

Bad Credit, No Problem? Credit and Labor Market Consequences of Bad Credit Reports*

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Abstract

We study the effects of credit reports on financial and labor market outcomes. We use a difference-in-differences research design that compares changes in outcomes over time for Chapter 13 filers, whose personal bankruptcy flags are removed from credit reports after 7 years, to changes for Chapter 7 filers, whose personal bankruptcy flags are removed from credit reports three years later. Using credit bureau data, we show that the removal of the Chapter 13 bankruptcy flag leads to a large increase in credit scores, and economically significant increases in credit card balances and mortgage borrowing. Using administrative tax records linked to personal bankruptcy records, we estimate a precise zero effect of improved credit reports on employment and wage outcomes. We conclude that credit reports are important for credit market outcomes, where they are the primary source of information used to screen applicants, but are of limited consequence for labor market outcomes, where employers rely on a much broader set of screening mechanisms.

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1 Introduction

The increasing availability and richness of credit report data is one of the most significant changes to consumer financial markets in the last 25 years. In the United States, credit reports — and the associated credit scores — are used in nearly all consumer lending decisions, including both approval and pricing decisions for credit cards, private student loans, auto loans, and home mortgages. Credit reports are also widely used in non-lending decisions, such as rental decisions for apartments and hiring decisions in the labor market.¹

Supporters of the increased availability and richness of credit reports argue that this trend is a key factor driving the expansion of lending to traditionally underserved segments of the population, including minority communities that have been historically shut out of formal credit markets (e.g., [Staten, 2014](#)). Critics recognize the importance of credit report data in theory, but argue that these benefits should be weighed against individuals' rights to privacy ([Shorr, 1994](#)). There are also growing concerns that the widespread use of credit reports might unfairly punish individuals for mistakes made many years ago.

Critics have been particularly concerned about the use of credit reports in hiring decisions in the labor market. In the years after the Great Recession, a series of prominent news articles reported on how a bad credit report can be a major impediment to finding a job.² Talking to the *New York Times*, a lawyer at the National Consumer Law Center called the scenario a “a bizarre, Kafkaesque experience ... Someone loses their job, so they can't pay their bills — and now they can't get a job because they couldn't pay their bills because they lost a job? It's this Catch-22 that makes no sense.” Indeed, since 2007, eleven states have passed laws to restrict employer credit checks and, in 2015, a similar law was introduced at the federal level.³ Determining whether the anecdotal evidence linking credit reports to employment represents a broad-based, causal phenomenon is an important empirical question.

This paper estimates the causal effect of improved credit reports on credit and labor market outcomes. Our research design uses the sharp removal of personal bankruptcy “flags” from credit reports

¹The Society of Human Resource Management reported that 60 percent of employer conduct background checks for some of their candidates in 2010, up from 25 percent of employers in 1998. See ([FRB, 2007](#)) and [CFPB \(2012\)](#) for additional discussion on the uses of credit reports.

²See [National Public Radio \(2012\)](#) and [New York Times \(2013\)](#). The April 10th, 2016 episode of the TV Show “Last Week Tonight with John Oliver” also reported on this issue.

³The federal bill, “The Equal Employment for All Act” (H.R. 321), aimed to “amend the Fair Credit Reporting Act to prohibit the use of consumer credit checks against prospective and current employees for the purposes of making adverse employment decisions.” The bill was introduced by Senator Elizabeth Warren in August, 2015. See [Clifford and Shoag \(2016\)](#) for more on these policies.

at statutorily determined time horizons. Nearly all households that file for bankruptcy file under Chapter 13 or Chapter 7 of the Bankruptcy Code.⁴ Under the Fair Credit Reporting Act (FCRA), credit bureaus are required to remove Chapter 7 bankruptcy flags ten years after filing. In contrast, credit bureaus traditionally remove Chapter 13 flags only seven years after filing, three years before the Chapter 7 flag is removed.⁵

We use this variation in a difference-in-differences research design that compares outcomes for Chapter 13 filers (the “treatment” group), who have their flags removed at 7 years, to Chapter 7 filers (the “control” group), who have their flags removed at 10 years and are therefore unaffected at the 7-year time horizon. The identifying assumption for this difference-in-difference specification is parallel trends — that is, in the absence of the Chapter 13 bankruptcy flag removal, outcomes for treated individuals, who experience a flag removal, and control individuals, who do not, would have evolved in parallel. To provide support for this assumption, we show that the path of outcomes for treated and control individual are virtually identical in the pre-flag removal period.

We measure the effects of flag removal using two large administrative datasets. We examine the effects on credit market outcomes — including credit scores and measures of both credit card and mortgage borrowing — using a dataset generated from the Federal Reserve Bank of New York Equifax Consumer Credit Panel (CCP). Equifax is one of the three main credit bureaus, and their data provides us with panel information on the nearly all credit products held by an individual over time. We examine the effects on labor market outcomes using data from individual bankruptcy filings merged to administrative tax records at the Social Security Administration (SSA). Our primary analysis sample, which focuses on individuals with a bankruptcy flag removal, covers roughly five hundred thousand individuals in the Equifax sample and 4.6 million individuals in the SSA sample, both over a period of 16 years.

We begin our analysis by examining the effect of the bankruptcy flag removal on credit scores. Since credit score are used in the vast majority of lending decisions, improvements in credit scores directly translate into increased credit availability, lower interest rates, or both. We show that bankruptcy

⁴Under Chapter 7, debtors forfeit all non-exempt assets in exchange for a discharge of eligible debts and protection from future wage garnishment. Under Chapter 13, filers propose a three- to five-year plan to repay part of their unsecured debt in exchange for a discharge of the remaining unsecured debt, protection from future wage garnishment, and protection of most assets. Nearly all unsecured debts are eligible for discharge under both chapters, including credit card debt, installment loans, medical debt, unpaid rent and utility bills, tort judgments, and business debt. See [Dobbie and Song \(2015\)](#) for additional details of the bankruptcy system in the United States.

⁵Under FCRA, Chapter 13 flags are not mandated to be removed earlier than Chapter 7 flags, but credit bureaus do so voluntarily. All three national credit bureaus state that Chapter 13 flags are removed at 7 years in their documentation, and we have confirmed this independently in the credit report data.

flag removal leads to an immediate 12 point increase in credit scores. The jump occurs precisely in the quarter of bankruptcy flag removal and declines over time.

We next show that flag removal has a statistically and economically significant effect on credit card borrowing. The effects appear immediately and grow linearly over time. At a three year horizon, we estimate that flag removal increases credit limits by \$1,430, or 45 percent on a pre-flag removal mean of \$3,143, and raises credit card balances by \$831, or 44 percent on a pre-flag removal mean of \$1,908. The implied marginal propensity to consume (MPC) of 0.58 is large, although we caution that some of the effect may operate through lower interest rates, which we do not observe.

We similarly find large effects of flag removal on measures of mortgage borrowing. The effects again appear immediately, but, in contrast to the credit card results, decline modestly over time. At a one year time horizon, the fraction of individuals with a mortgage increases by 2.3 percentage points, or 5.5 percent on a pre-flag removal base of 41.8 percent. These results are consistent with significant pent-up demand for mortgage borrowing — that is, a mass of consumers who would like to take out a mortgage but are unable to take out a loan or who put off taking out a loan until they can receive better contract terms.

In stark contrast to our credit market results, we estimate a precise zero effect of flag removal on labor market outcomes. At a two year horizon, the 95 percent confidence intervals rules out employment effects larger than 0.3 percentage points, or 0.4 percent on a pre flag removal base of 80.0 percent. We also find that flag removal actually *decreases* log wage earnings over this time horizon by a statistically significant 0.03 log points, or 0.3 percent on a pre flag removal mean of 8.25 log points. Thus, for wage earnings, the 95 percent confidence intervals rule out all positive effects. We also estimate zero effects on an indicator for self employment and an indicator for either formal sector employment or self employment, indicating that these results are not masking reallocation between different types of work.

Because of the contrary anecdotal evidence, we conduct a broad set of heterogeneity and sensitivity analysis of these labor market results. None of the results suggest economically important effects. We estimate precise and small effects for different demographic groups, including for minorities where there has been particular concern about the employment consequences of derogatory credit reports.⁶ We also find no evidence that employment shifts towards industries like finance, which more

⁶For instance, the NAACP and National Council of La Raza, among many other organizations, wrote a letter cosponsoring the “The Equal Employment for All Act” (H.R. 321), which aimed “to prohibited employers from using credit checks as part of their hiring and promotion decisions for most positions,” because they viewed credit checks as discriminatory,

frequently use credit checks to screen applicants (SHRM, 2012). Finally, we find no employment effects in states that did not implement bans on credit checks (or in states that implemented bans). The consistent zero effects across a range of subsamples and outcomes suggest that employers are not basing their hiring decisions primarily on credit scores.

We conclude from this evidence that credit reports are important for credit market outcomes where they are the primary source of information used in screening loan applicants, but are of limited consequence for labor market outcomes where employers use a broad range of mechanisms to screen job applicants so that the impact of an improved credit reports does not have a measurable impact on the margin. Our results also suggest that recent political attempts to limit the use of credit reports by employers are unlikely to affect labor market outcomes, either positively or negatively.

Our paper builds on Musto (2004), who studies the impact of bankruptcy flag removal on credit scores and credit card borrowing using an event-study design. Musto (2004) finds that flag removal has a sharp short run effect on credit scores and credit card borrowing, but has adverse longer run consequences, with higher delinquency levels and lower credit scores at 18 months after flag removal. In contrast, our difference-in-differences research design, which is better suited to study longer run effects, does not show such strong evidence of adverse longer run outcomes. We are also able to examine a much richer set of outcomes, including mortgage loans and labor market outcomes.

Our paper is also related to a number of recent working papers on the link between employment and credit reports conducted in parallel to our study. Bos, Breza and Liberman (2016) study a policy that reduce the time information on default was listed on credit reports in Sweden. Their analysis focuses on individuals who took out a pawnshop loan, which make up approximately four percent of the Swedish population (Bos, Carter and Skiba, 2012). They find that the removal of default information leads to a three percentage point increase in employment. By comparison, we focus on individuals who have filed for bankruptcy, which make up approximately 15 percent of the United States population according to our calculations using the CCP data, and we rule out employment effects greater than 0.3 percentage points with 95 percent confidence. We are also unable to find any subgroup where employment effects are at least 3 percentage points, suggesting that the labor market effects of bad credit reports is much smaller in the United States than Sweden, at least for the respective populations examined in these studies.

A recent working paper by Clifford and Shoag (2016) studies the impact of recent state-level re-

among other reasons. The bill was introduced in January 2011, but did not pass.

restrictions on employer credit checks using difference-in-differences research design. Using employment and credit score data aggregated to the Census tract level, the authors find that restrictions on employer credit checks have no effect, at the mean, for census tracts with average credit scores under 650. The null effect at the mean is driven by large increases in employment in tracts with average credit scores under 620, which represent less than 10 percent of the working population, and modest decreases in employment in tracts with average credit scores between 620 and 650, which represent a much greater portion of the working population. The authors argue that these results are consistent with large employment effects of the credit check bans, with employment being redistributed from workers with “mid-to-low” credit scores to those with “subprime” credit scores. However, the authors are unable to identify specific individuals with low or high credit scores likely to be affected by the credit check bans, and are therefore unable to distinguish the effects of these bans from broader economic trends affecting lower income census tracts during this time period (i.e., 2007 to 2013).

Our paper is also related to a recent working paper by [Herkenhoff, Phillips and Cohen-Cole \(2016\)](#), which uses credit records from TransUnion merged with administrative employment records from the Census to study the impacts of bankruptcy flag removal on employment, earnings, and entrepreneurship. Using an event study research design, the authors find no net effect on self-employment or formal sector earnings, but a small positive effect on formal sector employment. Within the zero self-employment effect, they find that churn in and out of self-employment increases post-flag removal. Similarly, within the small formal sector employment result, they find that churn increases post-flag removal. They interpret these findings as evidence that (i) some formerly non-self employed people now have enough credit to start a business, and that (ii) some formerly self-employed people now have a clear credit record and can get a formal sector job.

The rest of our paper proceeds as follows. Section 2 presents background on credit reporting and describes our data. In Section 3, we presents our research design. Section 4 presents our results for the credit market and labor market outcomes. Section 5 concludes.

2 Background and Data

2.1 Credit Reporting

The history of credit reporting in the United States can be traced back to the nineteenth century, when third parties sold lists of deadbeat borrowers to local merchants to help them manage the provision of store credit. The industry grew throughout the twentieth century but was highly fragmented, with

2,250 local and regional firms in 1970. Over the 1970s and 1980s, the rapid growth in credit card lending fueled an expansion and consolidation of the credit bureau industry. Today, there are three national credit reporting agencies—Equifax, TransUnion, and Experian—that provide most credit reports. See [CFPB \(2012\)](#) for more on the history of the credit reporting system.

Along with basic information on name, address, and Social Security number, consumer credit reports provide four main categories of information:

- (i) The *tradeline* segment provides information on contract characteristics, utilization, and delinquency or default at the product level. For instance, for a individual credit card, the tradeline data includes information on the credit limit, account balance, and whether the consumer is in delinquency or default. The tradeline data are provided to the credit bureaus by the lenders, which are typically large national banks.
- (ii) The *public records* segment includes information on bankruptcies and tax liens. Non-financially relevant public information, such as marriage records, are not included in the credit report. These data are obtained from the Public Access to Court Electronic Records (PACER) system and government offices.
- (iii) The *collections* segment provides information on debts under collection, and is reported to the credit bureaus by third-party collection agencies.
- (iv) The *inquiries* segment provides information on consumer-initiated credit requests, known as “hard” inquiries. “Soft” inquiries, which result, for example, from a bank-initiated pre-screening, are typically not reported.

The Fair Credit Reporting Act (1970) limits the amount of time information can be maintained on credit reports. Chapter 7 bankruptcies may be listed for 10 years after the order for relief or date of adjudication. Conversely, information on Chapter 13 bankruptcies is traditionally removed after a period of only 7 years.⁷ The FCRA also stipulates that information on late payments, delinquencies, and collection items be removed after 7 years.

Requestors of credit bureau information do not necessarily receive the full set of credit bureau data. Potential employers, for instance, usually receive modified credit reports that do not contain

⁷Under FCRA, Chapter 13 flags are not mandated to be removed earlier than Chapter 7 flags, but all three national credit bureaus do so voluntarily. All three credit bureaus state that the Chapter 13 flag is removed at 7 years in their documentation, and we have confirmed this independently using the Experian credit report data described below. We have also confirmed that the Chapter 7 flag is removed at 10 years, as mandated by the FCRA, using the Experian data.

information on the potential employee’s credit score or date of birth. Lenders, on the other hand, usually receive the consumer credit scores and all of the standard credit report information aggregated to the individual level. These credit scores are sometimes developed by third parties, such as the Fair Isaac Corporation (FICO), and sometimes provided by the credit bureaus (e.g., VantageScore). There are dozens of types of credit scores based on different outcome variables and used for different types of lending decisions. The most common credit scores aim to predict the probability that a consumer will become 90 days delinquent on a new loan within two years. See [CFPB \(2012\)](#) for more background on the U.S. credit reporting system.

2.2 Data Sources and Sample Construction

We use two separate datasets to estimate the impact of removing a Chapter 13 bankruptcy flag on credit scores, financial outcomes, and formal sector employment and earnings. The first dataset uses information from the Federal Reserve Bank of New York’s Equifax Consumer Credit Panel (CCP). The second dataset uses information from individual bankruptcy filings merged to administrative tax records at the Social Security Administration (SSA).

The first dataset used in our analysis is constructed using records from the CCP, a representative five percent random sample all individuals in the U.S. with credit files.⁸ Like other credit report data, the CCP data are derived from public records, collections agencies, and trade lines data from lending institutions. The data include a comprehensive set of consumer credit outcomes, including information on credit scores, unsecured credit lines, auto loans, and mortgages. The data also include geographic location at the ZIP code-level and year of birth. No other demographic information is available at the individual level. Importantly, the data also include information on bankruptcy chapter, the bankruptcy outcome, and the quarter that a bankruptcy flag is both placed and removed from the credit file.⁹ The CCP data are available quarterly from 1999 to 2015. See [Avery et al. \(2003\)](#) and [Lee and der Klaauw \(2010\)](#) for additional details.

We make three sample restrictions to the CCP data. First, we restrict the sample to individuals who file for bankruptcy protection between 1995 and 2003. This restriction allows us to observe credit

⁸The CCP data is a representative sample of all individuals with a credit file, but does not include the roughly 11 percent of the U.S. population without credit files. As a result, the CCP data will be more representative for high income individuals than for low income individuals.

⁹We are unable to observe filing quarter for individuals filing before the first quarter of 1991. For these individuals, we infer the filing quarter based on when their bankruptcy flag is removed from their credit report. Specifically, we impute the filing quarter as being 7 years before the quarter of flag removal for Chapter 13 filers, and 10 years before the quarter of flag removal for Chapter 7 filers. In results available upon request, we find that the number of Chapter 7 and Chapter 13 filings in the CCP data closely track the number of filings observed in administrative bankruptcy records.

outcomes both before and after the flag removal. Second, we restrict our sample to individuals who were between 25 and 54 years old in the year of their bankruptcy filing to focus on working-age adults. Third, we restrict the sample to individuals who were granted bankruptcy protection to focus on a similar set of Chapter 7 and Chapter 13 filers.

The second dataset constructed for this study consists of individual bankruptcy filings merged to administrative tax records at the SSA. Bankruptcy records are available from 1992 to 2009 for the 81 (out of 94) federal bankruptcy courts that allow full electronic access to their dockets.¹⁰ We matched the individual-level bankruptcy records to administrative tax records from the SSA using last name and the last four digits of the filer’s social security number. We were able to successfully match over 90 percent of the bankruptcy records, with nearly all of the unmatched records resulting from a shared name and last four digits of the social security number in the SSA data.¹¹ The SSA data include information on all formal sector earnings and employment from annual W-2s, and self-employment earnings from annual 1040s at the IRS. Individuals with no W-2 or self-employment earnings in any particular year are assumed to have had no earnings in that year. Individuals with zero earnings are included in all regressions throughout the paper. The SSA data are available annually from 1978 to 2013.

Following our sample restrictions for the CCP data, we restrict the matched bankruptcy-SSA data to individuals who filed for bankruptcy protection between 1995 and 2003, who were between 25 and 54 years old in the year of their bankruptcy filing, and who were granted bankruptcy protection. All dollar amounts are adjusted to year 2013 dollars using the CPI-U.

Table 1 provides summary statistics on the CPP and SSA data, separately for Chapter 7 and for Chapter 13 filers, and for the combined sample.

3 Research Design

We estimate the impact of bankruptcy flag removal using a difference-in-differences research design that compares outcomes for Chapter 13 filers (the “treatment” group), who have their flags removed at 7 years, to Chapter 7 filers (the “control” group), who have their flags removed at 10 years and are therefore unaffected at the 7-year time horizon.

¹⁰We thank Tal Gross, Matthew Notowidigdo, and Jialan Wang for providing the bankruptcy data used in this analysis. See Gross, Notowidigdo and Wang (2014) for additional details on the PACER bankruptcy data.

¹¹The SSA data include every individual who has ever acquired a SSN, including those who are institutionalized. However, illegal immigrants without a valid SSN are not included in the SSA data. The SSA data also does not include information on informal earnings or employment.

We conduct our analysis using individual-level data collapsed to a more aggregate level to speed up the regression analysis. In the CPP data, we collapse by the full interaction of chapter of filing, cohort of filing, time period, state of residence, and 5-year age bins. In the SSA data, we also observe race (defined as white or non-white) and gender, and additionally collapse on these dimensions.¹² In our regression specifications, we weight each of the resulting cells by the number of underlying individual observations so that our estimates are representative of the underlying individual-level data. As we discuss below, since we cluster our standard errors above the level of aggregation, collapsing the data does not affect inference.

Let i index *filing groups*, defined by the full interaction of chapter of filing, cohort of filling, state of residence, and 5-year age bin. Let s index *calendar-time* at the year-quarter level. Let t indicate *event-time*, defined as quarters relative to the 7-year horizon when Chapter 13 bankruptcy flags removed. We define t using this 7-year horizon for both Chapter 13 and Chapter 7 filers even though Chapter 7 filers have their flags removed at 10 years. The collapsed data is at the $i \times t$ level.

For a given outcome, y_{it} , our difference-in-differences regression specification takes the form:

$$y_{it} = \alpha_i + \alpha_t + \alpha_{s(i,t)} + \sum_{t \neq -1} \left[\beta_t \cdot \mathbf{1}(\text{Chapter 13}) \right] + \varepsilon_{it}, \quad (1)$$

where α_i are filing group fixed effects, α_t are event-time fixed effects, $\alpha_{s(i,t)}$ are calendar-time fixed effects, $\mathbf{1}(\text{Chapter 13})$ is an indicator for filing under Chapter 13, and β_t are coefficients on Chapter 13 that vary non-parametrically by event time. We omit the period prior to flag removal, $\beta_{t=-1}$, so that the other β_t 's can be interpreted relative to this pre-removal baseline period. We also drop the base effect for the quarter prior to flag removal, $\alpha_{t=-1}$, as it is not separately identified from the other fixed effects in the specification. When we estimate this model using the annual SSA data, the event-time and calendar-time fixed effects are defined at the annual level, but otherwise the specification is unchanged.¹³

In this specification, the β_t coefficients can be interpreted as the differential change in y_{it} for Chap-

¹²In the CPP data, cohort and time period are defined at the year-quarter level and state of residence is defined using the state of residence 6 years after filing. In the SSA data, which is only available at the annual level, we define cohorts and time periods at the year level and state of residence is defined at the time of filing. We have examined the effect of flag removal on state of residence and find no effect. These results are available upon request.

¹³In this specification, we are only able to control for year, rather than year-quarter, calendar-time fixed effects due to collinearity. In our table results, we are able to control for year-quarter calendar-time fixed effects since our post flag removal estimates are simplified into three coefficients. When we estimate this model using the annual SSA data, we are analogously only able to control for two-year calendar time fixed effects but control for single-year calendar-time fixed effects in the tables.

ter 13 filers compared to Chapter 7 filers following the Chapter 13 bankruptcy flag removal. The identifying assumption is parallel trends: conditional on our controls, y_{it} would have followed a similar evolution for both groups of filers in the absence of the Chapter 13 flag removal. This identifying assumption would be violated if Chapter 13 and Chapter 7 filers have different trends in t . For example, our identifying assumption would be violated if Chapter 13 filers recover either faster or slower from a bankruptcy filing compared to Chapter 7 filers.

Our main approach to assess the validity of this assumption to examine outcomes for the treated and control filers in the pre-flag removal period. As discussed below, our plots of the raw data, and the non-parametric specifications, both show that outcomes for Chapter 13 and Chapter 7 filers move in close parallel during the pre-flag removal period for most outcomes. These results give us confidence that our control group is valid and provides us with an accurate counterfactual for what would have happened to the treatment group in the absence of flag removal.

The primary advantage of estimating the non-parametric event study in Equation (1) is that it allows us to flexibly assess the pattern of outcomes before and after the flag removal. To gauge the magnitude and statistical significance of the results, we also estimate a specification that pools the effect across sets of consecutive quarters in the post flag removal period. In particular, we estimate a specification where replace the quarter-specific coefficients in the post flag removal period with three post-flag removal coefficients: β_1 , which pools over quarters $t \in [0, 4)$, β_2 , which pools over quarters $t \in [4, 8)$, and β_3 , which pools over quarters $t \in [8, 12)$. Other than these pooled coefficients, the specification is identical to that in Equation (1).

In all specifications, we cluster our standard errors at the full interaction of the chapter of filing, cohort of filing, and state of residence, defined in the pre-flag removal period. This approach is more conservative than clustering at the individual level, and, for example, allows individuals who filed for Chapter 13 in California in 1998 to face correlated credit and labor market shocks when their bankruptcy flags are removed in 2005. We think allowing for correlated labor market shocks is important. Results are qualitatively similar clustering at either a more or less aggregated level.

4 Results

In this section, we examine the effects of the Chapter 13 flag removal using our difference-in-differences design. We first analyze the effects of flag removal on credit scores, before turning to its effects on unsecured borrowing, secured borrowing, and labor market outcomes.

4.1 Credit Scores

We begin with a descriptive analysis of how credit scores evolve for Chapter 13 and Chapter 7 bankruptcy filers just before and after flag removal. Since credit scores are used in the vast majority of lending decisions, improvements in credit scores should directly translate into increased credit availability and/or decreased interest rates (FRB, 2007).

Figure 1 plots average credit scores for Chapter 13 filers (the “treatment” group) and Chapter 7 filers (the “control” group) for each quarter relative to Chapter 13 flag removal. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter of Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. In the raw data, average credit scores are approximately seven points lower for Chapter 13 filers compared to Chapter 7 filers.

In the pre-removal quarters, credit scores for Chapter 13 and Chapter 7 filers trend upwards together, confirming the parallel trends identifying assumption. In the quarter that the Chapter 13 flag is removed, there is a clear jump of approximately 12 points in the credit scores of Chapter 13 filers relative to Chapter 7 filers.

After the flag removal, average credit scores for the control group, Chapter 7 filers, continue their upward trend, although at a slightly lower rate. This decrease in slope is due to a change in the rate of removal of flags for delinquencies and collections items, which are also removed after 7 years. In particular, prior to bankruptcy flag removal, the upward slope in credit scores partially reflects the removal of these flags. After the bankruptcy filing, there are no more delinquencies and collection items, and therefore no more flag removals 7 years further on.¹⁴ Average credit scores for Chapter 13 filers decline slightly in absolute value and more strongly relative to the Chapter 7 control group. However, even at 3 years post-removal, the credit scores of Chapter 13 filers remain approximately 5 points above those of Chapter 7 filers.

Appendix Figure A1 shows the corresponding difference-in-differences regressions for the effect of flag-removal on credit scores from our non-parametric specification in Equation (1). Table 2 shows results from our parametric specification that pools the effect across the first, second, and third years after flag removal. In both specifications, we omit the quarter before flag removal, so that the effects can be interpreted relative to the pre-removal period. We also control for chapter-by-cohort-by-age-by-

¹⁴See Dobbie, Goldsmith-Pinkham and Yang (2015) for an analysis of pre- and post-filing trends in credit scores, delinquencies, collection items, and so on. Dobbie and Song (2015) conduct an analogous analysis on pre- and post-filing trends in labor market outcomes.

state and calendar-year fixed effect in both specifications, and cluster standard errors at the chapter-by-cohort-by-state level. The point estimates indicate that credit scores increase by 11.3 points in the first year following flag removal, 7.9 points in the second year, and 5.0 points in the third year.

Consistent with the results from Figure 1, we also find that there is no systematic relationship between flag removal and credit scores in the pre-removal quarters. These pre-removal results are consistent with our identifying assumption of parallel trends for Chapter 13 and Chapter 7 filers.

In the Appendix, we examine heterogeneity in the effect on credit scores — and other outcomes — by *predicted* pre-flag removal credit score. In our context, splitting the analysis by actual pre-flag removal credit score is problematic because of mean reversion in credit scores. Specifically, we observe that individuals with particularly low credit scores in one time period see large credit score increases in subsequent time periods, likely because of an endogenous borrowing response to higher credit access or short-term flags from events such as mistakenly missing a credit card payment. As a result of this mean reversion, splitting by actual pre-flag removal credit scores will overstate the gains of individuals with low baseline credit scores. The solution is to split the data on stable characteristics that are predictive of credit scores but do not suffer from mean reversion. Thus, we segment the data by running a linear regression of pre-flag removal credit scores on age and state fixed effects six years after bankruptcy filing, and then split the data based on quartiles of the predicted credit scores from this regression.

Appendix Figure A2 plots average credit scores for Chapter 13 filers and Chapter 7 filers for different quartiles of the predicted pre-flag removal credit score. Perhaps the most striking result from this analysis is the heterogeneity in reversion of the initial increased in credit scores. For the lowest predicted credit score group, average actual credit scores for the treated Chapter 13 filers drop below the average credit scores for the Chapter 7 control group. The degree of reversion is decreasing by predicted credit score. For the highest predicted credit score group, average credit scores for the treated Chapter 13 filers drop very little in absolute or relative terms. While interesting, these results do not have a clear interpretation, as changes in credit scores across groups reflect both heterogeneity in economic outcomes and functional form and other modeling assumptions that go into the credit score formula.

4.2 Credit Card Debt

We examine two summary measures for credit card borrowing: (i) credit limits aggregated across all cards and (ii) total balances aggregated across all cards. The aggregate credit limit is a good summary measure of credit access because it captures changes due to the opening or closing of credit card accounts and changes due to limit increases or decreases on existing credit cards. The aggregate balance is analogously a good summary measure because it captures changes on both the extensive and intensive margin. Balances reported in the credit bureau data reflect interest bearing debt, which the consumer pays interest on, and transaction volume, which is fully repaid at the end of the billing cycle and therefore does not accrue interest. However, [Agarwal et al. \(2015\)](#) show that for consumers with credit scores in the bottom quartile of the distribution, which is representative of the sample analyzed in this paper, more than 90 percent of balances is of the interest bearing variety, suggesting that interpreting balances as borrowing is a reasonable approximation.

Figure 2 shows average credit limits and balances for Chapter 13 and Chapter 7 filers for each quarter relative to the Chapter 7 flag removal. Appendix Figure A4 shows the corresponding difference-in-differences regressions from our non-parametric specification in Equation (1). Table 3 shows results from our parametric specification that pools the effect across the first, second, and third years after flag removal.

There is an economically and statistically significant impact of flag removal on credit card limits and balances that grows fairly linearly over time. The regression estimates in Table 3 show that flag removal increases credit limits by on average \$476 per year, on a pre-flag removal mean of \$3,143. In particular, at three years after flag removal, credit limits have increased by \$1,430, or 45 percent. Credit card balances increase by on average \$277 per year, on a pre-flag removal mean of \$1,908. At three years after flag removal, balances have increased by \$831, or 44 percent. Consistent with the credit score results, both figures show no pre-trend in credit card limits or balances.

It is probably inappropriate to interpret the ratio of the increase in balances to the increase in credit limits as a pure marginal propensity to consume out of liquidity (MPC), such as that estimated in [Gross and Souleles \(2002\)](#) or [Agarwal et al. \(2015\)](#), for a number of reasons. First, bankruptcy flag removal likely leads to a reduction in interest rates, and thus the increase in balances reflects both liquidity and price effects. Second, balances probably include a small amount of transaction volume that does not accrue interest, and debatably should not be considered borrowing. Third, there may be some reverse causality if higher credit limits raise balances but then higher balances have a feedback

effect on credit limits, complicating the interpretation. These issues notwithstanding, taking the ratio of the increase in balances to the increase in credit limits results in a value of 44 to 58 percent over the three years, relative to a pre-flag removal utilization rate of 61 percent. These values are large, but almost identical to the the MPC out of liquidity estimated in [Agarwal et al. \(2015\)](#) for the bottom quartile of the credit score distribution.

In the Appendix, we examine heterogeneity in these effects by pre-flag removal credit score, splitting the sample in the same way we did for the credit score heterogeneity analysis. Appendix Figures [A5](#) through [A8](#) shows trends and difference-in-difference plots of the effects on credit limits and balances, and Appendix Table [A1](#) shows parameters estimates. The effect on credit limits and balances are increasing in pre-flag removal credit score. The implied MPC decrease modestly in pre-flag removal credit score, from 67 percent in the bottom quartile to 50 percent at the top quartile, based on the effects at three years after flag removal.

4.3 Mortgage Debt

We examine two measures of mortgage borrowing: (i) an indicator for whether the individual has at least one mortgage, and (ii) the natural logarithm of aggregate mortgage balances plus one. We use the logarithm of aggregate mortgage balances, rather than unadjusted levels, because the distribution of mortgage balances is highly right-skewed. We add one so that we can keep observations with no mortgage balances and hence balance results combine both extensive and intensive margin changes as a result of the bankruptcy flag removal.

Figure [3](#) shows averages values from these outcomes for Chapter 13 and Chapter 7 filers in each quarter relative to Chapter 13 flag removal. Appendix Figure [A9](#) shows the corresponding difference-in-differences regressions from our non-parametric specification in Equation (1). Table [4](#) shows results from our parametric specification that pools the effect across the first, second, and third years after flag removal.

There is an economically and statistically significant effect on both the extensive margin and aggregate measure of mortgage debt that occurs almost immediately after the Chapter 13 flag removal. The regression estimates in Table [4](#) show that in the first year after flag removal, the fraction of individuals with a mortgage increases by 2.3 percentage points, or 5.5 percent on pre flag removal base of 41.8 percent. Aggregate mortgage balances increase by 0.255 log points. For the standard reason, this estimate should be interpreted as the average proportional increase in mortgage balances, not the

proportional increase in the average mortgage balance.¹⁵ The regression estimates for both measures decline moderately over time. Figure 3 shows this decline is due, in roughly equal proportion, to absolute increases in outcome measures for the Chapter 7 control group and absolute declines in these measures for the Chapter 13 treated group. The pre-trends, shown in Figure A9, are zero on average across the pre-flag removal period. They exhibit a small upward trend in the year before Chapter 13 flag removal, but this increase is small economically and small relative to the increases following the flag removal.

The results are consistent with significant pent-up demand for mortgage borrowing — that is, a mass of consumers who would like to take out a mortgage but are unable to take out a loan or who put off taking out a loan until they can receive better contract terms. When the flags are removed, these households immediately take out a mortgage, and then repay this mortgage slowly over time. The timing and size of the effect suggests that consumers are aware of the date of flag removal or apply for a mortgage on a frequent enough basis to generate a rapid response. The size of the effect also underscores the importance of credit scores in this market, and is potentially surprising given the role of other factors like down payments and income-based requirements, which are also used to evaluate mortgage applicants.

In the Appendix, we examine heterogeneity in these effects by pre-flag removal credit score, splitting the sample in the same way we did for the credit score heterogeneity analysis. Appendix Figures A10 and A11 shows trends and difference-in-difference plots of the effects on an indicator for having a mortgage, and Appendix Table A1 shows parameters estimates. The effect on mortgages are small for the bottom quartile in pre-flag removal credit score, but approximately the same for the top three quartiles of the pre-score distribution. The pre-trends for the bottom quartile suggest that some of the improvement happens in the quarter before, and so the effect is slightly underestimated.

4.4 Labor Market Outcomes

Our analysis of labor market outcomes follows the above analysis of financial outcomes. On the extensive margin, we examine the effects of flag removal on (i) formal sector employment, (ii) self-employment, and (iii) the combination of either formal sector employment or self-employment. We also examine effects on (iv) formal sector wage earnings, (v) self-employment earnings, and (vi) total earnings from both formal sector employment and self-employment. We use the natural logarithm of

¹⁵We plan to experiment with alternative specifications that allow us to estimate the proportional increase in the average mortgage balance in future drafts of the paper.

each earnings measure plus one as the distribution of earnings are also right skewed. We again add one to each of the earnings measures so that we can keep observations with no reported earnings. Prior to flag removal, approximately 20.0 percent of individuals report no wage earnings, 93.4 percent report no self-employment earnings, and 16.8 percent report no earnings of either type.

Figure 4 shows averages of these outcomes for Chapter 13 and Chapter 7 filers for each quarter relative to the quarter of Chapter 13 flag removal. Appendix Figure A12 shows the corresponding difference-in-differences regressions from our non-parametric specification in Equation (1). Table 5 shows results from our parametric specification that pools the effect across the first, second, and third years after flag removal.

In contrast to the financial outcomes, there is no evidence of economically significant effects of flag removal on labor market outcomes, at least for the average bankruptcy filer. Flag removal increases wage employment in the second year by a statistically insignificant 0.1 percentage points, a 0.1 percent increase from the pre flag removal mean of 80.0 percent. The 95 percent confidence intervals also rule out employment effects of larger than about 0.3 percentage points, or 0.4 percent, over this time period. Similarly, we find that flag removal actually *decreases* log wage earnings by a statistically significant 0.03 log points over this time horizon, or 0.3 percent from the pre flag removal mean of 8.25 log points. Thus, for wage earnings, the 95 percent confidence intervals rule out positive effects altogether.

Self employment increases by only 0.1 percentage points in the second year, a 1.5 percent increase, and self-employment earnings increase by only 0.005 log points, a 1 percent increase. Not surprisingly, there are also no economically or statistically significant effects on measures that combines formal sector and self-employment earnings. None of the results suggest economically important effects of flag removal on the labor market outcomes of the average bankruptcy filer. Consistent with our earlier results, there is also no evidence of different pre-trends for any of our labor market outcomes. Thus, our identifying assumption also appears to be valid for these outcomes.

In the Appendix, we also examine labor market transitions. In particular, we define indicator variables for whether someone who had a job in the previous year takes on a new job (“change job”), maintains the same job (“same job”), or no longer has a job (“end job”). And we define indicators for whether someone who did not have a job in the prior year finds a job (“start job”) or continues to be without work (“no job”). Appendix Figure A13 shows trends for these indicator and Appendix Figure A14 plots coefficients from the corresponding difference-in-differences regressions. There are no economically significant effects on any of these outcome variables.

4.4.1 Interpretation, Heterogeneity Analysis, and Robustness

Our results thus far have shown that there are no economically important effects of flag removal on labor market outcomes, at least for the average filer. In interpreting the null effects on labor market outcomes, a natural question is whether employers consider a 7-year-old bankruptcy filing to be important relative to other derogatory items that could appear on a job applicant's credit report. A survey by the Society of Human Resource Management [SHRM \(2012\)](#) sheds some light on this matter. In response to the question, "how many years of credit history are the most influential in your assessment of the job candidate's credit standing?" 49 percent of firms reported focusing on time horizons of more than 6 years. Moreover, across industries and occupations, firms report that bankruptcies are one of the most important items they consider, along with whether the consumer had accounts in collection or current outstanding judgments. In contrast, firms did not place much weight on whether the job applicant had medical or education debt or whether they had a foreclosure flag on their report.

A second consideration is whether the zero effects for the average filer mask positive effects for specific subgroups. In particular, many advocates of restricting employer credit checks have argued that minorities are most likely to suffer adverse labor market consequences from poor credit reports, although it is unclear whether this is because these advocates think derogatory items are more common for this subset of the population or because they think that the casual effect of having a derogatory item is larger for this group.¹⁶

Table 6 investigates this claim by reporting estimates of the effect of flag removal on formal sector employment for mutually exclusive subsets of the sample determined by the full interaction of gender, race (defined as either white or non-white), and age (defined as either younger or older than 40 years old at the time of filing). The effects are economically small for all of the demographic groups. In circumstances where the estimates are statistically significant, the effects are of similar size for both whites and non-whites (e.g., white males 25-39 and non-white males 25-39).

To investigate heterogeneity in the estimates more broadly, Figure 5 plots the estimated effects of flag removal on formal sector employment at 2 years for 576 mutually exclusive subsamples determined by the full interaction of gender, race, age, and state of filing. The parameter estimates in the histogram are weighted by the number of underlying observations so that estimates from subsamples

¹⁶For instance, the NAACP and National Council of La Raza, among many other organizations, wrote a letter cosponsoring the "The Equal Employment for All Act" (H.R. 321), which aimed to prohibited employers from using credit checks as part of their hiring and promotion decisions for most positions, because credit checks are discriminatory, among other reasons. The bill was introduced in January, 2011 but did not pass.

with a large number of individuals are more prominent. The employment estimates in our sample are precisely and symmetrically clustered around zero. The small number of estimates that are statistically significant only accounts for 4 percent of the overall sample. Results are qualitatively similar at different time horizons and for our other labor market outcomes. While we cannot definitively rule out a positive effect for every subgroup, these results suggest that the outliers are likely to be the result of statistical noise.

A related consideration is whether our zero effects masks underlying churn across industries. For instance, the [SHRM \(2012\)](#) survey indicates that credit checks are used more frequently for jobs that involve “fiduciary and financial responsibility,” which are concentrated in the finance industry. At the other extreme, federal, state and local agencies are prohibited from considering bankruptcy in a hiring decision.¹⁷ Appendix Figures [A15](#) shows employment trends by industry, Appendix Figure [A16](#) shows the non-parametric difference-in-differences plots, and Appendix Table [A2](#) shows the parameter estimates. In short, there is no economically significant evidence of a reallocation of employment towards finance or any other systemic trends in the effects across industries. For the financial industry, we can rule out effects greater than 0.2 percent with 95 percent confidence after 2 years, and can rule out any positive effect in public sector employment with 95 percent confidence.

A third question is whether our non-effects may be partially explained by the state bans on credit checks that were introduced in the last few years of our sample ([Clifford and Shoag, 2016](#)). In response to fears of discriminatory hiring practices due to credit checks, ten states have banned the practice of credit checks over the past decade. These states are listed in Appendix Table [A3](#). In states with a ban on credit checks, we would not be surprised to find a non-result of bankruptcy flag removal on employment, as this information is not be available to potential employers. To account for this margin, we split our data into three groups: individuals filing in states that never have a ban, individuals filing in states that implement a ban after their flag removal, and individuals filing in states that implement a ban before their flag removal (but after their filing date). Table [7](#) shows that in all three cases, there is no statistically or economically significant effect of the Chapter 13 flag removal on the probability of formal sector employment. Results are again qualitatively similar for our other labor market outcomes.

In sum, we find zero effect of flag removal on average and zero effects across numerous demographic groups, including for minorities where there has been particular concern about the employ-

¹⁷See, for example: <http://www.nolo.com/legal-encyclopedia/will-bankruptcy-affect-my-job-future-employment.html>.

ment consequences of derogatory credit reports. We also find no evidence that employment shifts towards industries like finance, which more frequently use credit checks to screen applicants. Finally, we find no employment effects in states that did not implement bans on credit checks (or in states that implemented bans). The consistent zero effects across a range of subsamples and outcomes suggests that employers are not basing their hiring decisions primarily on credit scores. Instead, our results are consistent with employers using a broad range of mechanisms to screen job applicants so that the impact of an improved credit report does not have a measurable impact on the margin.

If credit reports do not matter for employment, why do employers purchase this information? There are a number of potential explanations. Perhaps the most straightforward are to reduce legal liability and to comply with state-level legal requirements. According to the [SHRM \(2012\)](#) survey, 22 percent of firms report that the primary reason for conducting a credit background check is to reduce legal liability for negligent hiring, and a further 7 percent report that the primary reason is complying with state laws requiring background check for certain professions (e.g., day care teachers, licensed medical practitioners). A slightly more involved explanation is that credit checks are a less costly way to screen job applicants that counterfactually would have been screened out later in the hiring process. To give a concrete example, suppose that virtually all job applicants who fail a credit check also fail to convert a final round job interview. However, suppose that a credit check is much cheaper to the firm than the cost of conducting a final round interview. Then it is optimal to conduct and reject applicants based on the credit checks even if exogenously varying the outcome of the credit check has no impact on the hiring decision on the margin.¹⁸

5 Conclusion

This paper estimates the causal effect of improved credit reports on credit and labor market outcomes. We find that the removal of a Chapter 13 bankruptcy flag leads to large increases in credit scores and both credit card and mortgage borrowing. In sharp contrast, we find a precise zero effect of improved credit reports on employment and earning outcomes. We conclude that credit reports are most important for outcomes where they are the primary source of information used in screening applicants, such as credit card or mortgage borrowing, but are of limited consequence for outcomes where a broad range of additional information is available, such as hiring decisions. Our results also

¹⁸Indeed, such a model could explain the anecdotal evidence cited in the media. It would be natural for people who failed a credit check to attribute their unemployment to their credit report, even though, counterfactually, even if they had passed the credit check, they still would have been denied the job.

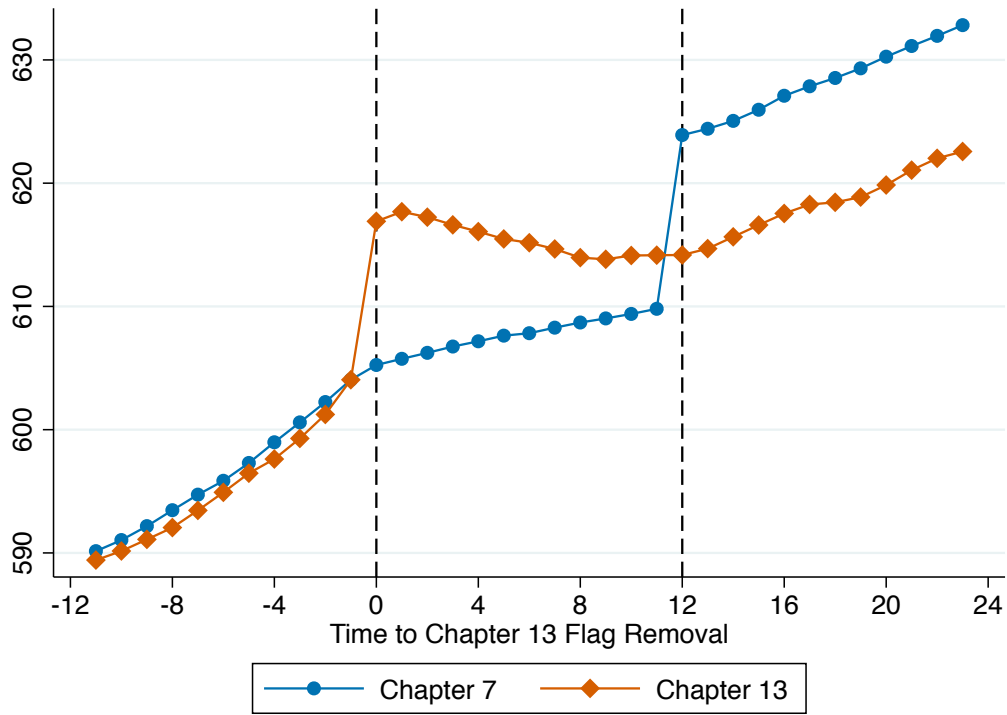
indicate that recent political attempts to limit the use of credit reports by employers are unlikely to affect labor market outcomes, either positively or negatively, for the targeted populations.

There are at least three caveats to our analysis. First, our employment data is measured annually, and job transitions are infrequent occurrences, so any effects of credit worthiness may not be reflected over the three year time horizons we currently examine. Second, the zero average effects we estimate for labor market outcomes do not rule out the existence of some groups of individuals who are affected by flag removal. In our heterogeneity analysis, we find economically small, but statistically significant, effects for some subgroups of the population, and so cannot rule out completely the presence of an effect for some subgroups in the population. Finally, the effects of other changes in the credit worthiness, such as the removal of a foreclosure flag or delinquent debt flag, may be different than the effects examined here. While beyond the scope of this paper, estimating the effects of other improvements in credit worthiness is an important area of future work.

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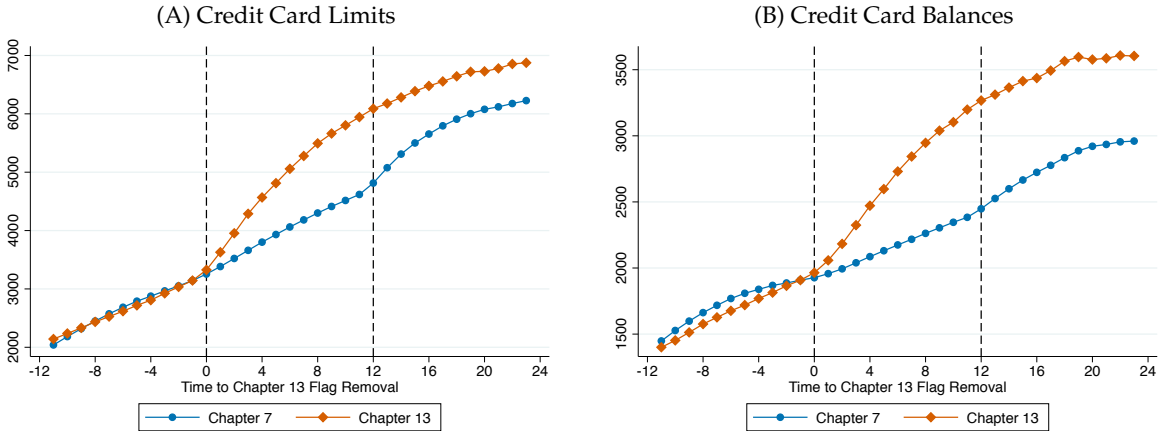
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Figure 1: Credit Score Trends



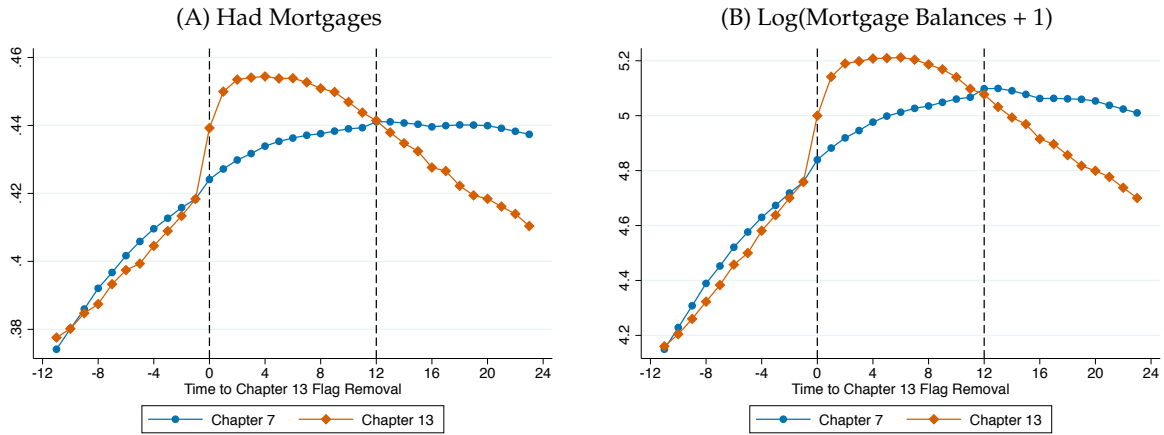
Note: This figure plots average credit scores for Chapter 13 and Chapter 7 bankruptcy filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 2: Credit Card Trends



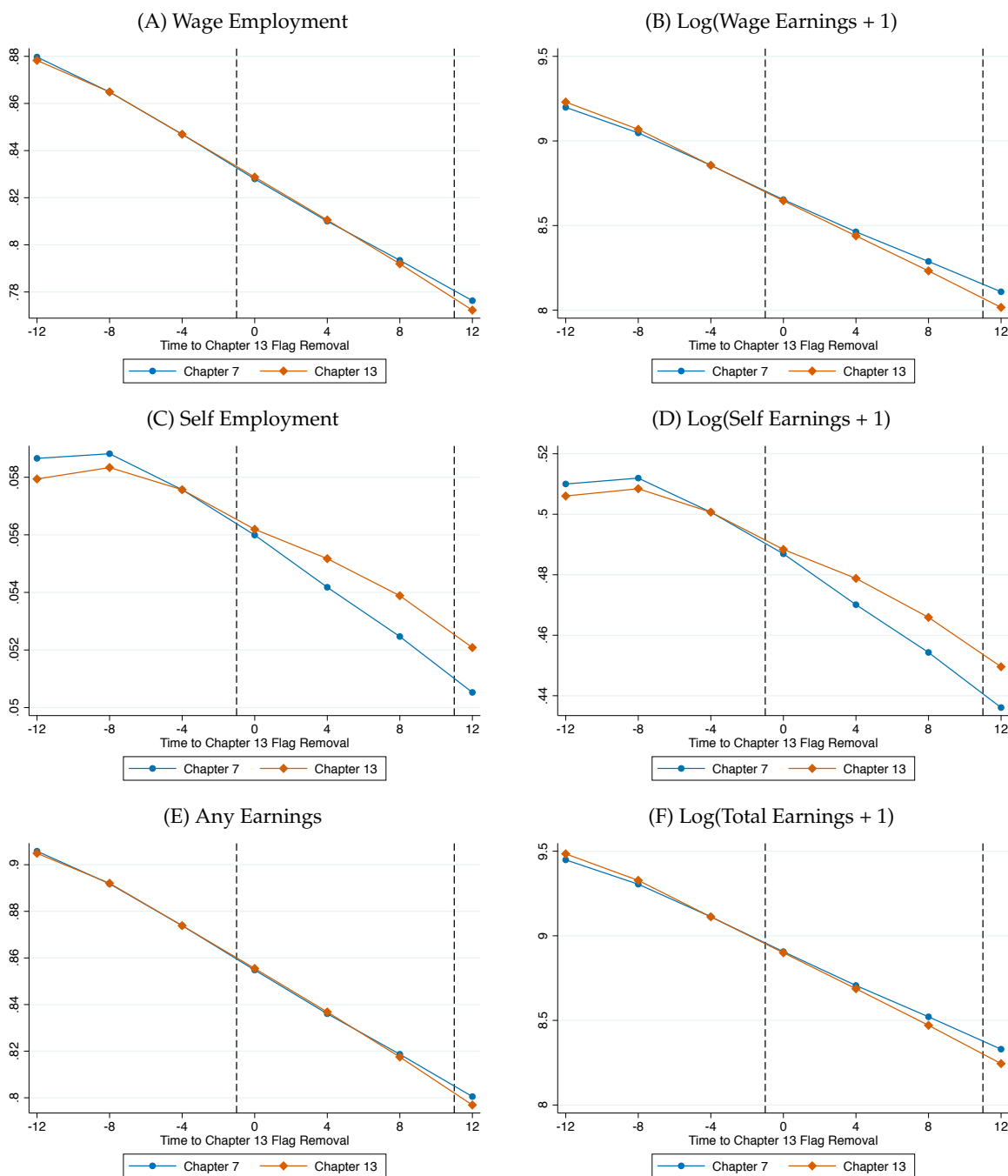
Note: This figure plots the average credit card outcomes of bankruptcy filers to the event of bankruptcy flag removal. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 3: Mortgage Trends



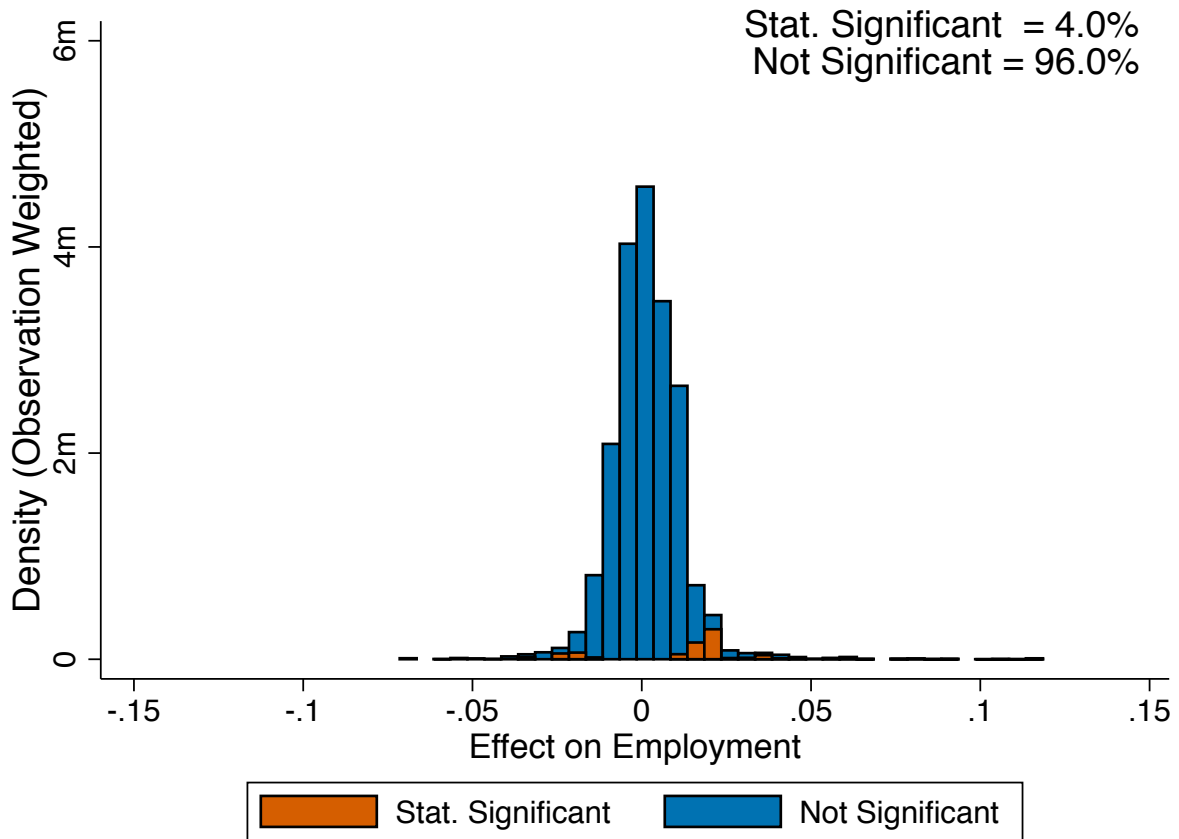
Note: This figure plots the average mortgage outcomes of bankruptcy filers to the event of bankruptcy flag removal. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 4: Labor Market Trends



Note: This figure plots the average labor market outcomes of bankruptcy filers to the event of bankruptcy flag removal. The horizontal axis denotes time, in years, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal and the year Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the year prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 5: Employment Results by Gender \times Race \times Age \times State



Note: This figure plots the distribution of difference-in-differences estimates for employment at one year at the gender \times race \times age \times state level. We weight each estimate by the number of observations. We also report the fraction of estimates that are statistically significant at the 5 percent level from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 1: Summary Statistics

	Chapter 13 Filers		Chapter 7 Filers		Pooled	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Credit Report Data:</i>						
Credit Score	605.89	42.71	608.60	22.11	608.34	24.90
Credit Card Limits	3,770.78	3,630.05	4,983.03	2,047.05	4,864.48	2,280.10
Credit Card Balance	2,164.22	2,141.10	2,906.67	1,077.56	2,834.07	1,242.79
Had Mortgage Loan	0.42	0.22	0.33	0.10	0.34	0.12
Log(Mortgage Balance + 1)	4.82	2.56	3.74	1.14	3.85	1.39
<i>Labor Market Data:</i>						
Wage Employment	0.83	0.08	0.78	0.09	0.78	0.09
Self-Employment	0.06	0.01	0.06	0.01	0.06	0.01
Any Employment	0.85	0.08	0.81	0.09	0.81	0.09
Wage Earnings	38,016.08	4,888.65	32,790.82	4,567.41	33,360.71	4,880.68
Self Employment Earnings	655.74	134.18	741.07	151.46	731.76	151.93
Total Earnings	37,208.25	4,774.21	32,299.29	4,486.39	32,834.68	4,768.24
Log(Wages + 1)	8.64	0.91	7.98	0.94	8.06	0.96
Log(Self-Employment Earnings + 1)	0.49	0.06	0.56	0.08	0.55	0.08
Log(Earnings + 1)	8.89	0.91	8.29	0.95	8.35	0.96

Note: This table reports summary statistics. The Chapter 13 sample includes individuals who were age 25-55 when they filed for Chapter 13 bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The Chapter 7 sample includes individuals with the same characteristics, but whose flag was removed 12 quarters after. All outcomes are measured for the 11 quarters before and after the Chapter 13 flag removal. Credit and mortgage outcomes come from quarterly CCP data. Labor market outcomes come from administrative tax data at the SSA. Employment is an indicator for non-zero wage earnings, self employment is an indicator for non-zero self employment earnings, and any earnings is an indicator for non-zero wage or self employment earnings.

Table 2: Effect of Chapter 13 Flag Removal on Credit Scores

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
Credit Score	604.049 (38.016)	11.269*** (0.323)	7.897*** (0.489)	4.952*** (0.624)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit scores. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the quarter prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal from 0-3, 4-7, and 8-11 quarters, respectively. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The regressions include 28,558,837 person-year-quarter observations for 471,561 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 3: Effect of Chapter 13 Flag Removal on Credit Card Outcomes

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
Credit Card Limits	3,143.685 (2,668.301)	380.566*** (29.340)	1,035.424*** (67.750)	1,429.701*** (91.392)
Credit Card Balance	1,908.004 (1,709.473)	166.786*** (15.557)	552.486*** (35.679)	830.795*** (51.472)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit card outcomes. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the quarter prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal from 0-3, 4-7, and 8-11 quarters, respectively. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The regressions include 8,558,837 person-year-quarter observations for 471,561 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 4: Effect of Chapter 13 Flag Removal on Mortgage Outcomes

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
Had Mortgage Loan	0.418 (0.220)	0.023*** (0.002)	0.022*** (0.003)	0.015*** (0.003)
Log(Mortgage Balance + 1)	4.759 (2.544)	0.255*** (0.029)	0.252*** (0.033)	0.166*** (0.037)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on mortgage outcomes. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the quarter prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal from 0-3, 4-7, and 8-11 quarters, respectively. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The regressions include 8,558,837 person-year-quarter observations for 471,561 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 5: Effect of Chapter 13 Flag Removal on Labor Market Outcomes

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
Wage Employment	0.800 (0.081)	0.001 (0.001)	0.000 (0.001)	-0.002* (0.001)
Self-Employment	0.066 (0.025)	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)
Any Employment	0.832 (0.079)	0.000 (0.001)	0.000 (0.001)	-0.002* (0.001)
Log(Wages + 1)	8.255 (0.891)	-0.009 (0.015)	-0.030** (0.015)	-0.064*** (0.014)
Log(Self-Employment Earnings + 1)	0.573 (0.223)	-0.001 (0.005)	0.005 (0.005)	0.006 (0.006)
Log(Earnings + 1)	8.561 (0.884)	-0.011 (0.017)	-0.028* (0.016)	-0.062*** (0.015)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the year prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal for years 1-3. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The regressions include 32,797,030 person-year observations for 4,685,290 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 6: Employment Results by Filer Characteristics

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
White Males 25-39	0.855 (0.038)	0.003** (0.001)	0.007*** (0.002)	0.007*** (0.001)
Non-White Males 25-39	0.845 (0.046)	0.004** (0.002)	0.004** (0.002)	0.006*** (0.002)
White Males 40-55	0.725 (0.074)	0.000 (0.001)	0.000 (0.002)	-0.003* (0.002)
Non-White Males 40-55	0.733 (0.083)	0.000 (0.002)	-0.001 (0.002)	-0.003 (0.002)
White Females 25-39	0.825 (0.034)	0.002 (0.002)	0.003 (0.002)	0.002 (0.002)
Non-White Females 25-39	0.877 (0.047)	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)
White Females 40-55	0.745 (0.066)	-0.001 (0.002)	-0.002 (0.002)	-0.004* (0.002)
Non-White Females 40-55	0.781 (0.083)	-0.000 (0.002)	-0.002 (0.002)	-0.005** (0.002)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal separately by borrower subgroup. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the year prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal for years 1-3. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The regressions include 32,797,030 person-year observations for 4,685,290 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table 7: Employment Results and State Bans on Credit Checks for Employment

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
States with No Ban	0.802 (0.083)	0.001 (0.001)	0.000 (0.001)	-0.002 (0.001)
Pre-Ban in States with Ban	0.793 (0.075)	-0.001 (0.003)	-0.001 (0.003)	-0.004 (0.003)
Post-Ban in States with Ban	0.796 (0.073)	-0.004 (0.011)	-0.006 (0.011)	-0.009 (0.011)

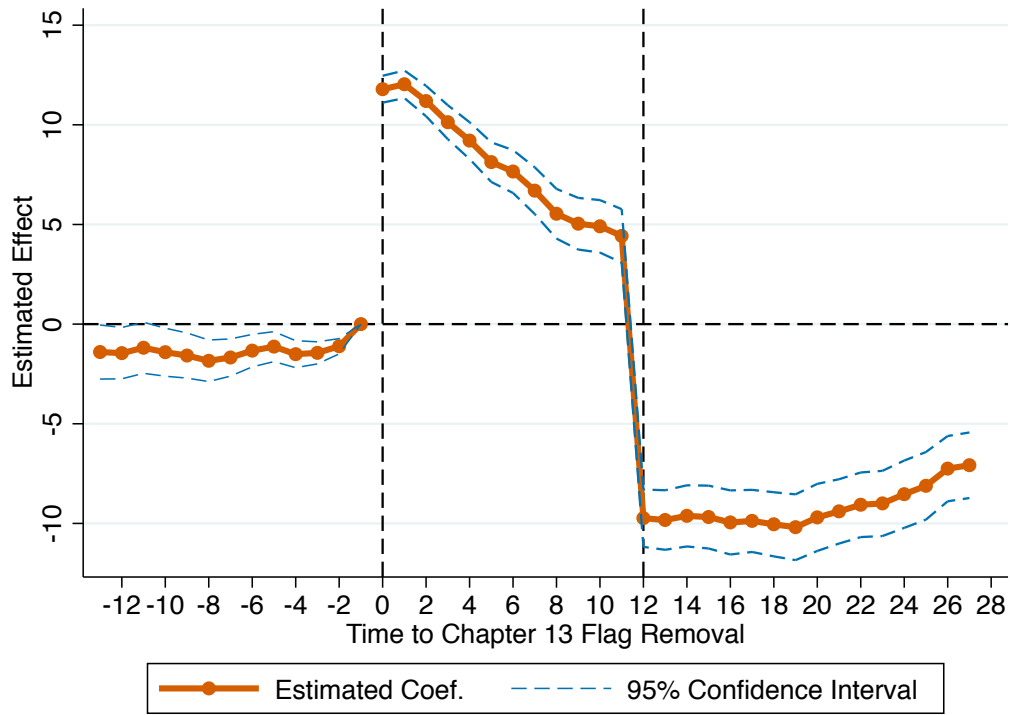
Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal separately for states with and without bans on credit checks for employment. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the year prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal for years 1-3. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2002-2011. The regressions include 32,797,030 person-year observations for 4,685,290 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Bad Credit, No Problem? Credit and Labor Market Consequences of Bad Credit Reports

Online Appendix

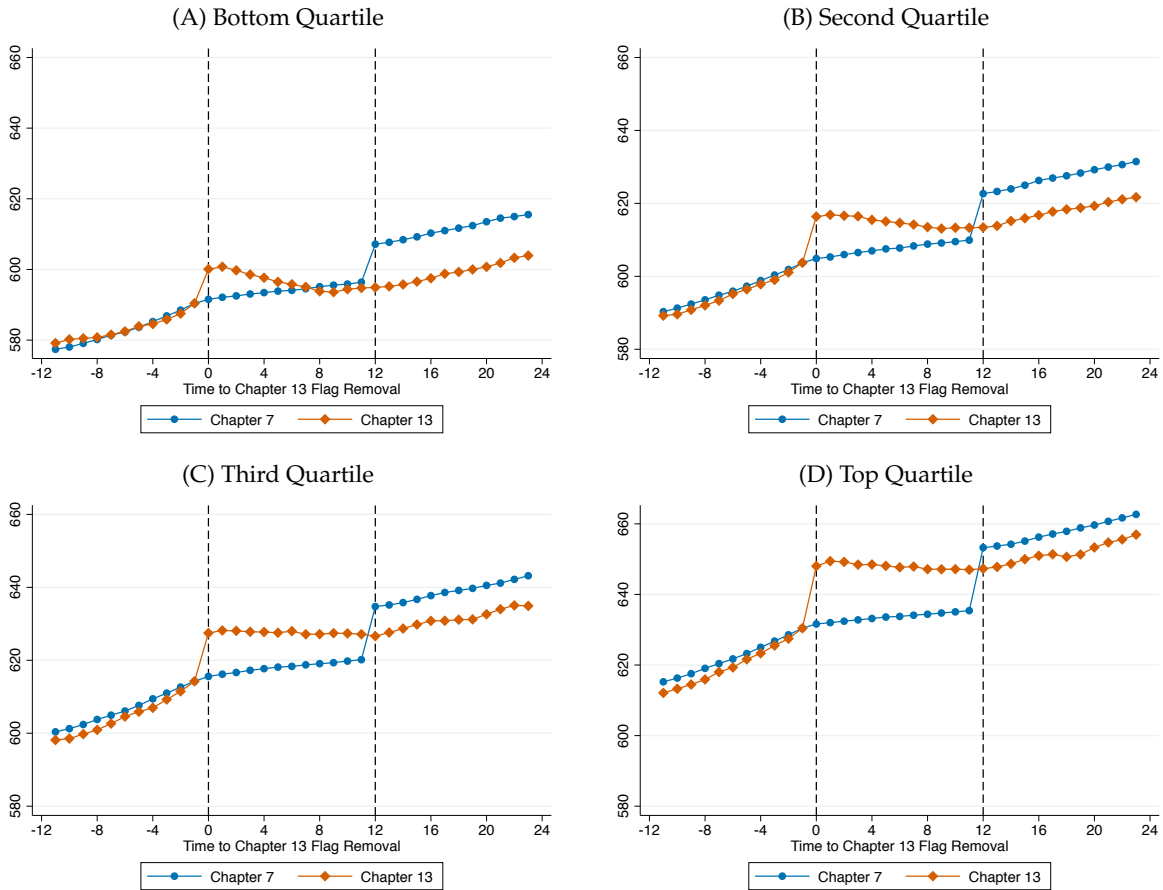
Will Dobbie Paul Goldsmith-Pinkham Neale Mahoney Jae Song

Figure A1: Effect of Chapter 13 Flag Removal on Credit Scores



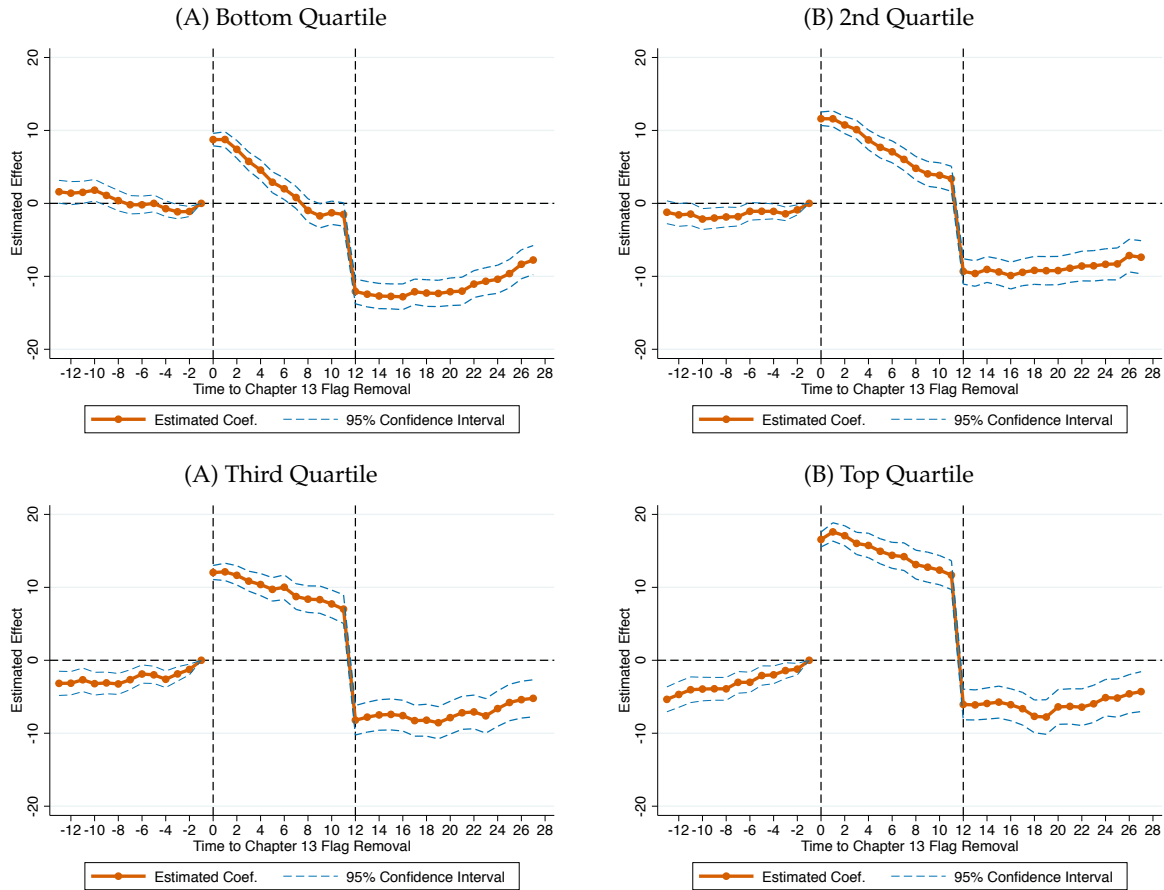
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit scores. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A2: Credit Score Trends by Pre-Flag Removal Credit Score



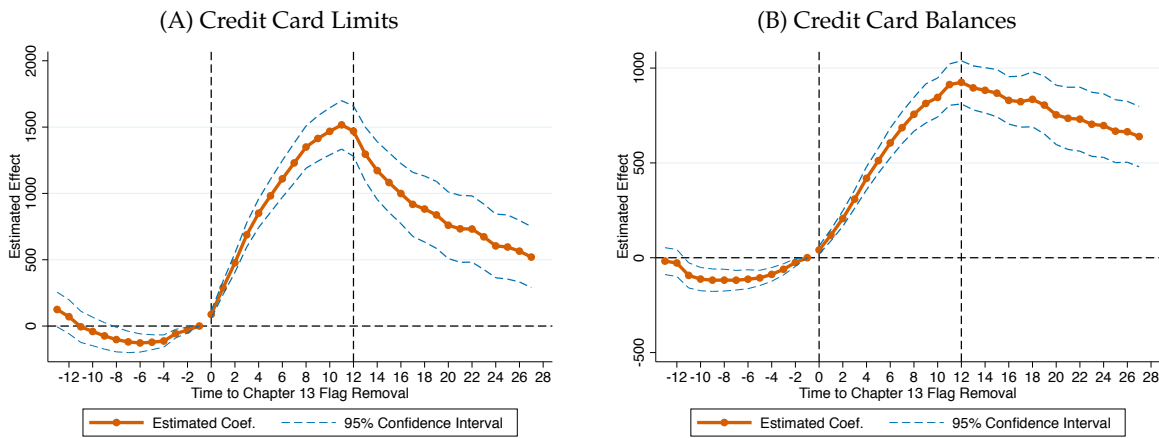
Note: This figure plots average credit scores for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A3: Credit Score by Pre-Score



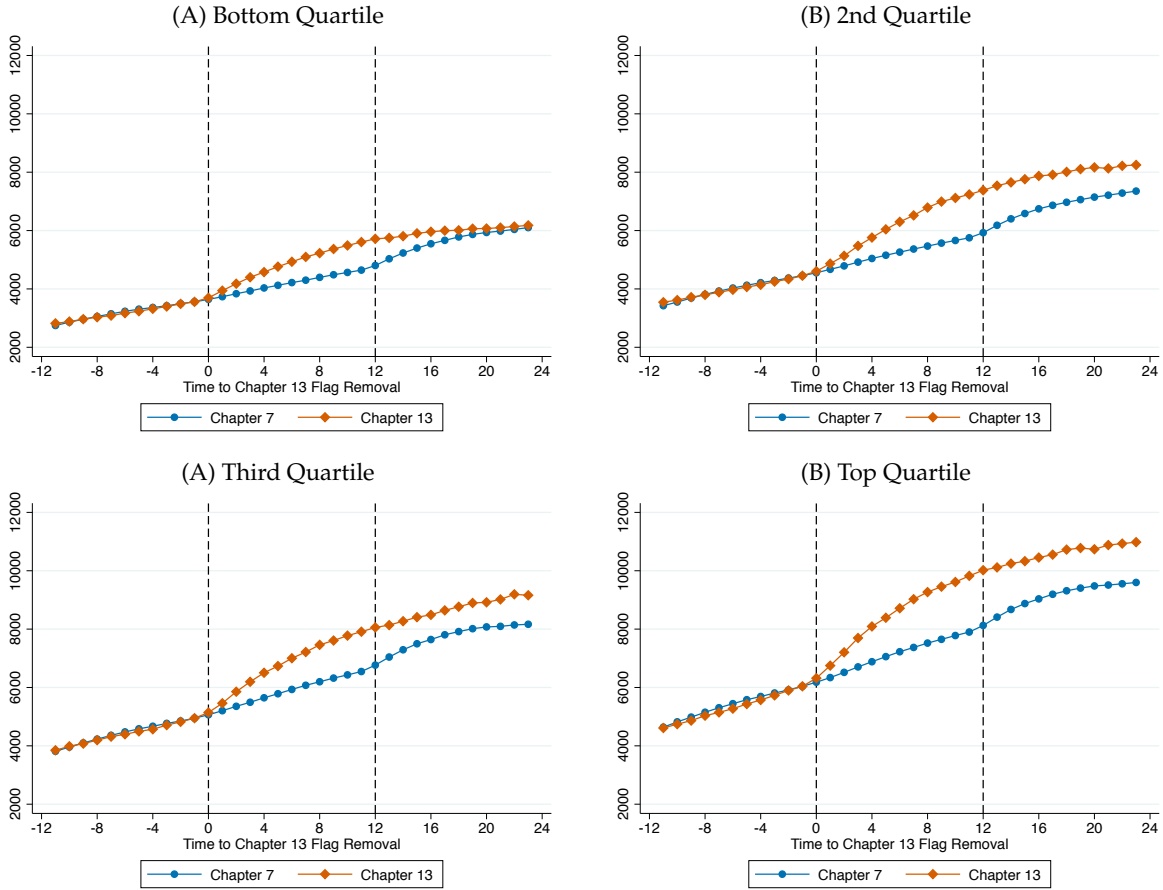
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit score outcomes by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A4: Effect of Chapter 13 Flag Removal on Credit Card Outcomes



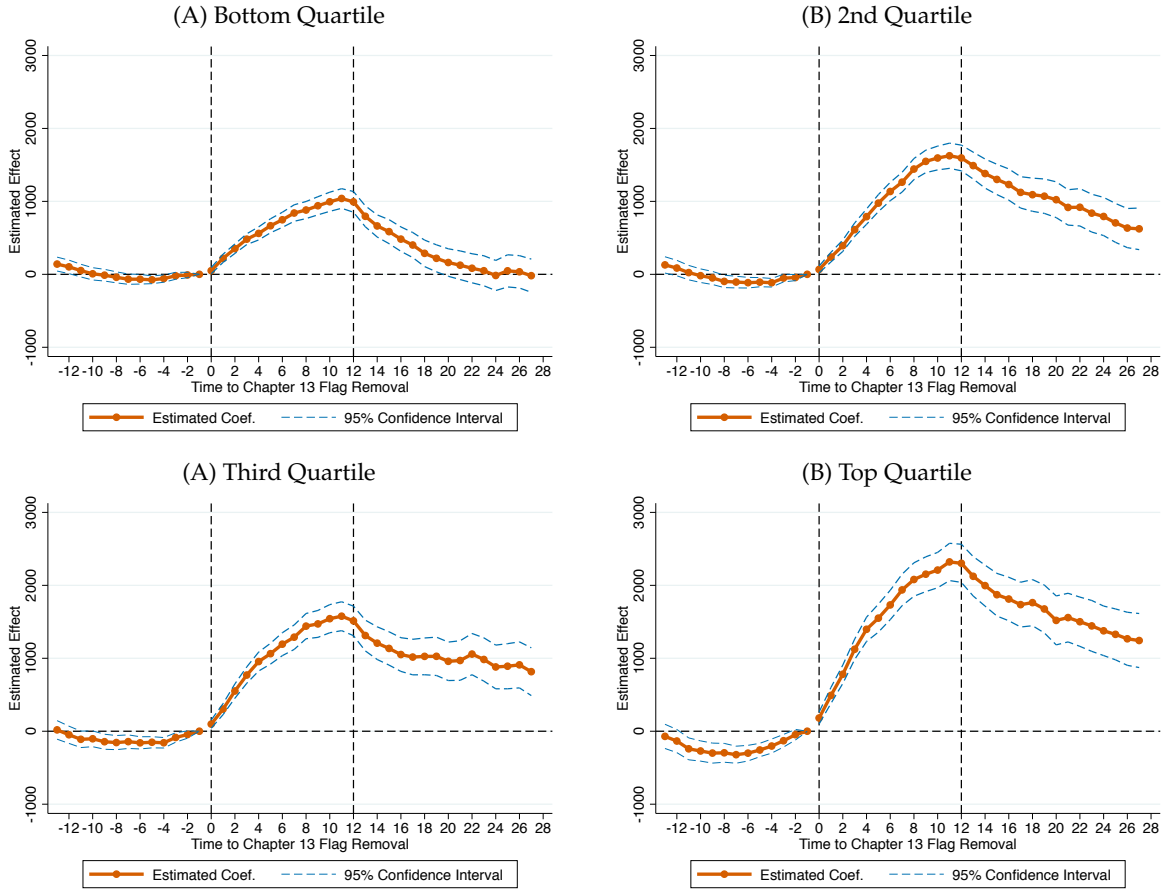
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit card outcomes. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A5: Credit Card Limits by Pre-Score



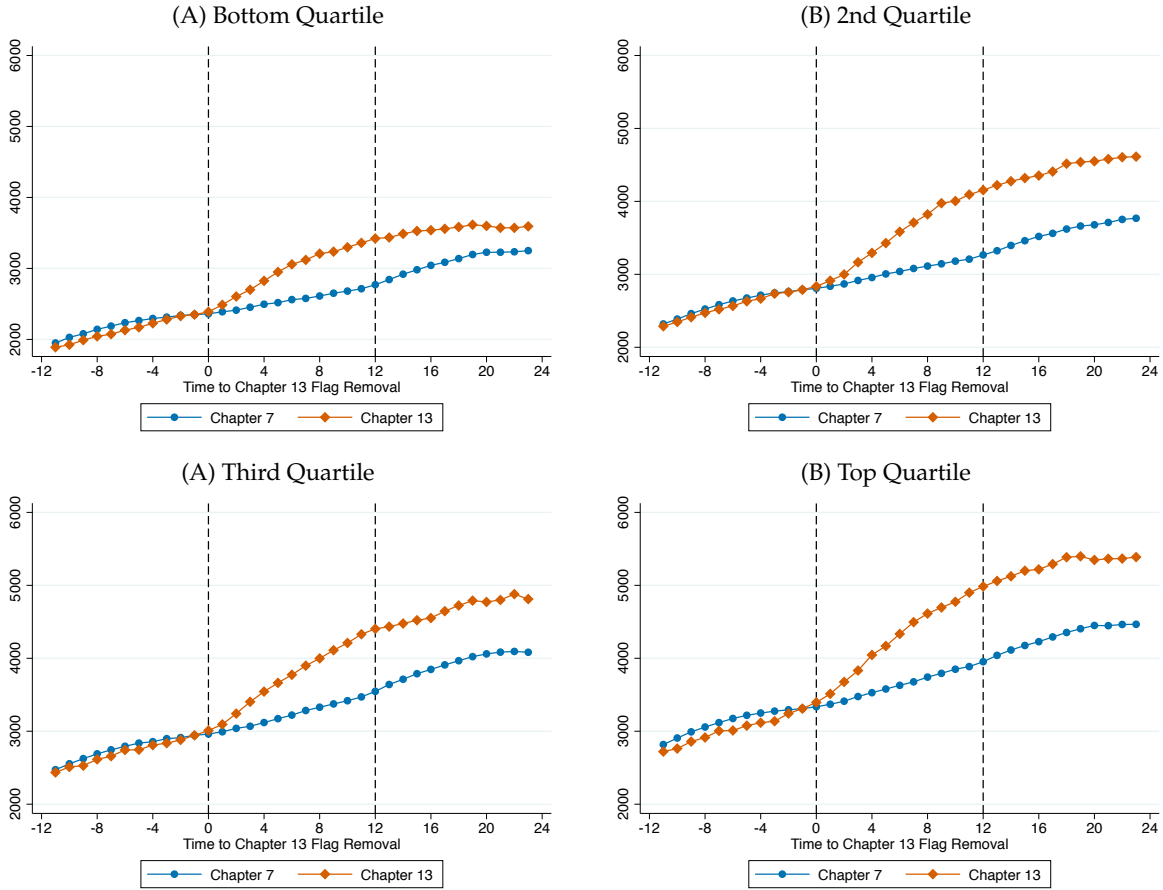
Note: This figure plots average credit card limits for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A6: Credit Card Limits by Pre-Score



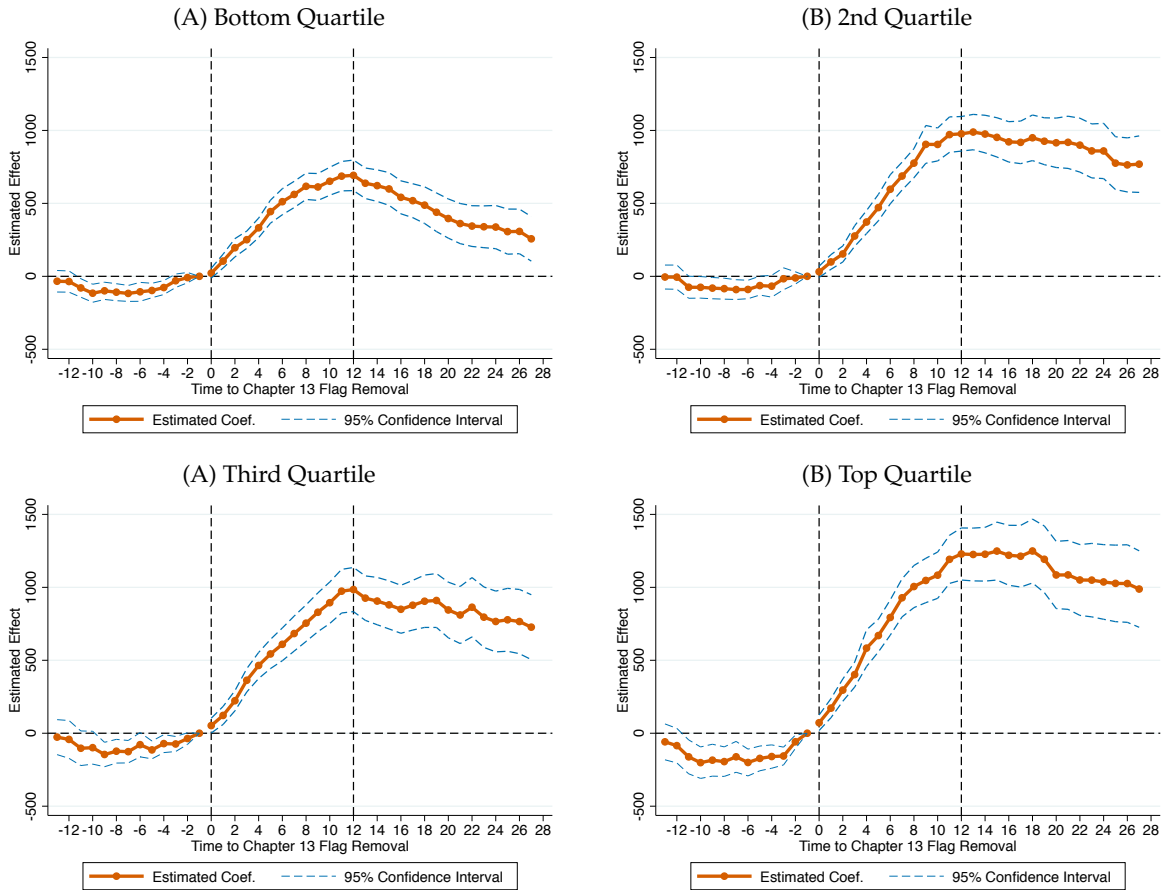
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit card limits by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A7: Credit Card Balance by Pre-Score



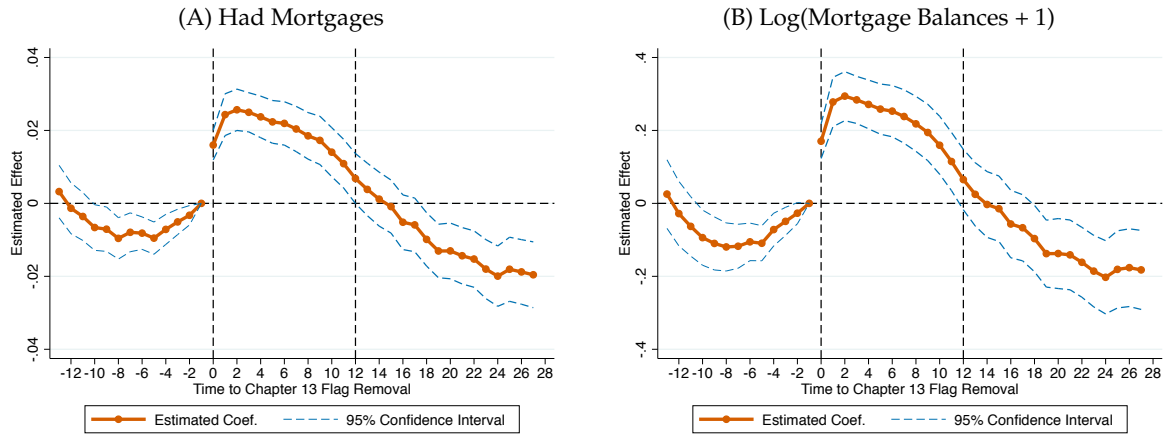
Note: This figure plots average credit card balances for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A8: Credit Card Balance by Pre-Score



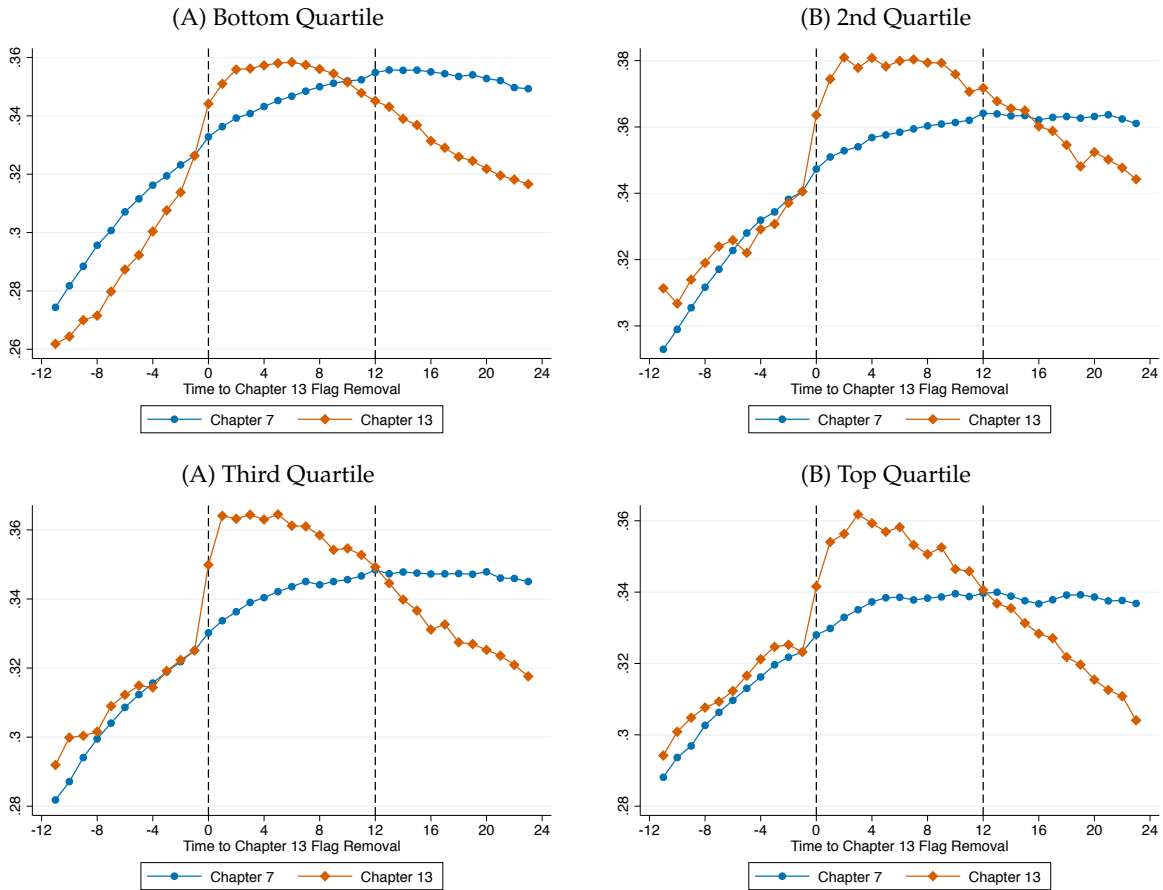
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on credit card balances by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A9: Effect of Chapter 13 Flag Removal on Mortgage Outcomes



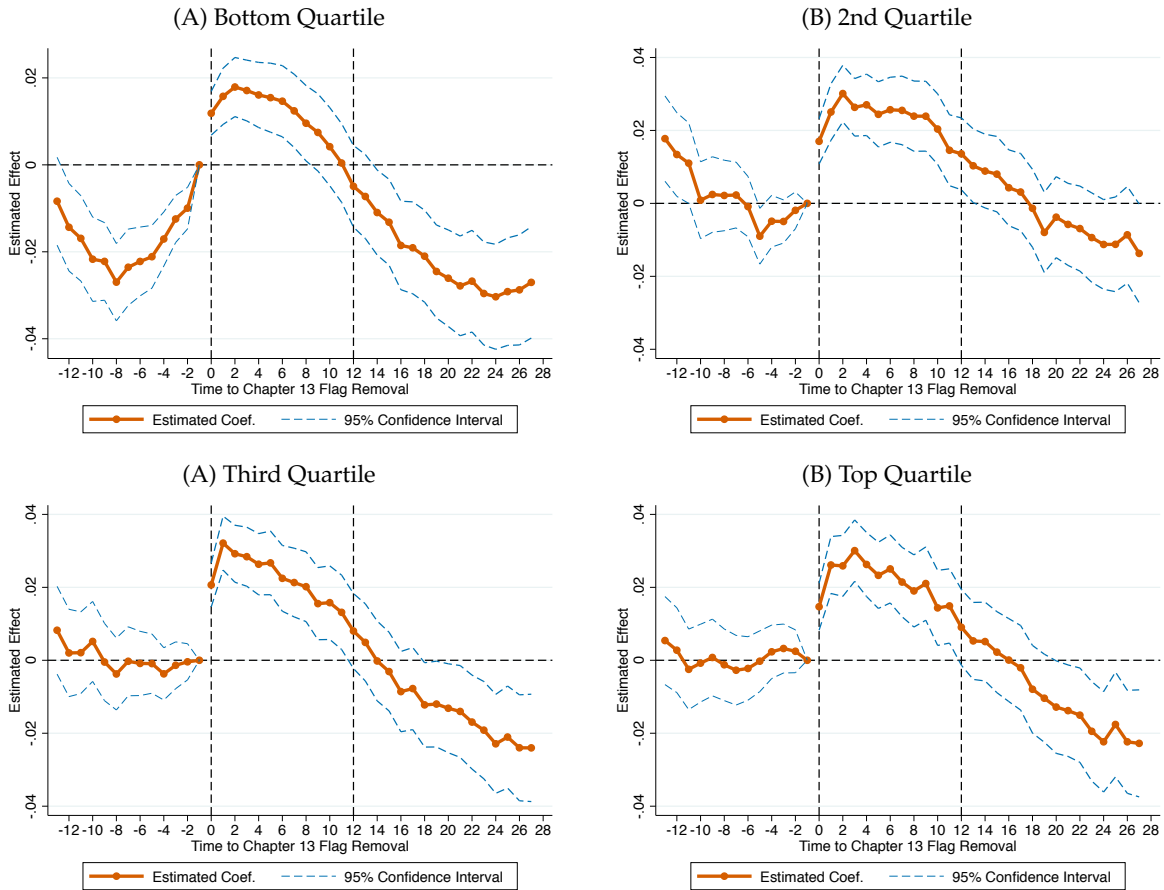
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on mortgage outcomes. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A10: Had Mortgage by Pre-Score



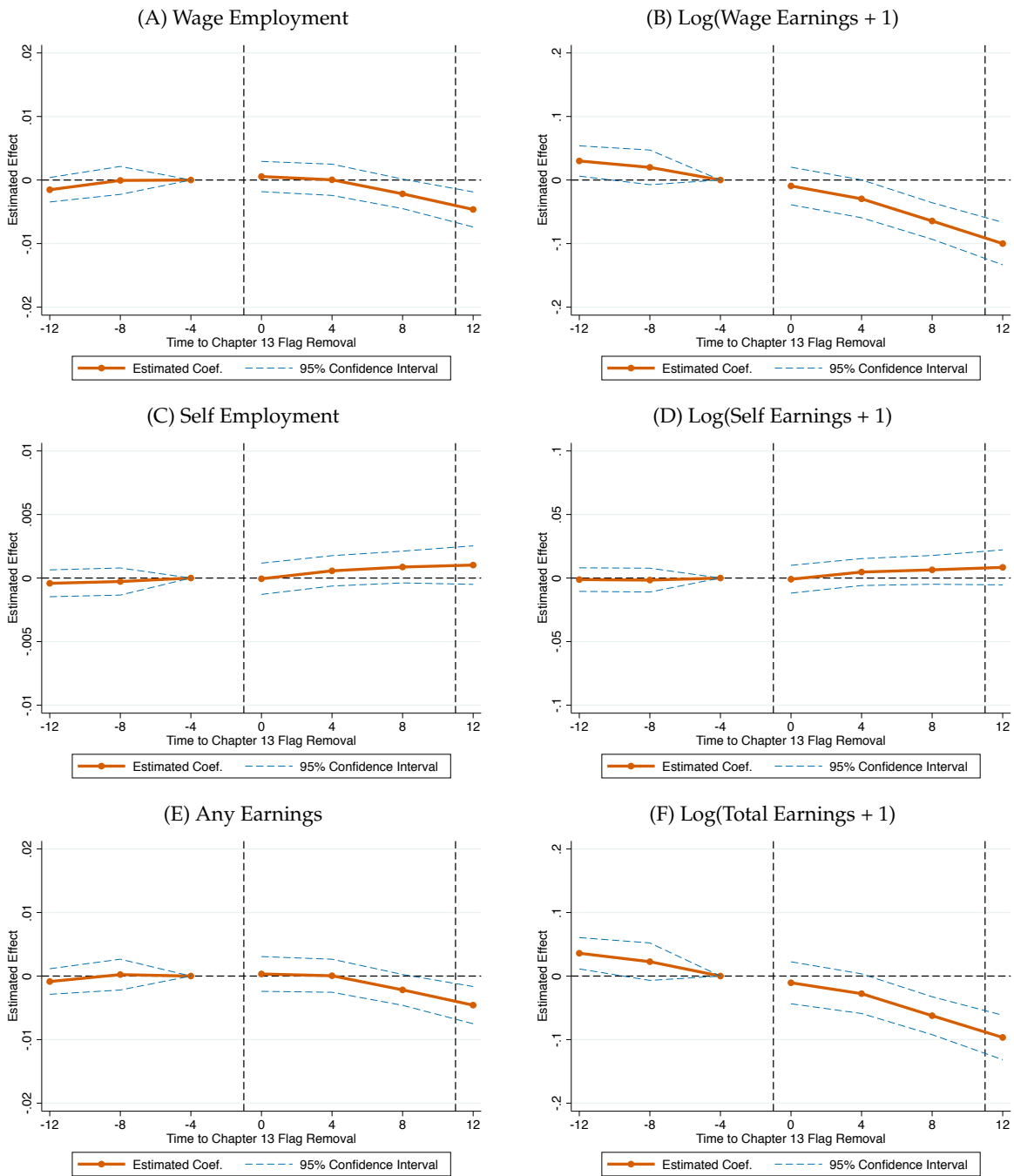
Note: This figure plots average fraction of individuals with a mortgage for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A11: Had Mortgage by Pre-Score



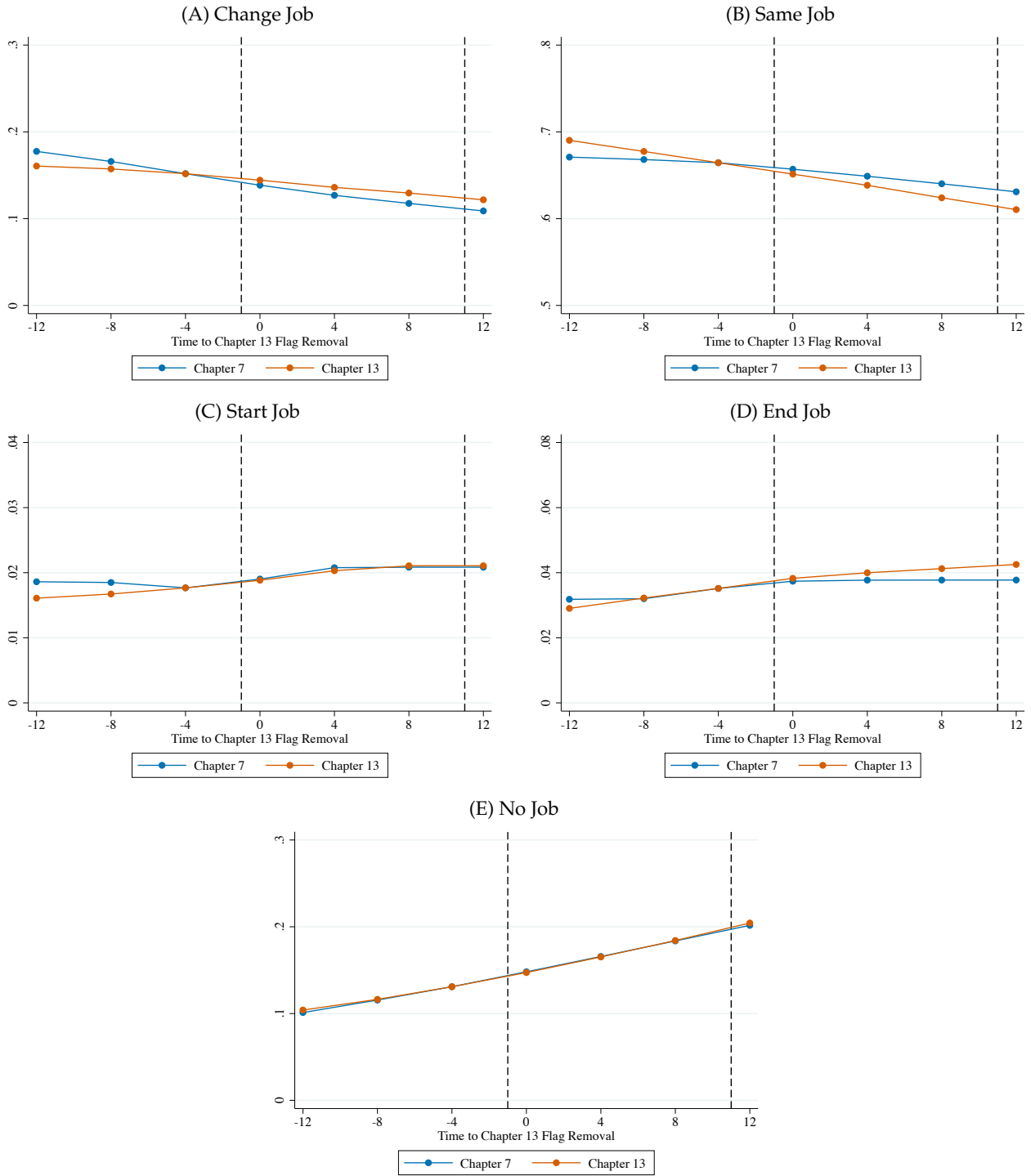
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on a mortgage indicator by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the quarter before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A12: Effect of Chapter 13 Flag Removal on Labor Market Outcomes



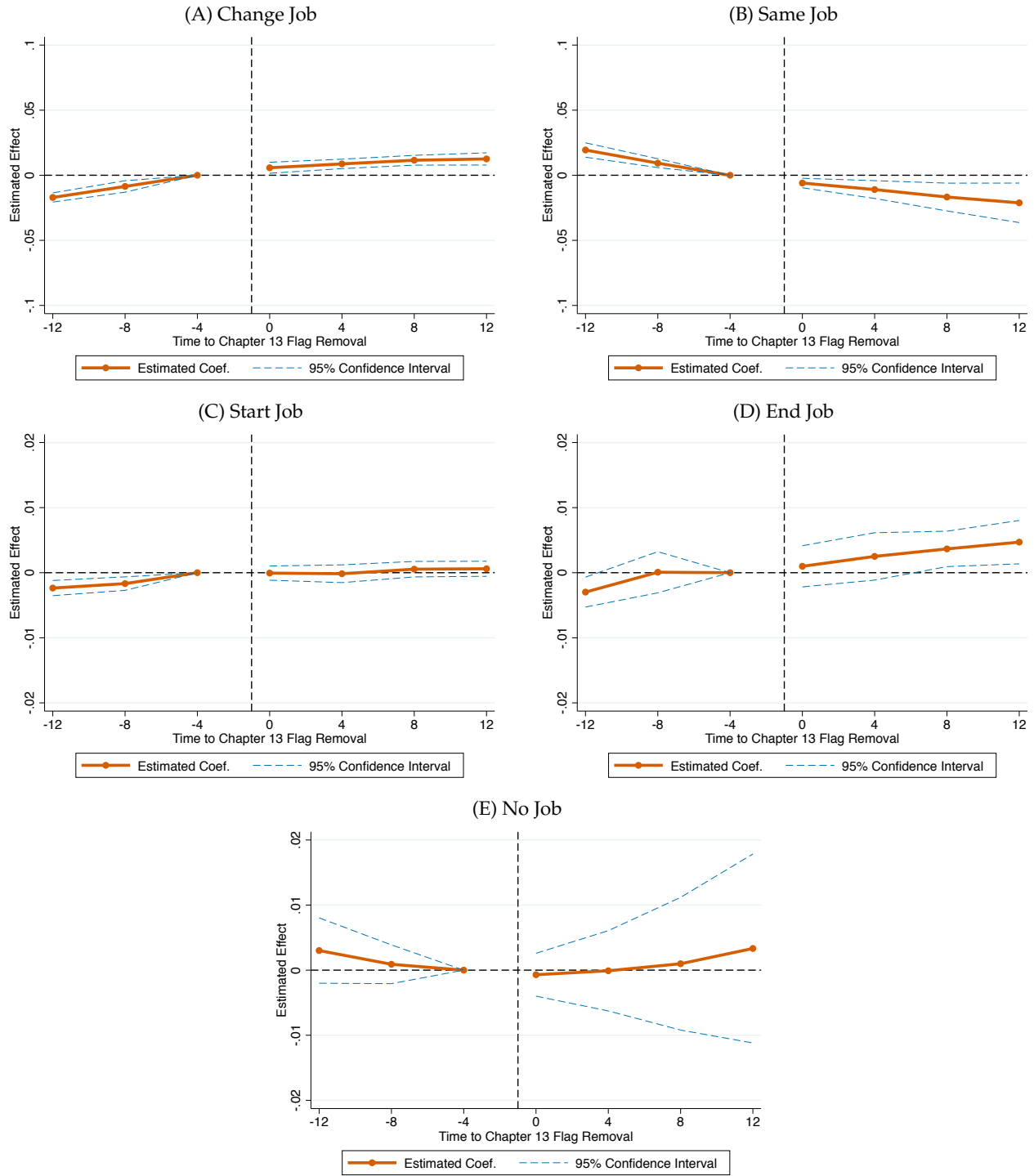
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes. The horizontal axis denotes time, in years, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal and the year Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the year before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A13: Employment Transitions Trends



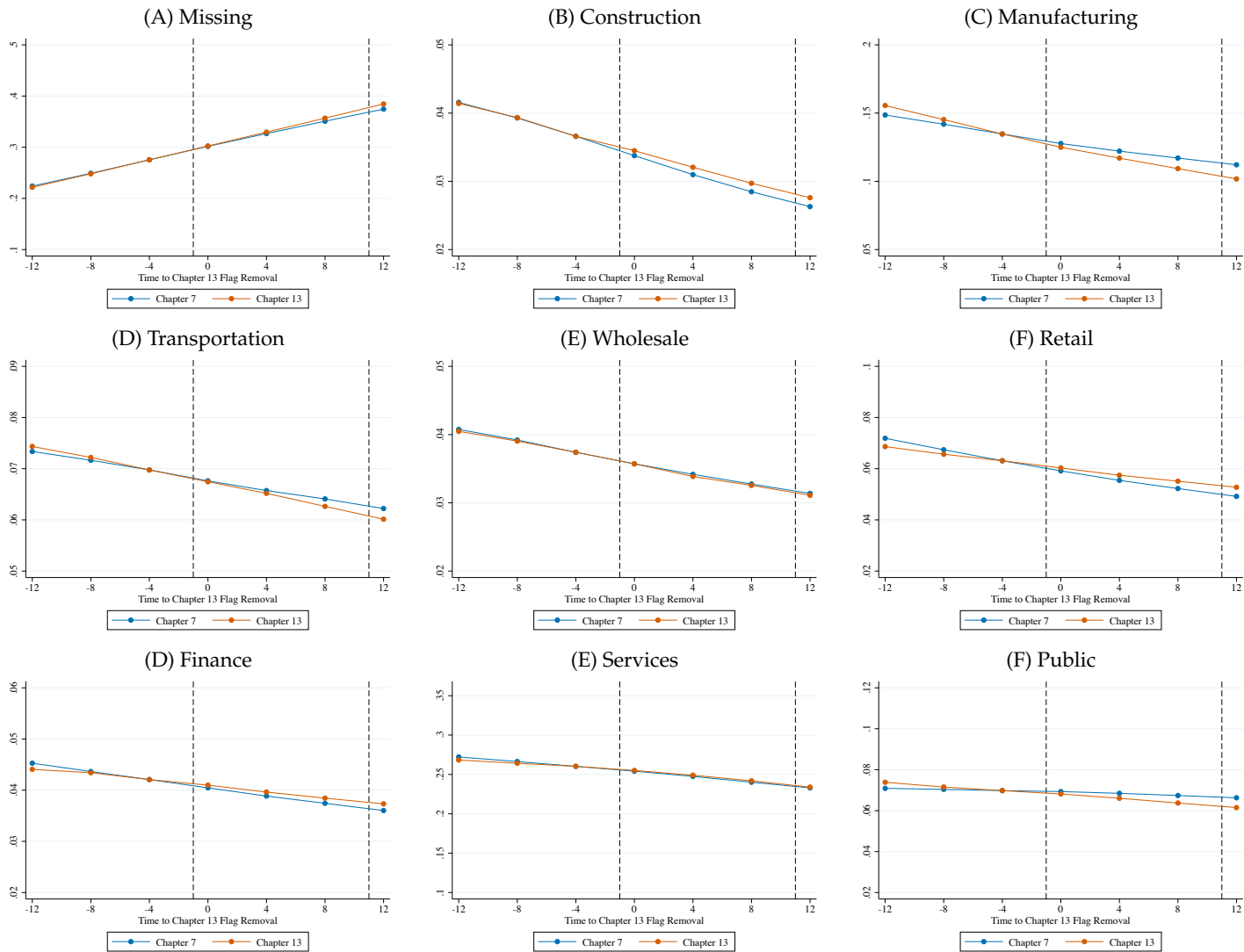
Note: This figure plots the average labor market outcomes of bankruptcy filers to the event of bankruptcy flag removal. The horizontal axis denotes time, in years, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal and the year Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the year prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A14: Effect of Chapter 13 Flag Removal on Employment Transitions



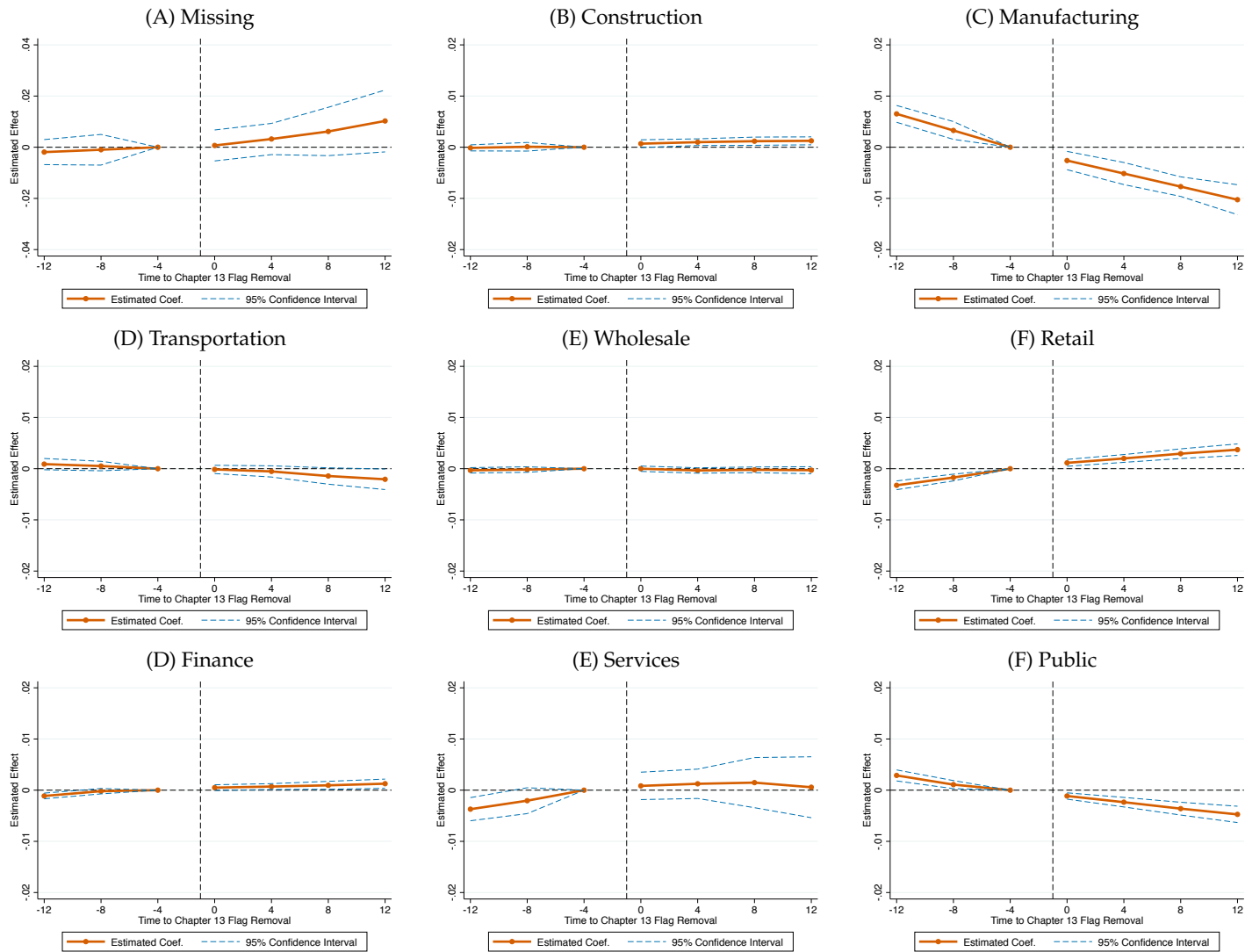
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes. The horizontal axis denotes time, in years, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal and the year Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the year before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A15: Employment by Industry Trends



Note: This figure plots the average labor market outcomes of bankruptcy filers to the event of bankruptcy flag removal. The horizontal axis denotes time, in years, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal and the year Chapter 7 flag removal, which occurs 3 years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the year prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A16: Effect of Chapter 13 Flag Removal on Employment by Industry



Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes. The horizontal axis denotes time, in years, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal and the year Chapter 7 flag removal, which occurs 3 years later. The estimated effect is normalized to zero in the year before Chapter 13 flag removal period. The dashed lines are 95 percent confidence intervals from standard errors clustered at the chapter-by-cohort-by-state level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A1: Credit Results by Pre-Score

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
<i>Score Quartile 1:</i>				
Credit Score	581.958 (31.491)	7.659*** (0.482)	2.568*** (0.669)	-1.370* (0.757)
Credit Card Limits	2,138.805 (1,670.319)	275.844*** (24.689)	704.142*** (47.572)	964.405*** (61.943)
Credit Card Balance	1,474.034 (1,114.509)	143.417*** (20.600)	463.891*** (37.891)	644.333*** (45.572)
Had Mortgage Loan	0.383 (0.196)	0.016*** (0.003)	0.015*** (0.004)	0.005 (0.004)
<i>Score Quartile 2:</i>				
Credit Score	600.589 (32.770)	11.004*** (0.489)	7.362*** (0.679)	4.029*** (0.803)
Credit Card Limits	2,910.358 (2,185.702)	322.861*** (29.892)	1,035.858*** (57.593)	1,548.508*** (77.688)
Credit Card Balance	1,840.818 (1,443.876)	137.834*** (22.126)	529.939*** (40.277)	889.074*** (54.404)
Had Mortgage Loan	0.427 (0.213)	0.025*** (0.003)	0.026*** (0.004)	0.020*** (0.005)
<i>Score Quartile 3:</i>				
Credit Score	614.503 (35.089)	11.650*** (0.532)	9.698*** (0.772)	7.845*** (0.898)
Credit Card Limits	3,495.116 (2,878.317)	425.098*** (36.231)	1,117.600*** (71.746)	1,503.226*** (92.155)
Credit Card Balance	2,080.997 (1,928.362)	186.390*** (28.814)	569.670*** (50.208)	858.038*** (66.994)
Had Mortgage Loan	0.433 (0.234)	0.027*** (0.003)	0.024*** (0.004)	0.015*** (0.005)
<i>Score Quartile 4:</i>				
Credit Score	630.800 (35.906)	16.758*** (0.566)	14.706*** (0.818)	12.349*** (0.957)
Credit Card Limits	4,602.524 (3,429.113)	632.961*** (47.995)	1,632.403*** (96.254)	2,166.135*** (120.686)
Credit Card Balance	2,468.119 (2,259.208)	230.228*** (29.599)	735.610*** (56.966)	1,076.704*** (75.241)
Had Mortgage Loan	0.444 (0.242)	0.024*** (0.003)	0.023*** (0.004)	0.016*** (0.005)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal by predicted pre-flag removal credit score. We construct pre-removal credit scores from a linear regression of pre-flag removal credit score on five-year age buckets and state fixed effects. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the quarter prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal from 0-3, 4-7, and 8-11 quarters, respectively. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. The regressions include 8,558,837 person-year-quarter observations for 471,561 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A2: Employment Results By Industry

	Mean at	Difference-in-Difference Estimates		
	$t = -1$	1 Year	2 Years	3 Years
	(1)	(2)	(3)	(4)
Missing	0.317 (0.080)	0.001 (0.001)	0.002* (0.001)	0.005*** (0.002)
Construction	0.039 (0.031)	0.000 (0.000)	0.001** (0.000)	0.001** (0.000)
Manufacturing	0.106 (0.057)	-0.003*** (0.001)	-0.006*** (0.001)	-0.008*** (0.001)
Transportation	0.059 (0.028)	-0.001** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Wholesale	0.036 (0.018)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Retail	0.075 (0.024)	0.002*** (0.000)	0.003*** (0.000)	0.003*** (0.000)
Finance	0.046 (0.022)	0.000 (0.000)	0.001* (0.000)	0.001* (0.000)
Services	0.256 (0.091)	0.002*** (0.001)	0.003*** (0.001)	0.003*** (0.001)
Public	0.054 (0.029)	-0.001*** (0.000)	-0.002*** (0.000)	-0.003*** (0.000)

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes in each industry. Column 1 reports the dependent variable mean and standard deviation for Chapter 13 filers in the year prior to flag removal. Columns 2-4 report coefficients on the effect of flag removal for years 1-3. The sample includes Chapter 13 and Chapter 7 filers who were age 25-55 when they filed for bankruptcy, successfully discharged their debt, and had their bankruptcy flag removed sometime between 2001-2011. The regressions include 32,797,030 person-year observations for 4,685,290 unique individuals. Standard errors are clustered at the chapter-by-cohort-by-state level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A3: State Credit Check Bans

State	Year of Ban
Washington	2007
Hawaii	2009
California	2010
Illinois	2010
Oregon	2010
Maryland	2011
Connecticut	2012
Vermont	2012
Colorado	2013
Nevada	2013

Note: Note: This table lists all states with a law banning employer credit checks. All states except Washington have an exception for the financial industry. See Clifford and Shoag (2016) for additional details.