month. One can interpret the point estimates as describing the change in the given outcome after flag removal per 10-point change in credit scores.

	(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 7. 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 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 8. Policy Simulation

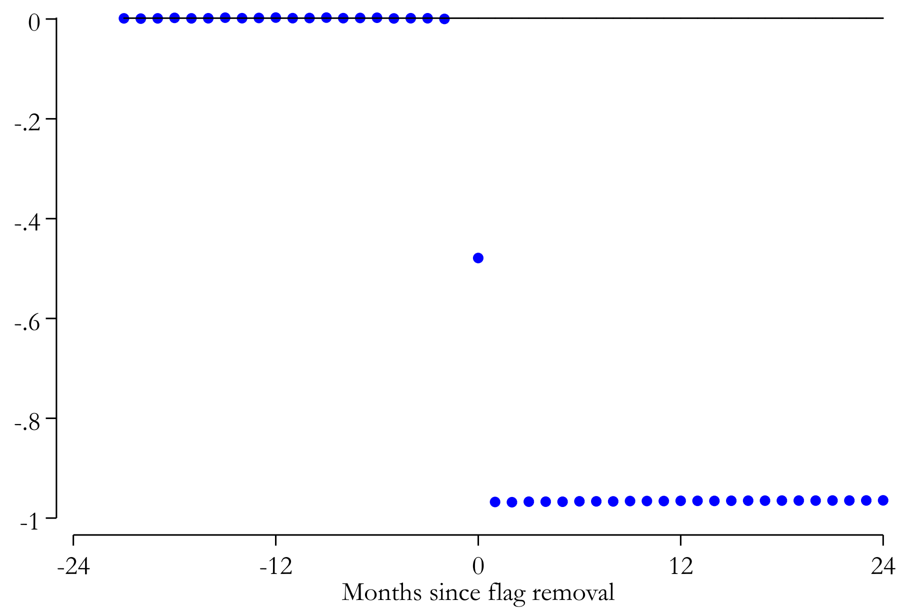
This table presents an illustrative simulation of a hypothetical stimulus policy that increases credit limits by $1,000 for all Americans with credit scores under 700. The predicted spending impacts are based on the MPC estimates from Table 4, Panel A.

	(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 time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.

A. Bankruptcy Flag on Record



B. Credit Score

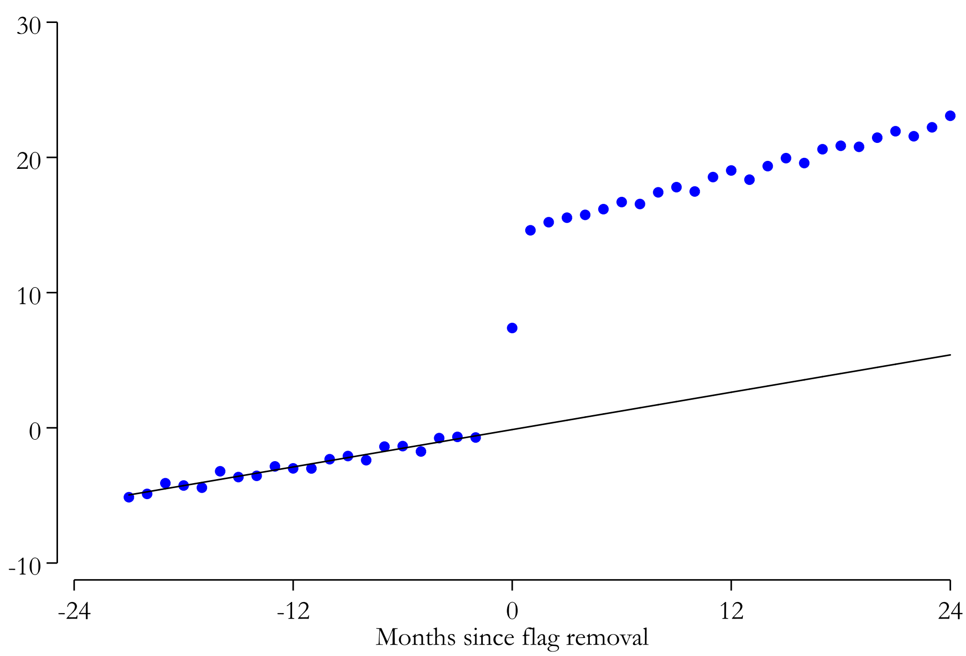


Figure 2. Effect of Bankruptcy Flag Removal on Summary Outcomes

The circular markers in the figure plot the estimated effects of event time, controlling for calendar time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.

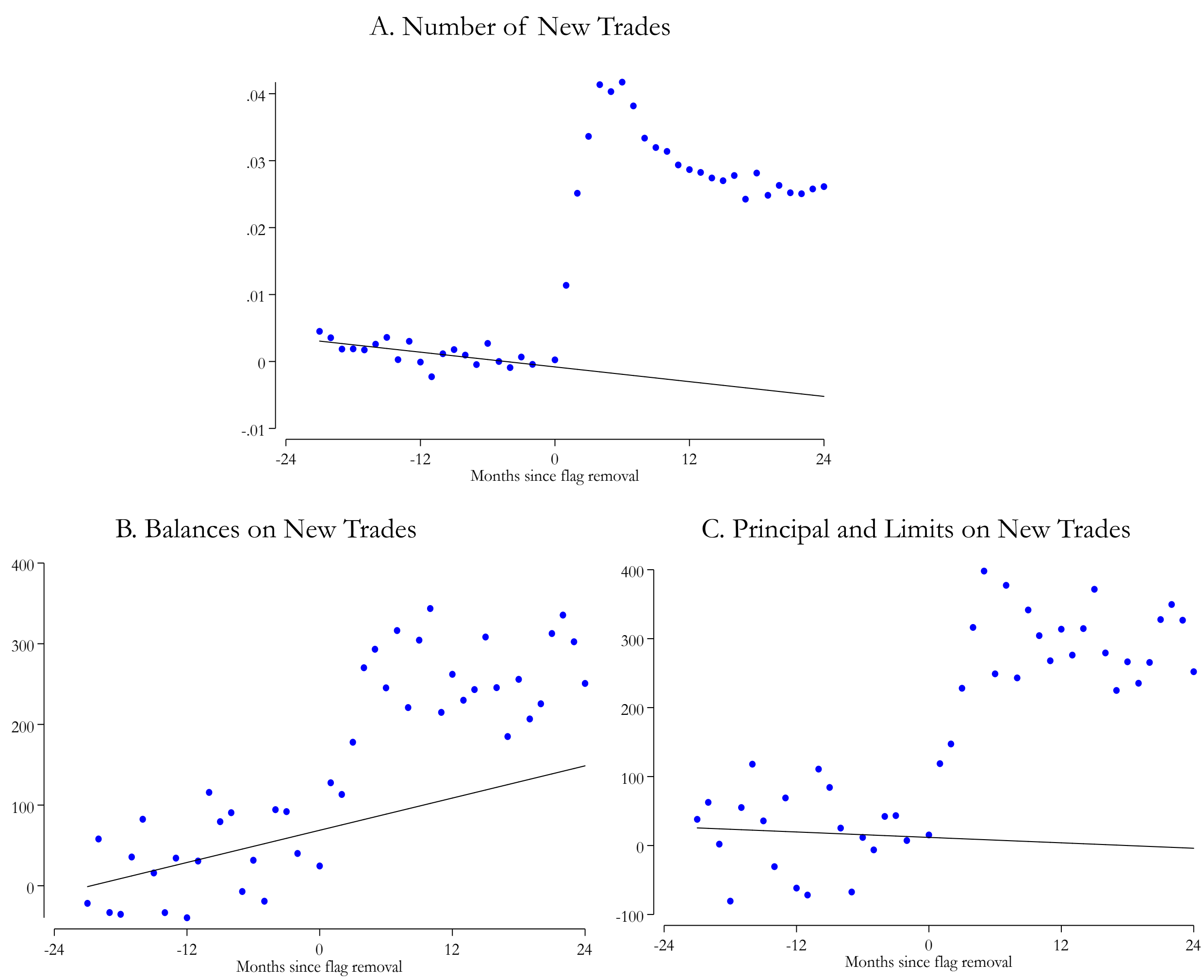
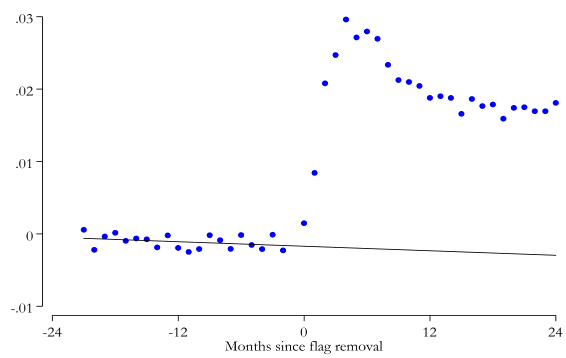


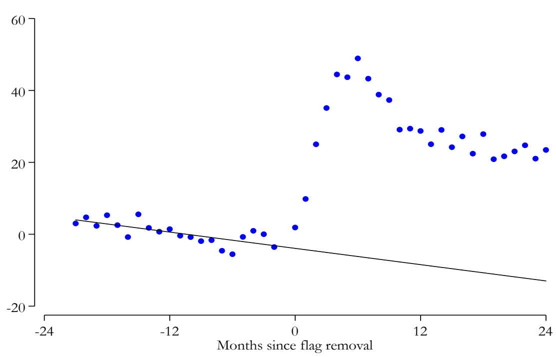
Figure 3. Effect of Bankruptcy Flag Removal on Credit Cards

The circular markers in the figure plot the estimated effects of event time, controlling for calendar time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.

A. Number of New Credit Card Trades



B. Balances on New Credit Cards



C. Credit Limits on New Card Trades

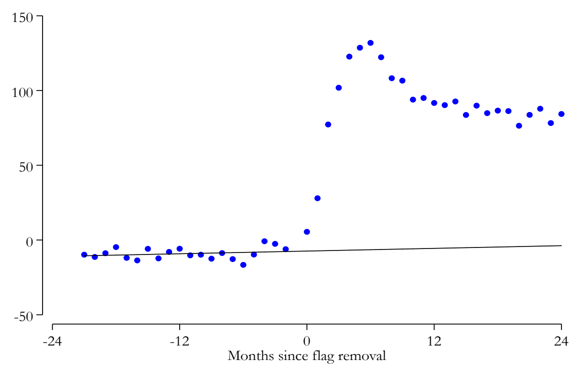


Figure 4. Effect of Bankruptcy Flag Removal on Auto Loans and Mortgages

The circular markers in the figure plot the estimated effects of event time, controlling for calendar time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.

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.

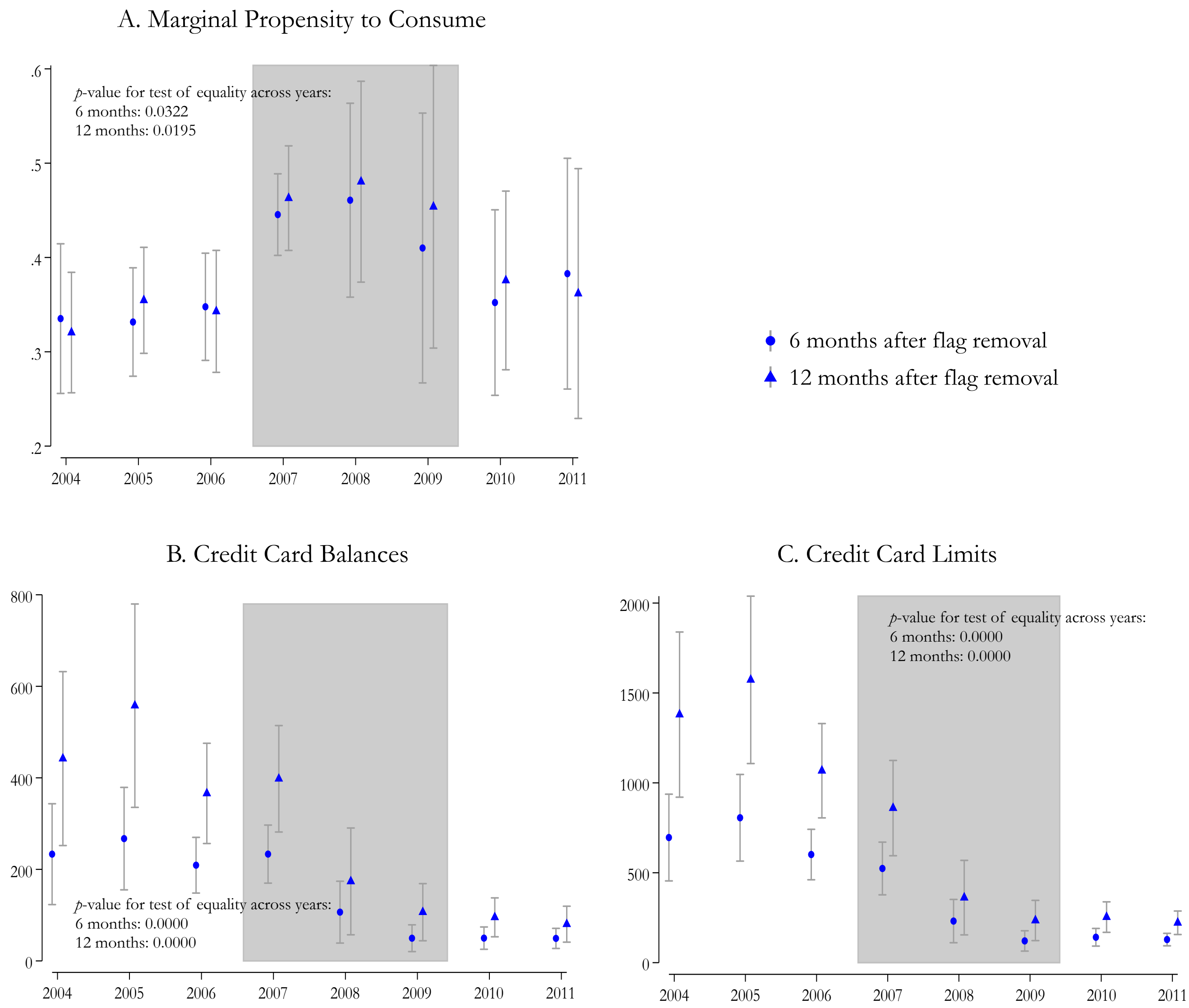


Figure 6. Effect of Bankruptcy Flag Removal on Inquiries and New Trades per Inquiry

The circular markers in the figure plot the estimated effects of event time, controlling for calendar time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.

Figure 7. Effect of Bankruptcy Flag Removal on Delinquency

The circular markers in the figure plot the estimated effects of event time, controlling for calendar time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.

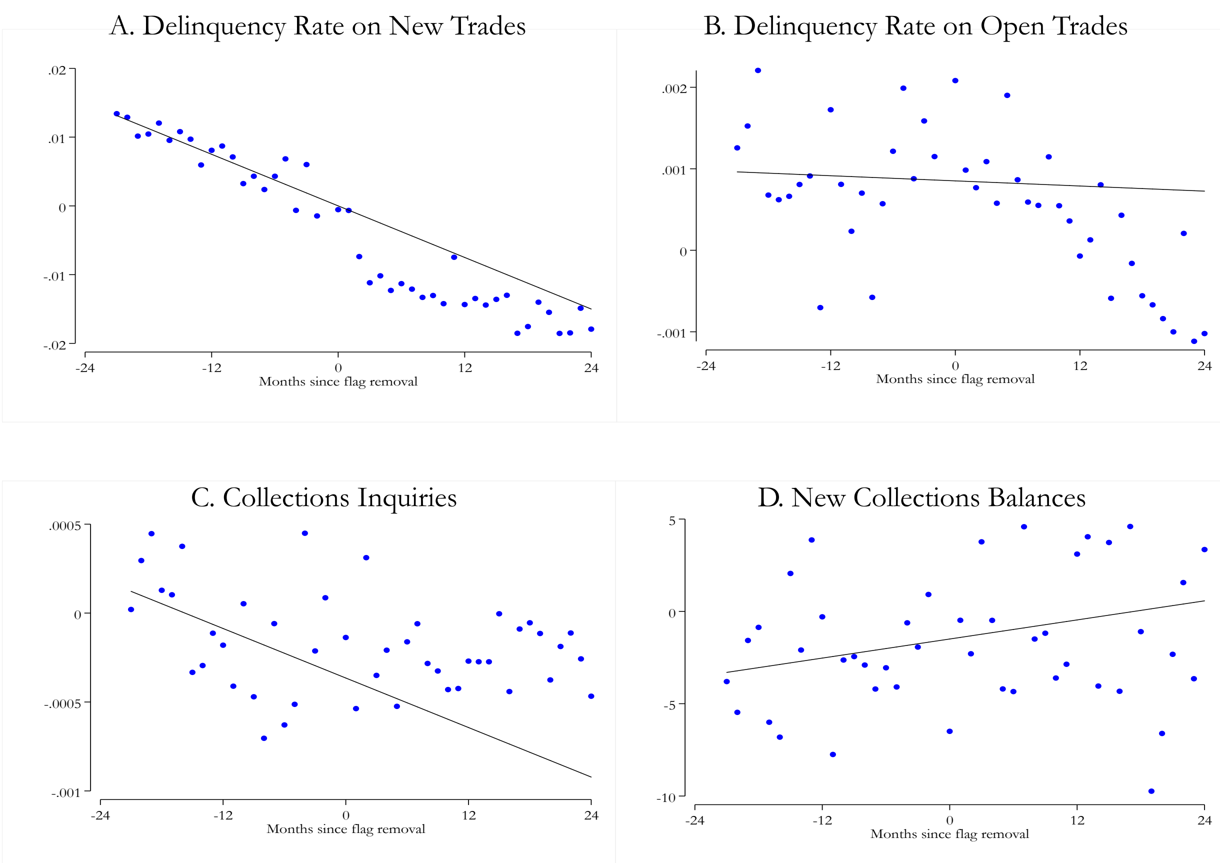
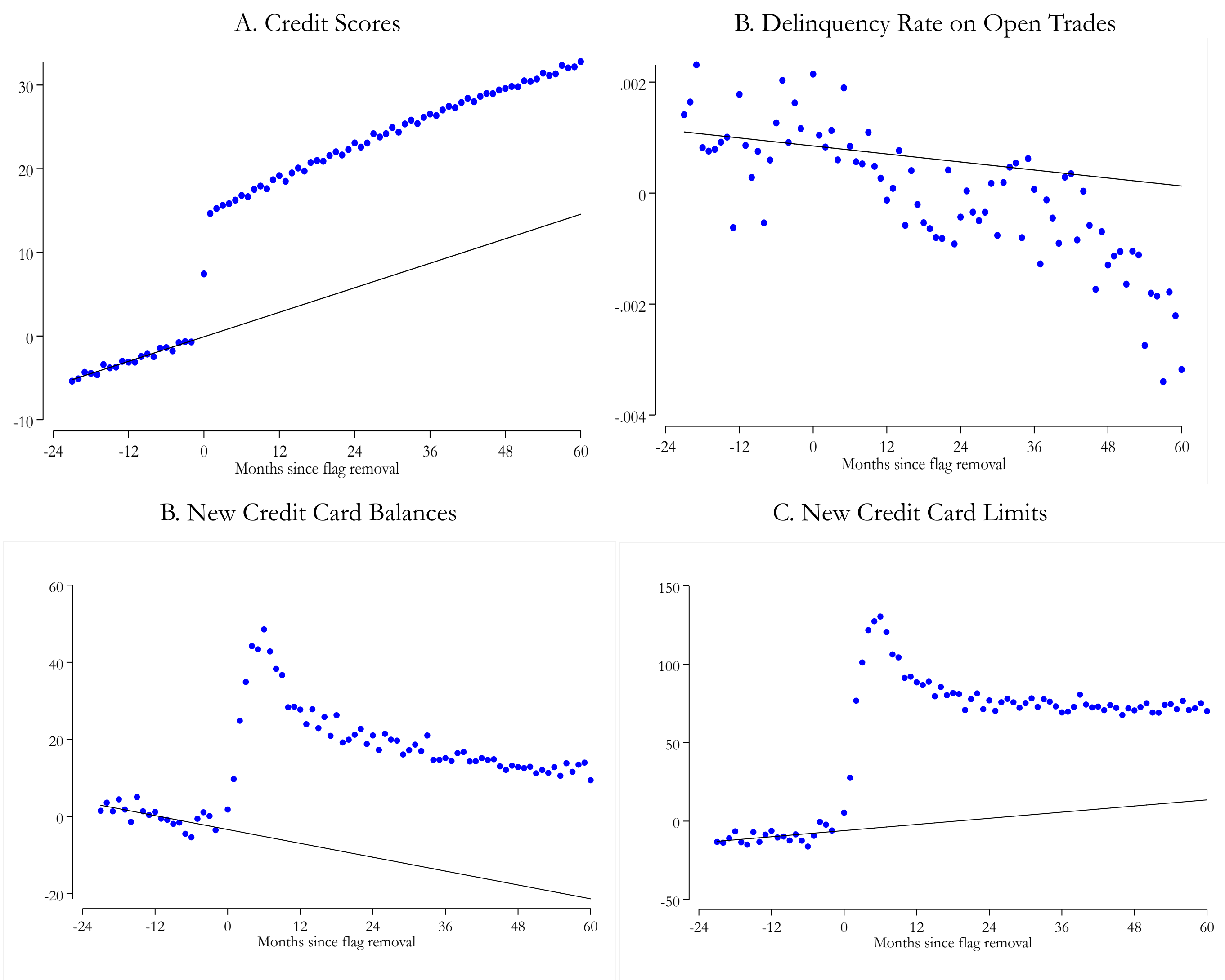


Figure 8. Long-Run Changes in Outcomes

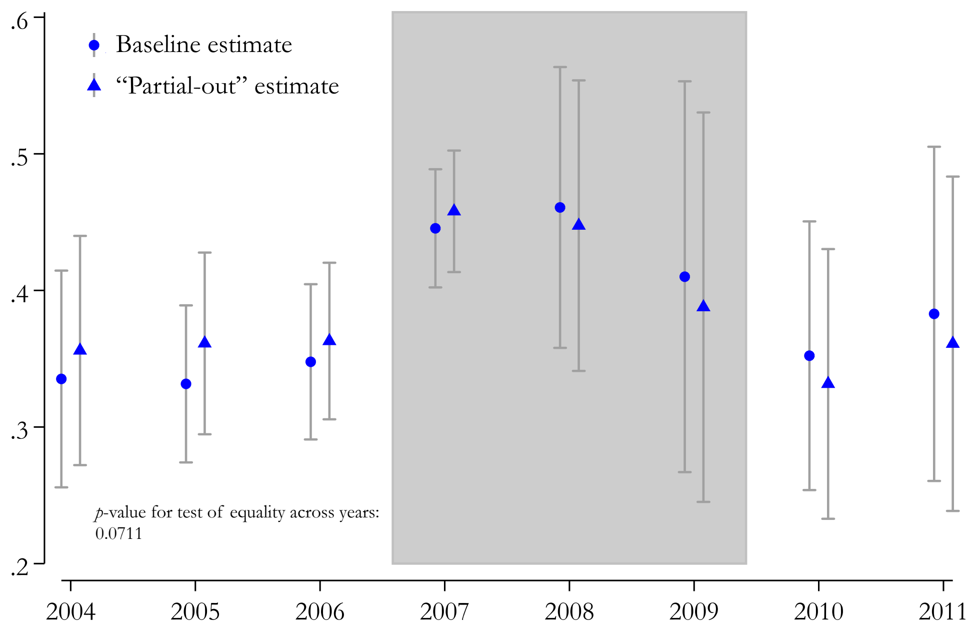
The circular markers in the figure plot the estimated effects of event time, controlling for calendar time and flag-removal cohort. The three omitted time periods are -24, -23, and -22. The solid line is an OLS regression line fit to all pre-period event-study estimates.



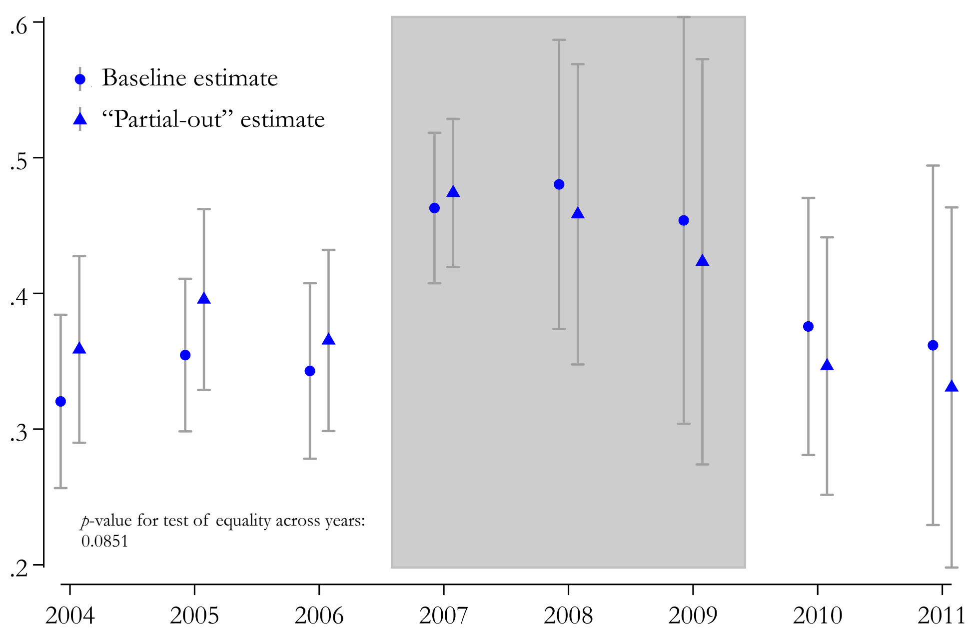
Appendix Figure A1. Estimates of the MPC Once Credit-Limit Effect Partialled Out

These figures present estimated MPC out of liquidity by year, adjusting for the changing credit limits. See text for details.

A. 6-Month Estimates



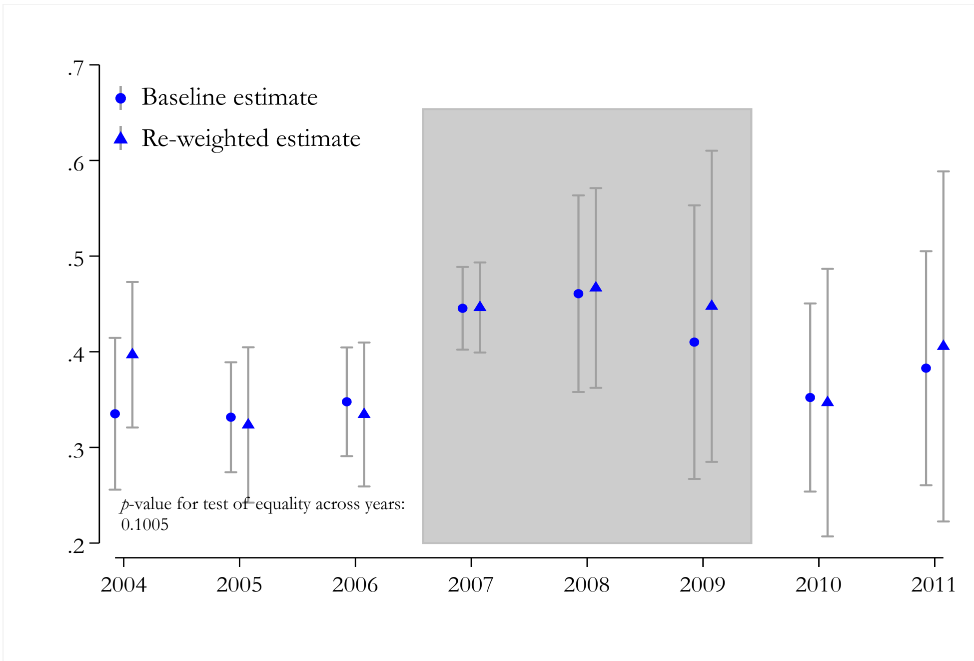
B. 12-Month Estimates



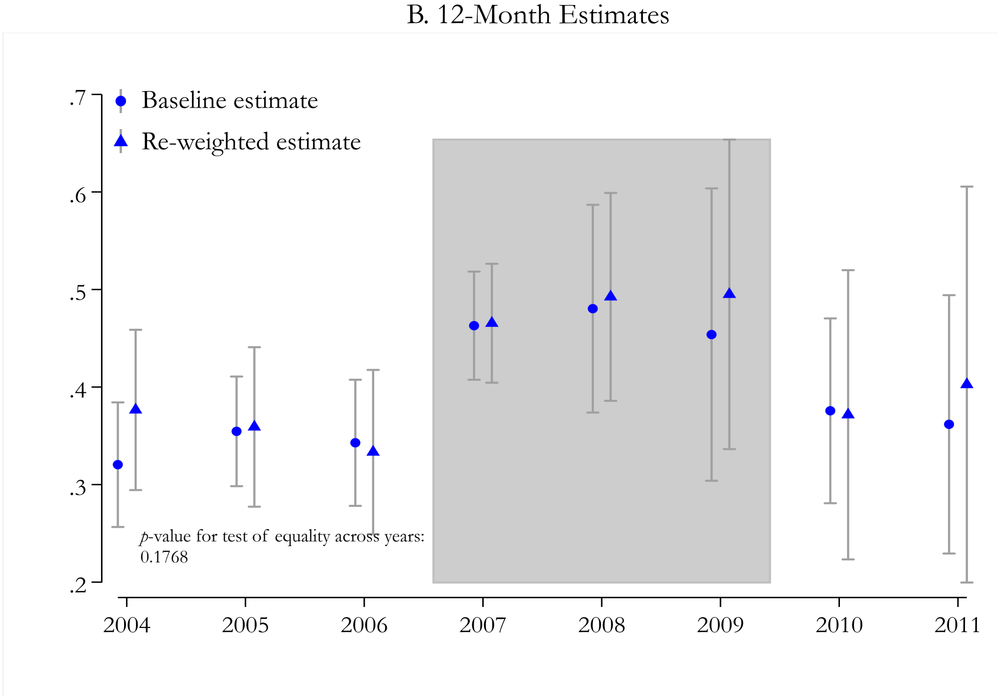
Appendix Figure A2. Re-weighting to Match 2008 Characteristics

These figures present estimated MPC out of liquidity by year once the sample has been re-weighted to match the financial characteristics of those who have bankruptcy flags removed in 2008. See text for details.

A. 6-Month Estimates



B. 12-Month Estimates



Appendix Figure A3. Credit Card Trades per Inquiry Over Time

These figures present the effect of bankruptcy flag removal on credit-card trades per inquiry 6 months and 12 months after flag removal, by year. See text for details.