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

Will Dobbie[†]   Paul Goldsmith-Pinkham[‡]   Neale Mahoney[§]   Jae Song[¶]

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Abstract

We study the financial and labor market impacts of bad credit reports. Using difference-in-differences variation from the staggered removal of bankruptcy flags, we show that bankruptcy flag removal leads to economically large increases in credit limits and borrowing. Using administrative tax records linked to personal bankruptcy records, we estimate economically small effects of flag removal on employment and earnings outcomes. We rationalize these contrasting results by showing that, conditional on basic observables, "hidden" bankruptcy flags are strongly correlated with adverse credit market outcomes but have no predictive power for labor market outcomes.

[†]Princeton University and NBER. Email: wdobbie@princeton.edu

[‡]Yale School of Management. Email: paulgp@gmail.com

[§]Chicago Booth and NBER. Email: neale.mahoney@gmail.com

[¶]Social Security Administration. Email: jae.song@ssa.gov

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]

Proponents of this trend argue that the increased use of credit reports 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) and the so-called right to be forgotten (Steinberg, 2014).

Critics have been particularly concerned about the use of credit reports in hiring decisions in the labor market. In the years following 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] The importance of credit reports in the labor market is not lost on the general public. In a recent nationally-representative survey, 52 percent of respondents reported that credit scores are typically used "by a potential employer deciding whether to offer you a job" (Armantier et al., 2017).

This paper studies the effect of an improved credit report on both financial and labor market outcomes. Our research design uses the sharp removal of personal bankruptcy "flags" from credit reports at statutorily determined time horizons. Nearly all households that declare bankruptcy file under either Chapter 7 or Chapter 13 of the U.S. Bankruptcy Code.[3] Under the Fair Credit Reporting Act (FCRA), credit bureaus are required to remove Chapter 7 bankruptcy flags ten years after filing.

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[1]The SHRM (2010) reported that 60 percent of employers conducted 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.

[2]See 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.

[3]Under 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 Section 4 for additional details of the bankruptcy system in the United States.

In contrast, credit bureaus traditionally remove Chapter 13 flags only seven years after filing, three years before the Chapter 7 flag is removed.[4]

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 seven years, to Chapter 7 filers (the "control" group), who have their flags removed at ten years and are therefore unaffected at the seven-year time horizon. The identifying assumption for this difference-in-differences specification is that, in the absence of the Chapter 13 bankruptcy flag removal, outcomes for treated and control individuals would have evolved in parallel. To provide support for this "parallel trends" 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 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 provide us with panel information on nearly all credit products held by an individual over time. To examine the effects on labor market outcomes, we use data from individual bankruptcy filings merged to administrative tax records at the Social Security Administration (SSA). Our primary analysis sample, which focuses on prime-age adults with a bankruptcy flag removal between 2002 and 2011, covers roughly 300,000 individuals in the Equifax sample and 3.5 million individuals in the SSA sample.

We begin our analysis by examining the "first stage" effect of the bankruptcy flag removal on credit scores. Since bankruptcy flags enter the credit score formula, this effect is "mechanical" in the sense that it could be directly calculated if one had the proprietary credit score formula and all of the input variables. In practice, we show that bankruptcy flag removal leads to an immediate 9 point increase in credit scores on a pre-flag removal mean of 594 (26 percent of the pre-flag removal standard deviation). The jump occurs precisely in the quarter of bankruptcy flag removal and its impact declines over time. Since credit scores are based on a regression of default on observables, we can also interpret this effect in terms of a change in the implied probability of default. Using this measure, we find that flag removal leads to a 3 percentage point decline in the implied default probability on a pre-flag removal mean of 32 percent (roughly a 9 percent decrease in riskiness).

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[4]Under 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 seven years in their documentation, and we have confirmed this independently using the Equifax credit report data described below. We have also confirmed that the Chapter 7 flag is removed at ten years, as mandated by the FRCA, using the Equifax data.

We next show that flag removal has a statistically significant and economically large effect on credit card borrowing. The effect appears immediately and grows linearly over time. At a three-year horizon, we estimate that flag removal increases credit limits by approximately one-half, or $1,328 on a pre-flag removal mean of $2,784 (56 percent of the pre-flag removal standard deviation), and raises credit card balances by more than 40 percent, or $781 on a pre-flag removal mean of $1,819 (48 percent of the pre-flag removal standard deviation). The ratio of the increase in balances to the increase in credit limits is 59 percent, although we caution that the effect should not be interpreted as a pure MPC out of liquidity because some of the effect may operate through lower interest rates, which we do not observe. Credit limits are also determined by credit card balances, so this ratio may reflect some reverse causality.

We find a similarly large effect on mortgage borrowing. In contrast to the credit card results, the mortgage effect is concentrated in the first year. One year after flag removal, the fraction of individuals with a mortgage increases by 2.0 percentage points on a pre-flag removal mean of 41.0 percent (9 percent of the pre-flag removal standard deviation). In our heterogeneity analysis, we show that the effect is concentrated among individuals who had their flags removed during the 2008 to 2011 period, with no mortgage effect of flag removal in prior years. These results are consistent with widespread mortgage access in the run-up to the Financial Crisis when subprime mortgage lenders provided loans to consumers with blemished credit reports, and substantial pent-up demand in the post-crisis period due to significantly tighter lending standards in the mortgage market.

The large effects of flag removal on credit market outcomes do not necessarily imply that bankruptcy flags will have similarly large effects on labor market outcomes, however. The removal of a bankruptcy flags should increase *labor demand* if these flags are a quantitatively important signal of labor market productivity, conditional on other variables observed by the employer, but should have little effect if they have little incremental predictive value. The increase in credit supply might also impact an individual's *labor supply*, but both the direction and magnitude of any effects are uncertain.

In stark contrast to our credit market results, we estimate economically small effects of flag removal on labor market outcomes. At a three-year horizon, the 95 percent confidence intervals allow us to rule out employment effects greater than 0.4 percentage points (1.3 percent of the pre-flag removal standard deviation) and earnings effects greater than 0.4 percent (4.6 percent of the pre-flag removal standard deviation). We estimate similarly small effects on self-employment and self-employment earnings, indicating that these results are not masking reallocation between different types of work.

We find some evidence of modest positive effects on job-finding rates among non-employed individuals, where we might expect the largest impact of flag removals, and on employment in the finance industry, the sector that is most likely to use credit checks to screen applicants (SHRM, 2010). However, these subsample results are very sensitive to the empirical specification we use, suggesting caution in over-interpreting the results.

The estimated labor market effects are not only small in absolute magnitude but also small relative to other work examining the link between credit and labor market outcomes. In Sweden, Bos, Breza and Liberman (2018) show that removing credit default information increases employment by about 3 percentage points and earnings by about 7 percent over the next year. In the United States, Dobbie and Song (2015) show that being granted Chapter 13 bankruptcy protection increases employment 6.8 percentage points and earnings by 25.1 percent over the next 5 years. The Dobbie and Song (2015) estimates combine the beneficial effects of being granted bankruptcy protection (e.g., discharging most unsecured debts and eliminating wage garnishment orders) with the negative effects from receiving a bankruptcy flag (i.e., the effects estimated in our paper). Our baseline 3-year employment effect of only 0.2 percentage points indicates that the negative effect of receiving a bankruptcy flag is an order of magnitude smaller than the net benefits of being granted bankruptcy protection in the United States, and very small compared to the effects of removing credit default information in the Swedish context. We discuss how our paper relates to Bos, Breza and Liberman (2018) and other related work in Section 2.

Because of the contrary anecdotal evidence linking credit reports and employment, as well as the larger effects of removing default information in Sweden, we conduct a broad set of heterogeneity and sensitivity analyses of these labor market results. (i) We estimate economically small effects for nearly all demographic groups, including for minorities for whom there has been particular concern about the employment consequences of derogatory credit reports.[5] (ii) We find economically small effects across the business cycle, including just after the Financial Crisis when labor markets were slack. (iii) We similarly find no labor market effects using a separate difference-in-differences analysis of the 11 state-level bans on employer credit checks that were implemented in the second half of our sample period.

In the last part of the paper, we consider two potential explanations for the economically small

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

labor market effects. The first is countervailing effects on labor supply and demand. Since flag removal increases access to credit, it might reduce labor supply through a credit smoothing channel, thereby offsetting any increase in employers' labor demand. This explanation is rejected by basic price theory. If an inward shift in labor supply and outward shift in labor demand have counteracting effects of equilibrium quantity, then a supply and demand diagram shows that equilibrium prices must increase. Since we find no wage or earning effects, we can reject this explanation. The economically small effects of state-level bans on employer credit checks provides further evidence against this explanation. Since these bans do not affect lending markets, they should isolate the effect of derogatory credit report information on employers' labor demand. The zero effect in this state-ban analysis therefore indicates that an offsetting labor supply effect is unlikely to explain our result.

The second potential explanation for the economically small labor market effects is that bankruptcy flags may have little value in predicting future job performance. We investigate this theory by examining the explanatory value of "hidden" bankruptcy flags – recently removed bankruptcy flags that are observed by the econometrician but unobserved by new lenders and employers. We first confirm that hidden bankruptcy flags are strongly predictive of future loan performance. Conditional on lagged credit scores, a hidden bankruptcy flag is associated with a 5.2 percentage point increase in the probability of having a credit card delinquency in the next three years relative to a non-filer mean of 19.3 percent (an increase of 0.33 standard deviations). For mortgage debt, individuals with a hidden flag are 11.1 percentage points more likely to have a delinquency relative to a non-filer mean of 11.7 percent (an increase of 1.0 standard deviations). In contrast, we find no correlation between the hidden bankruptcy flags and measures of job performance. Conditional on starting wage decile and industry, hidden flag is associated with no more than a 0.9 percentage point difference in the probability of being at the same job three years later relative to a non-filer mean of 35.2 percent (a difference of 0.04 standard deviations). For employment at any firm, the difference is less than 0.02 percentage points relative to a non-filer mean of 83.9 percent (a difference of 0.015 standard deviations). These results suggest that bankruptcy flags have limited value for predicting future job performance, rationalizing the economically small labor market effect documented above.

From a policy perspective, our results speak most directly to policy reforms that adjust the length of time that derogatory remarks remain on credit reports. Adjusting time horizons is a natural policy to consider. There is significant variation in the time horizons that different remarks stay on credit reports within the United States (e.g., seven years for Chapter 13 flags versus ten years for Chapter 7

flags), as well as the time that the same type of remark stays on credit reports across different countries (e.g., three years for most derogatory remarks in Sweden versus six years in the UK and Canada and 15 years in Brazil). The local average treatment effects (LATEs) that we estimate are the policy relevant parameters for understanding the effect of reforms that would modestly decrease (or increase) the length of time that bankruptcy flags remain on credit reports.

Our results also speak to policy reforms that restrict employer credit checks at all time horizons, although we are careful about extrapolating from our LATEs to the effects of these more comprehensive bans.[6] Our estimates are less relevant to these policies if employers place more weight on other types of derogatory remarks (e.g., default flags) or on more recent remarks (e.g., new bankruptcy flags).

The rest of our paper proceeds as follows. Section 2 discusses the related literature. Section 3 presents background on credit reporting and describes our data. In Section 4, we present our research design, and in Section 5, we presents our results for the credit market and labor market outcomes. Section 6 presents additional analysis to aid the interpretation of our findings. Section 7 concludes.

2 Related Literature

Our paper is closely related to two papers conducted in parallel to our own on the labor market consequences of bad credit reports. Herkenhoff, Phillips and Cohen-Cole (2016) use administrative employment data from the Census and an event-study design to estimate the employment effects of bankruptcy flag removal. The identifying assumption in their event-study design is that, conditional on controls, outcomes would not have differentially changed under the counterfactual where bankruptcy flags are not removed. In contrast, our difference-in-differences research design uses outcomes from the control group to construct the counterfactual under which bankruptcy flags are not removed, which allows the counterfactual to vary flexibly in event time and relies on the weaker parallel trends assumption.

Like us, Herkenhoff, Phillips and Cohen-Cole (2016) estimate economically small effects of flag removal on self-employment and formal sector employment and earnings. In contrast to our findings, however, they find that flag removal increases churn in and out of employment. We estimate the

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[6]Since 2007, 11 states having passed laws to restrict employer credit checks and federal legislators having introduced a similar law in 2015. The 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 Bartik and Nelson (2016), Clifford and Shoag (2016), Friedberg et al. (2017), and Cortés, Glover and Tasci (2018), for more on these policies.

effects of flag removal on transitions in and out of employment using both an event-study and our preferred difference-in-differences research design in Section 5, finding that the results are very sensitive to the estimating specification. This sensitivity raises questions about the strong interpretation of these findings.

Our paper is also related to Bos, Breza and Liberman (2018), who study a policy that reduced the time that information on default was listed on credit reports in Sweden. Focusing on individuals who defaulted on a pawnshop loan, Bos, Breza and Liberman (2018) find that the removal of default information leads to a roughly 3 percentage point increase in employment and 7 percent increase in earnings over the next year.

A natural explanation for the differing results is that levels of financial distress are more than an order of magnitude lower in Sweden than the United States. The 2008 EU-SILC survey indicates that only 0.6 percent of Swedish households have a mortgage in arrears and only 0.7 percent have a non-mortgage loan in arrears, while the 2007 Survey of Consumer Finances indicates that 20.8 percent of U.S. households are late on debt payments.[7] As a result, a derogatory remark on a credit report is a much more negative signal in Sweden than in the United States, explaining the differing labor market effects and suggesting that Bos, Breza and Liberman (2018) is less relevant for the U.S. policy debate that motivates our study.

Our study is also related to two papers that estimate the credit market effects of bad credit reports. In early work in this area, Musto (2004) estimates the impact of bankruptcy flag removal on credit scores and credit card borrowing using an event-study design and first-differenced outcomes. Musto (2004) finds that flag removal has a positive short-run effect on credit scores and credit card borrowing, but has adverse consequences over longer time horizons (more than 18 months). We find similar effects on short-run credit scores using our difference-in-differences design, but find no evidence of adverse effects over long time horizons (up to three years). Gross, Notowidigdo and Wang (2018) (discussed below) also do not find negative longer run effects, suggesting Musto (2004)'s results may be particular to their context.

In work conducted in parallel to ours, Gross, Notowidigdo and Wang (2018) build on Musto (2004) by estimating the effects of bankruptcy flag removal on credit card limits and credit card balances over time. They use these estimates to construct a time-varying measure of the marginal propensity to consume (MPC) out of liquidity, and find that the MPC is 20 to 30 percent higher during the Great Re-

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[7]See http://ec.europa.eu/eurostat/web/income-and-living-conditions/data/ad-hoc-modules and Table 5: https://www.federalreserve.gov/pubs/bulletin/2014/articles/scf/scf.htm

cession compared to surrounding years. While we do not focus on estimating MPCs over the business cycle, we find similar average effects on credit market outcomes as Gross, Notowidigdo and Wang (2018), lending support for both papers.

Finally, our paper is related to a number of papers studying the labor market effects of recent state-level bans on employer credit checks using state-year difference-in-differences research designs. Clifford and Shoag (2016) find that state credit check bans have a zero average effect among census tracts with average credit scores below 650, but large 1.9 to 3.3 percent employment effects in census tracts with the lowest average credit scores. Cortés, Glover and Tasci (2018) find that bans decrease the number of county-level job vacancies, especially in low-credit score counties, while increasing the number of county-level loan delinquencies and credit inquiries. Bartik and Nelson (2016) find that the bans reduce job-finding rates and increase separation rates for blacks, with mixed effects for whites and Hispanics using individual-level information from the Current Population Survey (CPS) and state unemployment records. Friedberg et al. (2017) find that state credit check bans increase job-finding rates by 25 percent among unemployed individuals who have had trouble meeting their expenses in data from the Survey of Income and Program Participation (SIPP), with no discernible impact among unemployed individuals who have not had recent financial trouble.

The results estimated using variation in the timing of these state-level bans should be interpreted with caution, however. The majority of the state-level bans were implemented just before, during, or after the Great Recession when the labor market was experiencing substantial disruptions, making it difficult to validate the necessary assumptions for a difference-in-differences research design. The existing work in this area also does not use individual-level labor market data linked to derogatory credit reports, making it difficult to isolate individuals affected by the state-level bans. As a result of these issues, the estimated effects could be picking up spurious trends from groups of individuals not actually affected by the bans. Despite our reservations about this research design, we estimate the effects of these state-level bans using individuals with Chapter 7 and Chapter 13 bankruptcy flags and a state-year difference-in-differences research design in Section 5, finding economically small and statistically insignificant effects.

3 Background and Data

3.1 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. The credit reporting industry grew throughout the twentieth century, but remained highly fragmented, with 2,250 local and regional firms as of 1970. During 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 (SSN), 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 an individual credit card, the tradeline data include 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 that information can be maintained on credit reports. Chapter 7 bankruptcies may be listed for ten years after the order for relief or date of adjudication. Conversely, information on Chapter 13 bankruptcies is traditionally removed after a pe-

riod of only seven years. The FCRA also stipulates that information on late payments, delinquencies, and collection items be removed after seven years.

Requesters 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 date of birth or credit scores. Lenders, on the other hand, usually receive at least one consumer credit score, in addition to all of the standard credit report information. These credit scores are sometimes developed by third parties, such as the Fair Isaac Corporation (FICO), and sometimes developed by the credit bureaus themselves (e.g., the VantageScore). There are also dozens of different types of credit scores, each based on different outcome variables and used for different types of lending decisions. The most commonly used credit scores aim to predict the probability that a consumer will become 90+ days delinquent on a new loan within the next 24 months. See CFPB (2012) for more background on the U.S. credit reporting system.

3.2 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 used in our analysis is constructed using records from the Federal Reserve Bank of New York's Equifax Consumer Credit Panel (CCP), a representative five percent random sample of 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 year of birth and geographic location at the ZIP-code level. No other demographic information is available at the individual level. Importantly, the data also include information on the 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.

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[8]The 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.

[9]We 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 seven years before the quarter of flag removal for Chapter 13 filers, and ten 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.

We make four sample restrictions to the CCP data. First, we restrict the sample to individuals who filed for bankruptcy protection between 1995 and 2004. This restriction allows us to observe credit outcomes both before and after the flag removal. Second, we restrict our sample to individuals who were between 23 and 47 years old at filing – and therefore between 30 and 54 years old at seven years after filling – to focus on working-age adults. Third, we restrict the sample to individuals who completed the bankruptcy process, receiving what is known as a discharge.[10] Fourth, we drop individuals from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See Appendix Table A1 for a complete list of states with credit check bans.

The second dataset constructed for this study consists of individual bankruptcy filings merged to administrative tax records at the Social Security Administration (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.[11] 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 SSN. 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 SSN in the SSA data.[12] 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 formal sector earnings in that year, as is standard in papers using administrative data containing the universe of formal sector employers (e.g., Dobbie and Song 2015). Individuals with zero earnings are included in all regressions throughout the paper. The SSA data are available annually from 1978 to 2014.

In the SSA data, we measure bankruptcy flag removal using the exact date-of-filing in the individual-level bankruptcy records and the seven- and ten-year removal rules mandated under the FCRA. To confirm the accuracy of this measure of flag removal, Appendix Figure A1 plots the distribution of flag removal timing in the subset of filers in the CCP data, where we observe both the exact quarter in which the bankruptcy flag is added and removed from the credit report data. Consistent with the removal rules mandated under FCRA, nearly all Chapter 13 and Chapter 7 bankruptcy flags are

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[10]Chapter 13 filers who do not receive a discharge have their flags removed after ten years and therefore cannot be used in our research design.

[11]We 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.

[12]The SSA data include every individual who has ever acquired an 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.

removed at exactly seven and ten years after the bankruptcy filing, respectively.

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 2004, were between 30 and 54 years old seven years after filing, successfully completed the bankruptcy process, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. All dollar amounts in the SSA dataset are adjusted to year 2013 dollars using the CPI-U.

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

4   Research Design

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

Our sample of 1995 to 2004 bankruptcy filings occurred before the 2005 bankruptcy reform (i.e., the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005, or BAPCPA). Below we provide a brief overview of Chapter 13 and Chapter 7 bankruptcy under the pre-reform bankruptcy code; see Dobbie and Song (2015), Dobbie, Goldsmith-Pinkham and Yang (2017) and Mahoney (2015) for a more in-depth treatment. Under Chapter 7, bankruptcy filers forfeit all non-exempt assets in exchange for a discharge of eligible debts and protection from future wage garnishment. Nearly all unsecured debts are eligible for discharge under Chapter 7, including credit card debt, installment loans, medical debt, unpaid rent and utility bills, tort judgments, and business debt. Student loans, child support obligations, and debts incurred by fraud cannot be discharged under Chapter 7, and secured debts such as mortgages, home equity loans, and automobile loans can only be discharged if filers give up the collateral.

Under Chapter 13 bankruptcy, filers propose a three- to five-year plan to partially repay their unsecured debt in exchange for a discharge of the remaining unsecured debt, a hold on debt collection, and the retention of most assets. Chapter 13 requires filers to use all of their disposable income, defined as their predicted income less predicted expenses, to repay creditors. Creditors must receive at least as much as they would have received if the filer's assets were liquidated under Chapter 7, a requirement known as the "best interest of creditors" test. Chapter 13 filers are also required to

fully repay priority claims, such as child support and alimony, unless the claimant agrees to a reduced payment. If a filer wants to keep any collateral securing a claim, he or she must keep up to date on all current payments and include any arrears in the repayment plan. The filer can also choose to give up the collateral and discharge the remaining debt. In our main results, we include all Chapter 13 filers, regardless of repayment plan length. In our estimation sample, Chapter 13 filers took an average of just over 3.5 years to complete their repayment plans. In results available upon request, we also find similar results restricting the sample to filers with either three-year or five-year repayment plans.

In any given year, approximately 70 percent of filers choose Chapter 7 of the bankruptcy code, with the remaining 30 percent choosing Chapter 13 (White, 2007). One reason why individuals choose Chapter 13 is that it allows filers to avoid a home foreclosure or the repossession of a car by including any arrears in the repayment plan, with the original debt contract reinstated on completion of the Chapter 13 repayment plan. Thus, perhaps not surprisingly, the biggest difference between Chapter 13 and Chapter 7 filers in Table 1 is the fraction of individuals with a mortgage (41.3 percent for Chapter 13 filers versus 33.1 percent for Chapter 7 filers). However, there is also evidence that filers are steered into Chapter 13 by lawyers who earn larger payments from Chapter 13 filings, generating variation in filing chapter that is more likely to be uncorrelated with the individual's financial circumstances (Braucher, Cohen and Lawless, 2012). As we discuss below, our research design does not rely on the random assignment of filing chapter. Rather, the key identifying assumption for our difference-in-differences specification is that the differences in outcomes for Chapter 13 versus Chapter 7 filers would have evolved in parallel in the absence of the Chapter 13 bankruptcy flag removal.

We conduct our analysis using individual-level data collapsed to a more aggregate level to speed up the regression analysis. In the CCP data, we collapse by the full interaction of chapter of filing, cohort of filing, time period, state of residence, and five-year age bins. In the SSA data, we also observe race (defined as white or non-white) and gender, so we additionally collapse on these dimensions.[13] 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, collapsing the data does not affect the statistical inference because we cluster our standard errors above the level of aggregation.

The precise regression specifications will naturally differ based on whether we use the quarterly

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[13]In the CCP data, cohort and time period are defined at the year-quarter level and state of residence is defined using the state of residence six 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.

CCP data or the annual SSA data. Consider first the quarterly CCP data. Let i index *filing groups*, defined by the full interaction of chapter of filing, cohort of filling, state of residence, and five-year age bin. Let s index *calendar-time*, defined at the year-quarter level. Let t indicate *event-time*, defined as quarters relative to the seven-year horizon when Chapter 13 bankruptcy flags removed. We define t using this seven-year horizon for both Chapter 13 and Chapter 7 filers even though Chapter 7 filers have their flags removed at ten 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:

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

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.[14]

In this specification, the β_t coefficients for $t > 0$ can be interpreted as the differential change in y_{it} for Chapter 13 filers relative 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 is 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

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[14]In this specification, we are only able to control for year, rather than year-quarter, calendar-time fixed effects due to the collinearity of the event-time and calendar-time fixed effects. In our table results, we are able to control for year-quarter calendar-time fixed effects since our event-time estimates are simplified into three coefficients. When we estimate this specification using the annual SSA data, we are analogously only able to control for two-year calendar-time fixed effects when we include all event-time fixed effects, but are able to control for single-year calendar-time fixed effects in the tables, where our event-time estimates are again simplified into three coefficients.

close parallel during the pre-flag removal period for most outcomes. These results give us confidence that our control group is valid and that it provides us with an accurate counterfactual for what would have happened to the treatment group in the absence of flag removal.

It is worth comparing this identifying assumption to that required by the event-study specifications used in related papers (see discussion in Section 2). The event study design is based on the assumption that outcomes would not have differentially changed under the counterfactual where bankruptcy flags are not removed, or that any changes can be captured by controls or a parametric trend estimated in the pre-flag removal period and extrapolated into the post-flag removal period. In contrast, our difference-in-differences research design uses outcomes from the control group to construct the counterfactual under which bankruptcy flags are not removed, which allows the counterfactual to vary flexibly in event time and relies on the weaker parallel trends assumption. Over shorter time horizons, the extrapolation of pre-flag removal trends into the post-removal period required by an event study design is reasonable, both in theory and practice. In Appendix B, for example, we show that our difference-in-differences estimates are roughly similar to parametric event-study estimates at a one-year time horizon. Over longer time horizons, however, the extrapolation of pre-flag removal trends into the post-removal period is more questionable. We show in Appendix B, for example, that a parametric event study design sometimes yields estimates that are substantially different from our preferred difference-in-differences approach.

To gauge magnitude and statistical significance of our parameter estimates, we estimate specifications that pool the effect across sets of consecutive periods. In the CCP data, we estimate a specification where we replace the quarter-specific coefficients with annual coefficients for the 3 years post flag removal. (Since the SSA data is annual, the baseline specification already has annual coefficients.) In CCP and SSA data, we estimate specifications where we replace the coefficients over the 3 years post-flag removal into a single pooled coefficient. Other than these pooled coefficients, these specification are identical to 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 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 report p-values that account for the family-wise error rate (FWER), defined as the probability

of making one or more false discoveries, using the Holm FWER method ([Holm, 1979]). Correcting for the FWER is necessary in our context because we examine multiple outcomes within the same domain, making it likely that at least some of our estimates will be statistically significant due to chance alone. For a given family of k-hypothesis tests, the Holm method uses step-down methods to provide corrected *p*-values. The Holm method is uniformly more powerful (i.e., lower risk of Type II errors) than the well-known Bonforroni correction, which multiplies the unadjusted *p*-values by the number of hypotheses that are tested. Following [Finkelstein et al. (2012)], we make these adjustments within domains defined by the set of outcomes considered in each table (e.g., we adjust within all labor market outcomes in Table 5). The resulting family-wise *p*-value corresponds to the probability of rejecting the null hypothesis of no effect on a given outcome under the null family of hypotheses of no effect on any outcome in the domain.

5 Results

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

5.1 Credit Scores

We begin with a descriptive analysis of how credit scores evolve for Chapter 13 and Chapter 7 bankruptcy filers just before and just after flag removal. The effect of credit scores can be thought of as a "first stage" – if there were no effect on credit scores, we would be unlikely to detect an effect on other outcomes. As we mention above, since bankruptcy flags enter the credit score formula, the credit score effect is "mechanical" in the sense that it could be directly calculated if one had the proprietary credit score formula and all of the input variables. Since credit scores are used in the vast majority of lending decisions, improvements in credit scores should directly translate into increased credit availability, lower interest rates, or both ([FRB, 2007]).

Figure 1 provides some background on credit scores. The credit score we observe – the Equifax Risk Score 3.0 – is the output of "a general-purpose risk model that predicts the likelihood of a consumer becoming seriously delinquent (i.e., 90+ days past due) within 24 months of scoring."[15] The observed credit score is constructed as an odds-scale measure of risk, with the odds of going seriously delinquent doubling for every 33 point decline in the credit score measure. The exact credit score

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[15]See http://www.equifax.com/biz/debt_recovery_pdfs/EFS-670-ADV%20ERS%203.0.pdf

formula is a proprietary trade secret.[16]

Panel A of Figure 1 plots the distribution of credit scores for a representative, unrestricted sample of the CCP data and for our analysis sample of bankruptcy filers. In the representative sample, credit scores have a mean of 686 and an interquartile range of 607 to 778. Our analysis sample is drawn from the lower part of the distribution, and has a mean of 600 and an interquartile range of 553 to 658. Panel B shows the relationship between observed credit scores and serious delinquency (90+ days past due).[17] The mean credit score of 600 in our analysis sample corresponds to a 30.2 percent probability of default in the following two years. To facilitate the economic interpretation of the effect on credit scores, we construct an alternative outcome measure called the *implied probability of default*, which is simply the credit score mapped into a probability of default using the relationship shown in Panel B of Figure 1.

Figure 2 plots average credit scores and the implied probability of default for Chapter 13 filers (the "treatment" group) and Chapter 7 filers (the "control" group) for each quarter relative to Chapter 13 flag removal. The vertical lines show the quarter of Chapter 13 flag removal and the quarter of Chapter 7 flag removal, which occurs three years later. Outcomes are normalized to the average value of the outcome for Chapter 13 filers in the quarter prior to flag removal.

Panel A shows that prior to flag removal, credit scores for both groups trend upwards together, confirming the parallel trends identifying assumption. In the quarter of Chapter 13 flag removal, there is a clear jump of approximately ten points for the credit scores of Chapter 13 filers (0.26 pre-flag removal standard deviations). After flag removal, average credit scores for Chapter 7 filers continue their upward trend, although at a slightly lower rate, and average credit scores for Chapter 13 filers decline slightly in absolute value and more strongly relative to the Chapter 7 control group.[18] At three years post-removal, the credit scores of Chapter 13 filers remain approximately 3 points above those of Chapter 7 filers.

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[16]The FICO credit score is constructed in a similar fashion. See CFPB (2011) for more information.

[17]To avoid complications from over-time variation in default rates, the figure focuses on credit scores in the first quarter of 2005 and default rates in the first quarter of 2007, which is a time period near the midpoint of our sample.

[18]The 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 seven years for both Chapter 13 and Chapter 7 filers. That is, since individuals are generally receiving flags for delinquencies and collections items *prior* to filing for bankruptcy, the upward slope in credit scores prior to bankruptcy flag removal partially reflects the removal of these flags. However, since individuals are generally no longer receiving new delinquencies and collections flags *after* the bankruptcy filing, there are no more removals of delinquencies and collections flags after the bankruptcy flag removal, and hence the slope of credit scores is flatter. Accounting for the change in slope around flag removal is a key advantage of our difference-in-differences research design relative to an event-study design. See Dobbie, Goldsmith-Pinkham and Yang (2017) for an analysis of pre- and post-filing trends in credit scores, delinquencies, collection items, and other credit market outcomes and Dobbie and Song (2015) for an analogous analysis on pre- and post-filing trends in labor market outcomes.

Panel B shows trends for the implied probability of default. The pre-flag removal trend is downward because of the inverse relationship between credit scores and default probabilities. In the quarter of Chapter 13 flag removal, the implied probability of default drops by approximately 3 percentage points on a pre-removal mean of 32 percent (0.24 pre-flag removal standard deviations). Post-flag removal, there is some reversion, and at three years post-flag removal, there is only a 0.5 percentage point difference between Chapter 13 and Chapter 7 filers in the implied probability of default.

Table 2 shows results from our parametric difference-in-differences regressions that pool the effect across the first, second, and third years after flag removal – along with showing an effect that pools over all three years. In these 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-year-by-age-by-state and calendar-year fixed effects. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise error rate p-values are in brackets. The point estimates indicate that credit scores increase by 9.4, 5.5, and 2.2 points, and that the implied probability of default declines by 2.6, 1.4, and 0.3 percentage points in the first, second, and third years after flag removal, respectively.

Appendix Figure A2 plots the coefficient of interest from the non-parametric difference-in-differences specification shown in Equation (1) with credit scores and the implied probability of default as the dependent variables. The plots show no systematic relationship between flag removal and these outcome variables in the pre-removal quarters, providing support for our parallel trends identifying assumption, and more generally corroborating our results on the timing and size of the effect. Appendix Section A shows additional results that examine heterogeneity in the credit score effect by predicted pre-flag removal credit score. Appendix B presents event study estimates of the effect of Chapter 13 flag removal for the credit score effects, and the other main outcomes we analyze. The event study estimates are typically similar in the first year after flag removal but differ over long time horizons.

5.2 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. 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. (2017) show that for consumers with credit scores in the bottom quartile of the distribution,

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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 3 shows average credit limits and balances for Chapter 13 and Chapter 7 filers for each quarter relative to the quarter of Chapter 13 flag removal. Table 3 shows estimates from the corresponding difference-in-differences regressions that pool the effect across the first, second, and third years after flag removal, and over all three years combined. Appendix Figure A3 plots the coefficients of interest from the non-parametric specification shown in Equation (1).

There is an economically and statistically significant impact of flag removal on both 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 an average of $443 per year. In particular, at three years after flag removal, credit limits have increased by $1,328, which is 48 percent the pre-flag removal mean of $2,784 and 56 percent of the pre-flag removal standard deviation. Credit card balances increase by an average of $260 per year. At three years after flag removal, balances have increased by $781, which is 43 percent of the pre-flag removal mean of $1,819 and 48 percent of the pre-flag removal standard deviation. Consistent with the credit score results, both figures show no pre-trend in credit card limits or balances.

It may be 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 estimated in Gross and Souleles (2002) or Agarwal et al. (2017) – 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. These issues notwithstanding, taking the ratio of the increase in credit card balances to the increase in credit card limits results in a value of 59 percent at the three-year horizon, relative to a pre-flag removal utilization rate of 65 percent. These values are large, but almost identical to the MPC out of liquidity estimated in Agarwal et al. (2017) for the bottom quartile of the credit score distribution.

In the Appendix, we examine heterogeneity in these effects by predicted pre-flag removal credit score, following the procedure outlined in the credit score heterogeneity analysis. Appendix Figures

A4 and A5 show trends plots of the effects on credit limits and balances, and Appendix Table A2 shows the parameter estimates. The effects on credit limits and balances are increasing in pre-flag removal credit score. The ratio of the increase in balances to credit limits declines modestly in predicted pre-flag removal credit score, from 66 percent in the bottom quartile to 51 percent at the top quartile, based on the effects at three years after flag removal.[19]

5.3   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 the unadjusted level, 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 capture both extensive and intensive margin changes in mortgage borrowing.

Figure 4 shows the averages values of these outcomes for Chapter 13 and Chapter 7 filers in each quarter relative to the quarter of Chapter 13 flag removal. Table 4 shows estimates from the corresponding difference-in-differences regressions that pool the effect across the first, second, and third years after flag removal, and over all three years combined. Appendix Figure A6 plots of the coefficients of interest from the non-parametric specification shown in Equation (1).

There is an economically and statistically significant effect of flag removal on both the probability of having at least one mortgage and the aggregate measure of mortgage debt. The effects occur almost immediately after Chapter 13 flag removal but fade out over the next three years. The regression estimates in Table 4 show that in the first year after flag removal, the fraction of individuals with a mortgage increases by 2.0 percentage points, which is 4.9 percent of the pre-flag removal mean of 41.0 percent and 9.1 percent of the pre-flag removal standard deviation. Aggregate mortgage balances (plus one) increase by 23 log points in the first year. 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. The pre-trends, shown in Appendix Figure A6, 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 flag

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[19]In Appendix Figure A7, we examine the responsiveness of credit card limits and balances to changes in the implied probability of default. We find that an immediate one percentage point decrease in the implied probability of default increases credit card limits by approximately 230 dollars two years later. In Panel B, we find that a similar decrease in the implied probability of default increases balances by around 30 dollars two years later.

removal.

In the Appendix, we examine heterogeneity in these effects by predicted pre-flag removal credit score, again following the procedure outlined in the credit score heterogeneity analysis. Appendix Figure A8 shows trends for the mortgage indicator, and Appendix Table A2 shows the corresponding parameter estimates. The effect of a flag removal on the probability of having a mortgage is small for the bottom quartile of the predicted pre-flag removal credit score distribution, but approximately the same for the top three quartiles of the distribution. The pre-trends for the bottom quartile also show that there is an increase in mortgage holdings in the quarter before flag removal, suggesting that the effect of flag removal is slightly underestimated for this subsample.

In both the pooled and subsample results, there is also an apparent reversal in the mortgage outcomes, with values for the Chapter 13 filers falling below those for Chapter 7 filers approximately three years after flag removal. In Appendix Figure A9, we further investigate this apparent reversal, focusing on the extensive margin of having any mortgage. Panel A shows the net flow into having a mortgage. Positive net flow is the result of new mortgage originations, and a negative net flow is the result of consumers paying off their mortgage, selling their home, or losing their home in foreclosure. The figure shows a nearly 1.5 percentage point increase in the net flow into mortgages in the quarter of flag removal, underscoring the sharp on-impact effect of the treatment. Consistent with the reversal in mortgage outcomes, however, Chapter 13 filers have lower net flows in the years following the flag removal.

In Panels B and C, we explore this issue further by estimating effects before and after the start of the 2008 Financial Crisis. The results are striking. There is a large, almost 3 percentage point on-impact effect of flag removal on net mortgage flows during the 2008 to 2011 period. In the pre-crisis period, however, there is no on-impact effect of flag removal on net mortgage flows, and a decline in net flows for Chapter 13 relative to Chapter 7 following flag removal. This decline in net flows for Chapter 13 after the flag removal indicates that our research design performs poorly at longer time horizons for mortgage outcomes, particularly during the pre-crisis period. Since homeownership is an important determinant of the choice between a Chapter 13 versus Chapter 7 filing, it is perhaps unsurprising that our research design performs relatively worse for this outcome variable. We therefore prefer to focus on the on-impact mortgage results that are less likely to be affected by this issue.

For these on-impact estimates, our results suggest an economically large effect of flag removal on mortgage originations and mortgage balances, particularly following the 2008 Financial Crisis.

These results are consistent with widespread mortgage access in the run-up to the Financial Crisis, with the subprime mortgage market providing loans to consumers with blemished credit reports, and substantial pent-up demand during the post-crisis period, with tighter lending standards in the subprime market. The timing and size of the effect suggest 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 mortgage effect also underscores the importance of credit scores and bankruptcy flags in this market, a potentially surprising result given the role of other factors like down payments and income-based requirements that are also used to screen mortgage applicants.

5.4   Labor Market Outcomes

Our analysis of credit market outcomes showed large effects, with a 48 percent increase in credit limits indicating that bankruptcy flags are an important determinant of credit worthiness. However, these results do not necessarily imply that bankruptcy flags will have similar large effects on labor market outcomes. The removal of a bankruptcy flags should increase *labor demand* if these flags are quantitatively important signal of labor market productivity, conditional on other variables observed by the employer, but should have little effect if they have little incremental predictive value. The increase in credit supply might also impact individual's *labor supply*, but both the direction and magnitude of any effects are uncertain. In this section, we examine the labor market impacts of bankruptcy flag removal. Section 6 presents additional analyses to aid in the interpretation of the labor market results.

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 each earnings measure, as the distributions of earnings are 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 12.5 percent of Chapter 13 filers have no formal sector earnings, 94.4 percent have no reported self-employment earnings, and 10.0 percent have no earnings of either type.

Figure 5 shows averages of these outcomes for Chapter 13 and Chapter 7 filers for each quarter relative to the quarter of Chapter 13 flag removal. Table 5 shows estimates from our parametric difference-in-differences specification that pools the effect across the first, second, and third years after

flag removal, and over all three years combined. Appendix Figure A10 plots the coefficient of interest from our non-parametric specification shown in Equation (1).

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. At three years after filing, flag removal decreases wage employment by a statistically and economically insignificant 0.2 percentage points on a pre-flag removal mean of 87.5 percent (0.6 percent of the pre-flag removal standard deviation). The 95 percent confidence interval allows us to rule out employment effects larger than about 0.4 percentage points (1.3 percent of the pre-flag removal standard deviation).[20] Similarly, we find that flag removal has virtually no effect on log wages (plus one) with a narrow 95 percent confidence interval at all time horizons. Flag removal also has a statistically and economically insignificant effect on positive self-employment earnings, with an increase of only 0.1 percentage points (1.1 percent of the pre-flag removal standard deviation) at the three-year time horizon and narrow 95 percent confidence intervals at all time horizons. We similarly estimate no discernible effect on self-employment earnings (plus one) at all time horizons. Not surprisingly, given the small effects for both the formal and self-employment outcomes, we also find no effect of flag removal on the outcomes that combine the formal sector and self-employment measures at all time horizons.

The estimated labor market effects are not only small in absolute magnitude but also small relative to other work on the link between credit and labor market outcomes. One comparison is Bos, Breza and Liberman (2018), who show that removing credit default information in Sweden increases employment by about 3 percentage points and earnings by about 7 percent over the next year. Our standard errors allow us to rule out effects greater than 0.4 percentage points for employment and 0.0 percent for earnings, which is almost an order of magnitude smaller than the preferred estimates in Bos, Breza and Liberman (2018).

A second comparison is Dobbie and Song (2015), who show that being granted Chapter 13 bankruptcy protection in the United States increases employment 6.8 percentage points and earnings by 25.1 percent over the next 5 years. The Dobbie and Song (2015) estimates combine the beneficial effects of being granted bankruptcy protection (e.g., discharging most unsecured debts and eliminating wage garnishment orders) with the negative effects from receiving a bankruptcy flag (i.e., the effects estimated in our paper). Our baseline 3-year employment effect of only 0.2 percentage points indicates

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[20]The downward trend in wage employment is not unique to our sample. In Appendix Figure A11, we examine employment trends for individuals of similar ages (age 30 to 55 in 2003) in the Panel Survey of Income Dynamics (PSID). In the PSID data, the percentage of individuals with positive hours worked and positive labor income follows a similar downward trend.

that the negative effect of receiving a bankruptcy flag is very small relative to the net benefits of being granted bankruptcy protection.

In summary, none of our results suggest economically important effects of flag removal on the labor market outcomes of the average bankruptcy filer, both in absolute magnitude and compared to prior work. Consistent with our earlier results, there is also no evidence of different pre-flag removal trends for any of our labor market outcomes. Thus, our identifying assumption also appears to be valid for these outcomes.

5.4.1 Heterogeneity Analysis and Robustness

Our results thus far have shown that there are no economically important effects of bankruptcy flag removal on labor market outcomes, at least for the average filer. Because of the contrary anecdotal evidence, as well as the larger effects of removing default information in Sweden, we conduct a broad set of heterogeneity and sensitivity analyses of these labor market results.

Demographic Heterogeneity: An important 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.[21]

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 flag removal). We estimate statistically insignificant effects for all subgroups except while males age 30-39. Even for this group, the point estimate at 3 years is an economically small 0.80 percentage points relative to a pre-flag removal mean of 91.1 percent (3 percent of the pre-flag removal standard deviation). The effects are smaller for all of other demographic groups and there is no evidence of consistently larger effects for minority groups.

To investigate heterogeneity in the estimates more broadly, Figure 6 plots the estimated effects of

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[21]For 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.

flag removal on formal sector employment at three years for 384 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 with a large number of individuals are more prominent. The employment estimates in our sample are precisely and symmetrically clustered around a median value of 0.597 and mean value of 0.580. The small number of estimates that are statistically significant at the one percent level only account for 3.0 percent of the overall sample, only slightly higher than we would have expected from noise alone.[22] When we construct Family-wise error rate *p*-values, the only group out of the 384 we consider that has a statistically significant effect is non-white females age 40 or older living in Rhode Island. Results are qualitatively similar at different time horizons and for our other labor market outcomes.

Business Cycle Differences: The zero average effect could also mask differences in strong and weak labor markets. For example, it is possible that employers were willing to overlook any blemishes on filers' credit reports during the strong labor market in the first six years of our sample period (i.e., 2002 to 2007). Appendix Figure A12 and Appendix Table A3 investigate this issue by reporting effects for filers whose flags were removed in 2002 to 2007 (the "boom") and for filers whose flags were removed in 2008 to 2011 ("the bust"). The employment effects are statistically and economically indistinguishable across the two time periods.

Labor Market Transitions: A related concern is whether the zero average effects mask large effects for individuals on the margin of the labor force. For instance, individuals who are currently unemployed might benefit the most from a bankruptcy flag removal. An analysis that looks at the overall employment rate might miss a positive effect for this group of individuals. To examine the effect on different groups, 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"). We also 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 outcomes and Appendix Table A4 reports parameter estimates from the corresponding difference-in-differences regressions. We find some evidence of modest positive effects on job-finding rates among non-employed individuals, with the point estimate indicating that flag removal increases the probability of moving from no job to a job at three years by 0.2 percent on a pre-

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[22] When we use a five percent criterion for statistical significance, 10.6 percent of the estimates are statistically significant.

flag removal mean of 1.7 percent (2.0 percent of the pre-flag removal standard deviation). However, the trend plots raise concerns about the parallel trends assumption for many of the job transition outcomes, suggesting that these results may reflect pre-existing trends, and not the causal impact of flag removal. The point estimates are also very small in economic terms for all job transition outcomes, even if we were to take the estimates at face value.

Industry Churn: The zero average effects may also mask churn across industries. For instance, a survey by the Society of Human Resource Management (SHRM, 2010) 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.[23] Appendix Figure A14 shows employment trends by industry and Appendix Table A5 shows the parameter estimates. Consistent with the SHRM (2010), we find some evidence that employment increases in the finance sector and decreases in the public sector following flag removal. However, the trend plots again raise concerns about the parallel trends assumption for these outcomes. The point estimates are also, again, very small in economic terms, with, for example, finance employment at three years increasing by only 0.08 percentage points on a base of 4.2 percent (1.9 percent of a standard deviation).

External Validity: Our primary difference-in-differences research design identifies the LATE of removing a bankruptcy flag seven years after filing. From a policy perspective, our results speak most directly to policy reforms that would modestly decrease (or increase) the length of time that bankruptcy flags remain on credit reports. Adjusting time horizons is a natural policy to consider. There is significant variation in the time horizons that different remarks stay on credit reports within the United States (e.g., seven years for Chapter 13 flags versus ten years for Chapter 7 flags), as well as the time that the same type of remark stays on credit reports across different countries (e.g., three years for most derogatory remarks in Sweden versus six years in the UK and Canada and 15 years in Brazil).

In interpreting the null effects on labor market outcomes, a natural question is whether employers consider a seven-year-old bankruptcy filing to be important relative to other derogatory items that could appear on a job applicant's credit report, such as foreclosure or default flags. The SHRM

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[23]See, for example: http://www.nolo.com/legal-encyclopedia/will-bankruptcy-affect-my-job-future-employment.html.

([2010](#)) survey sheds some light on this matter. Firms were asked, "how many years of credit history are the most influential in your assessment of the job candidate's credit standing?" In response, 33 percent reported focusing on time horizons of "up to 6 or 7 years" and a further 25 percent indicated they considered longer time horizons. 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.

Even so, we would not want to extrapolate from our results on the effects of bankruptcy flag removal to the effects of a broader restriction on employer credit checks if employers place weight on other derogatory items or down-weight a seven-year-old bankruptcy flag relative to a more recent flag. To address this potential limitation, we conduct a separate difference-in-differences analysis of the state-level bans on employer credit checks that were implemented in the second half of our sample period. To focus on individuals affected by the ban, we restrict our analysis to individuals who filed for either Chapter 7 or Chapter 13 bankruptcy in the prior four to six years.[24] To maintain a balanced sample, we also drop states that implemented bans after 2010, because our data does not allow us to track outcomes for a full three years after implementation.[25]

The top row of Appendix Figure A15 shows trends in the fraction employed among bankruptcy filers who reside in states that implemented a ban in 2007, states that implemented a ban in 2010, and control states that never implemented a ban.[26] The bottom row of Appendix Figure A15 shows difference-in-differences plots of the effect of the bans including all states that implemented bans between 2007-2010. The left column shows outcomes for Chapter 7 filers and the right column shows outcomes for Chapter 13 filers. Appendix Table A6 shows the corresponding difference-in-differences coefficient estimates for the full set of labor market outcomes.

We find no evidence of an effect of the state credit check bans on labor market outcomes. The trend and difference-in-differences plots show parallel trends for employment prior to ban implementation. While employment plummets between 2007 and 2010, employment for treatment and

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[24]We exclude individuals who filed seven or more years ago as their bankruptcy flags have already been removed and individuals who filed in three or fewer years to maintain a balanced sample, as we only have data on bankruptcy filers through 2009 but the majority of the state bans occurred since 2010 (see Appendix Table A1).

[25]However, for the years we can observe, including these states does not affect the results. These results are available upon request.

[26]We omit Hawaii, California, and Illinois from Appendix Figure A15 because these bans were implemented in 2009 and 2012 and would add an additional two lines to the plots. These states are included in all estimated results, however.

control states continue on parallel trends following the implementation of the bans. For Chapter 7 filers, there is a slight decrease in employment in treated states, although the effects are not statistically distinguishable from zero at most time horizons. For Chapter 13 filers, the point estimates are small and statistically insignificant at all time horizons. The patterns are very similar for the full set of employment and earnings measures shown in Appendix Table A6.[27]

While the state bans we analyze were implemented during the height of the Financial Crisis when counterfactual labor market outcomes were naturally harder to determine, we interpret these estimates as being broadly consistent with the results from our analysis of bankruptcy flag removal, bolstering the external validity of these findings.

In sum, we find economically small effects of flag removal on average, and economically small effects across different demographic groups, including minorities, for whom there have been particular concerns about the employment consequences of derogatory credit reports. We find economically small effects across the business cycle, including just after the Financial Crisis when labor markets were particularly tight. We find some evidence of modest positive effects on job-finding rates among non-employed individuals and reallocation towards the finance industry, although the effects are small in magnitude and the trend plots raise concerns about the parallel trends assumption. Finally, we find no evidence on employment effects with a separate difference-in-differences analysis of state-level bans on employer credit checks.

6   Interpretation

In this section, we consider two potential explanations for the zero labor market effect. The first is offsetting effects on labor supply and demand. The second is that bankruptcy flags are not an informative signal for employers, who already have substantial information on job applicants.

6.1   Labor Supply versus Labor Demand

In principle, a zero labor market effect might stem from countervailing effects on labor supply and demand. Since a flag removal increases access to credit, it might reduce labor supply through a credit smoothing channel, thereby offsetting any increase in employers' labor demand. The explanation of offsetting labor supply and labor demand effects is also rejected by basic price theory. If an outward shift in labor supply and inward shift in labor demand have counteracting effects of equilibrium quantity, then a supply and demand diagram shows that equilibrium wages must decrease. Since we find

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[27] Results are similar if we limit the analysis to Washington, the single state implementing a ban before the Financial Crisis.

no wage or earning effects, we can reject this explanation. The zero effect of state-level bans on employer credit checks provides further evidence against this explanation. Since these bans did not affect what information lenders can observe, and therefore did not have a direct effect on access to credit, they should isolate the effect of derogatory credit report information on employers' labor demand. The zero effect in this state-ban analysis therefore indicates that an offsetting labor supply effect is unlikely to explain our result.

In Appendix Section C, we further investigate the offsetting supply and demand theory by exploiting heterogeneity across individuals in the effect of flag removal on credit access. We show that there is a zero employment effect across individuals with different pre-flag removal credit card utilization rates, including individuals with relatively low credit card utilization. Since individuals with low pre-flag removal utilization were not credit constrained, this analysis isolates a group of individuals for whom changes in employers' labor demand should be the dominant force. In a second test, we show that the employment effect is also zero across individuals with different size increases in credit limits, including individuals with very small credit limit changes for whom the labor demand effect should again be dominant. These results provide additional evidence that a countervailing labor supply effect cannot explain the zero labor market effect.

6.2 Predictive Power of Hidden Bankruptcy Flags

A second potential explanation for the economically small labor market effects is that bankruptcy flags are not an informative signal for employers, who already have substantial information on job applicants. We investigate this theory by examining the predictive power of "hidden" bankruptcy flags – i.e., recently removed bankruptcy flags that are observed by the econometrician but unobserved by both lenders and employers. Specifically, we test the predictive value of hidden flags by comparing the outcomes of individuals who had a Chapter 13 flag removed in the prior year (and therefore have a hidden flag) to observably similar individuals who never declared bankruptcy (and therefore do not have a hidden flag). The logic behind this test is straightforward. If hidden flags are correlated with future job performance, then bankruptcy flags should be used when they are observed. If hidden flags have no predictive power, however, they should also have no causal effect on labor demand.

We begin by examining the predictive power of hidden flags for credit market outcomes as a benchmark for the labor market results. For these credit market results, we construct a sample of Chapter 13 bankruptcy filers in the first quarter after flag removal and randomly matched non-filers.

We then follow this sample over time to estimate the association between a hidden bankruptcy flag and the cumulative delinquency rate over the next three years, controlling for credit scores in the baseline period. Letting i denote individuals and t index quarters relative to the quarter of bankruptcy flag removal, we run regressions of the form:

$$\mathbf{1}(\text{Delinquency at Horizon } t)_i = \beta_t \cdot \mathbf{1}(\text{Hidden Chapter 13}_i) + \gamma X_i + f(\text{Credit Score}_{i,t=1}) + \varepsilon_i \quad (2)$$

where $\mathbf{1}(\text{Delinquency})$ is a cumulative indicator of whether the individual has been at least 30 days past due between the first quarter after flag removal and t, $\mathbf{1}(\text{Hidden Chapter 13}_i)$ is an indicator for having a hidden Chapter 13 flag, X_i is an indicator for the presence of delinquencies in the first period (i.e., $\mathbf{1}(\text{Delinquency}_{i,t=1})$), and $f(\text{Credit Score}_{i,t=1})$ is a fully non-parametric control in credit score in the first period after bankruptcy flag removal, interacted with time fixed effects. The β_t's are the coefficients of interest and measure the predictive power of a hidden bankruptcy flag on the delinquency probability at different time horizons.

Panels A and B of Figure 7 show the results from this analysis for credit card and mortgage debt, respectively. For non-filers, we plot the cumulative probability of delinquency. For bankruptcy filers, we plot the non-filer mean plus the relevant β_t from Equation (2). The estimates in Figure 7 show that hidden bankruptcy flags are strongly predictive of a delinquency. Table 7 presents the β_t for the credit results in the first two rows. At a three-year horizon, individuals with a hidden flag are 5.2 percentage points more likely to have a credit card delinquency, which is 27 percent higher than the non-filer mean of 19.3 percent and 33 percent of the non-filer standard deviation. For mortgage debt, individuals with a hidden flag are 11.1 percentage points more likely to have a delinquency, which is 94.4 percent of the non-filer mean of 11.8 percent and 100 percent of the non-filer standard deviation. These results indicate that hidden bankruptcy flags have substantial information content for credit market outcomes.

To examine the information content of bankruptcy flags in the labor market, we construct a sample of Chapter 13 filers who started a new job in the first full year after flag removal and randomly selected non-filers who started a job in the same year. We then compare employment outcomes over the next three years for individuals with and without a hidden bankruptcy flag, controlling for industry-by-wage decile effects at the initial employer, and for sex, race, and age effects. Letting i denote individuals and t denote *years* relative to the year of bankruptcy flag removal, we run regressions of

the form:

$$\mathbf{1}(\text{Same Job at Horizon } t)_i = \beta_t \cdot \mathbf{1}(\text{Hidden Chapter 13}_i) + \text{Wage Decile}_{i,t=1} \times \text{Industry}_{i,t=1} + \gamma X_i + \varepsilon_i,$$

$$(3)$$

where $\mathbf{1}(\text{Same Job}_{it})$ is an indicator for whether the individual remains at the same job, $\mathbf{1}(\text{Hidden Chapter 13}_i)$ is an indicator for having a hidden Chapter 13 flag, $\text{Wage Decile}_{i,t=1} \times \text{Industry}_{i,t=1}$ is a fully inter-acted set of controls for wage decile and industry in the first year after flag removal interacted with time fixed effects, and X_i includes controls for gender, race, and age. The β_t's are the coefficients of interest and capture the predictive power of hidden bankruptcy flags on the propensity to stay at the same job. We also estimate results for any employment and log wages (plus one) at both the same employer or any employer as alternative measures of job performance.

Panels C and D of Figure 7 shows results for the probability of being employed at the same job and the probability of being employed at any job, respectively. Following the credit market results, we plot the non-filer mean and the sum of the non-filer mean and the relevant β_t for each labor market outcome. In contrast to the credit market results, there is no persistent difference in the labor market outcomes of individuals with and without a hidden bankruptcy flag after we control for starting industry and wage decile. While there are statistically significant, although economically small, cor-relations at a one-year time horizon, we do not observe the type of persistent correlation that would result from hidden flags being a signal of productivity. At a three-year horizon, Table 7 shows that the difference in the probability of being at the same job is 0.3 percentage points on a non-filer mean of 35.2 percent and 1.4 percent of the non-filer standard deviation. For employment at any firm, the difference is 0.02 percentage points on a non-filer mean of 83.9 percent and 1.5 percent of the non-filer standard deviation. We also find a small and statistically insignificant effect on log wages (plus one) both at the same employer or any employer at the three-year horizon.

Thus, at least for these outcomes, bankruptcy flags seem to have little predictive power for future job performance, rationalizing the small labor market effects documented above.

6.3 Why Do Employers Buy Credit Reports?

If credit reports do not matter for predicting employment outcomes, why do employers purchase this information? There are a number of potential explanations. Perhaps the most straightforward expla-nation is that employers purchase credit reports to verify information only *after* a job offer has been

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made and, as a result, this credit information is only used in the context of other potentially ameliorating data points. Among employers running credit checks, only 2 percent report running a credit check during the initial screening, while 40 percent run a credit check in between the initial screening and the job offer and 58 percent run a credit check after the job offer has been made. Moreover, 80 percent of firms report hiring an individual with negative credit information (SHRM, 2010).

A second explanation is that the firms purchase this information is to reduce legal liability and to comply with state-level legal requirements, not screen job applicants. According to the SHRM (2010) survey, 27 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 to comply with state laws requiring background checks for certain professions (e.g., day care teachers, licensed medical practitioners).[28]

7 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 economically small effects of improved credit reports on employment and earning outcomes, with confidence intervals that are substantially smaller than evidence on the link between credit and labor markets in other contexts.

We rationalize these contrasting results by showing that, conditional on basic observables, "hidden" bankruptcy flags are strongly correlated with adverse credit market outcomes but have little predictive power for labor market 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 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, job transitions are infrequent occurrences,

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[28]A third explanation is that credit report data is cheap, employers hold the prior belief that "more information is better", and that most employers have not conducted the type of systematic analysis needed to overturn their incorrect prior. Employer credit checks cost about $10 to $15 when purchased separately, and credit check information is often bundled with other background check information which might be valuable. For employers to be able to rule out a performance effect that would justify an expenditure of $10 to $15, the employer would need to see ex-post outcomes for a very large sample of otherwise identical employees, a challenge that would be difficult to surmount without a large amount of high-quality data.

so any effects of creditworthiness may not be reflected over the three-year time horizon we currently examine. Second, the small 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. Finally, the effects of other changes in the creditworthiness, such as the removal of a foreclosure flag or delinquent debt flag, may be different than the effects examined here. Estimating the effects of other improvements in creditworthiness is an important area of future work.

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Figure 1: Economic Interpretation of Credit Score

(A) Histogram

(B) Probability of Default by Credit Score
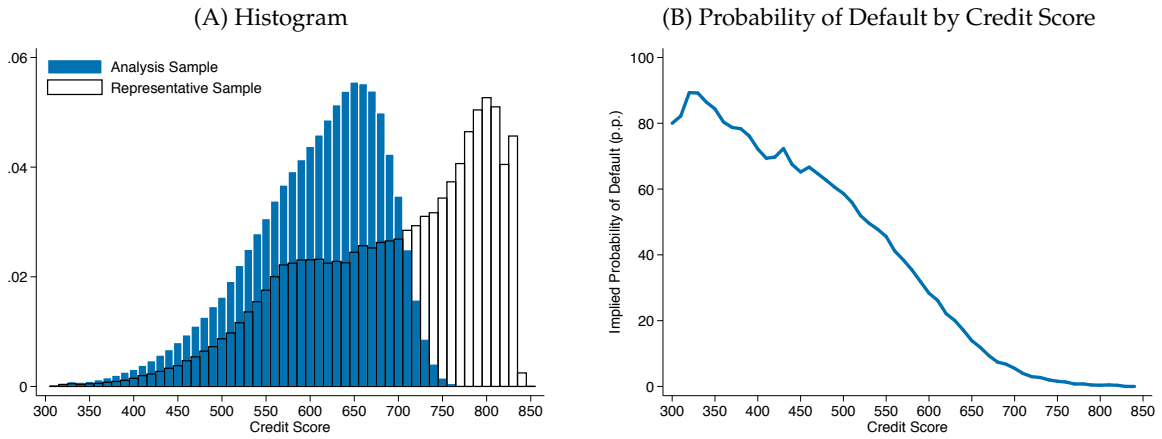
Note: Panel A of this figure plots the distribution of credit scores for a representative sample of the FRBNY Consumer Credit Panel / Equifax data in 2005q1 and the credit scores for our analysis sample of bankruptcy filers 6 years after filing. Panel B of this figure uses the representative sample from Panel A and plots the observed fraction of individuals with a tradeline that is at least 90 days past due (90+ DPD) in 2007q1 against the observed credit score in 2005q1.

Figure 2: Credit Score Trends

(A) Credit Score

(B) Implied Probability of Default

Note: This figure plots average credit scores and the implied probability of default 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 3: Credit Card Trends

(A) Credit Card Limits	(B) Credit Card Balances

Note: This figure plots the average credit card outcomes 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 4: Mortgage Trends

(A) Mortgage Loan

(B) Log(Mortgage Balances + 1)

Note: This figure plots the average mortgage outcomes 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure 5: Labor Market Trends
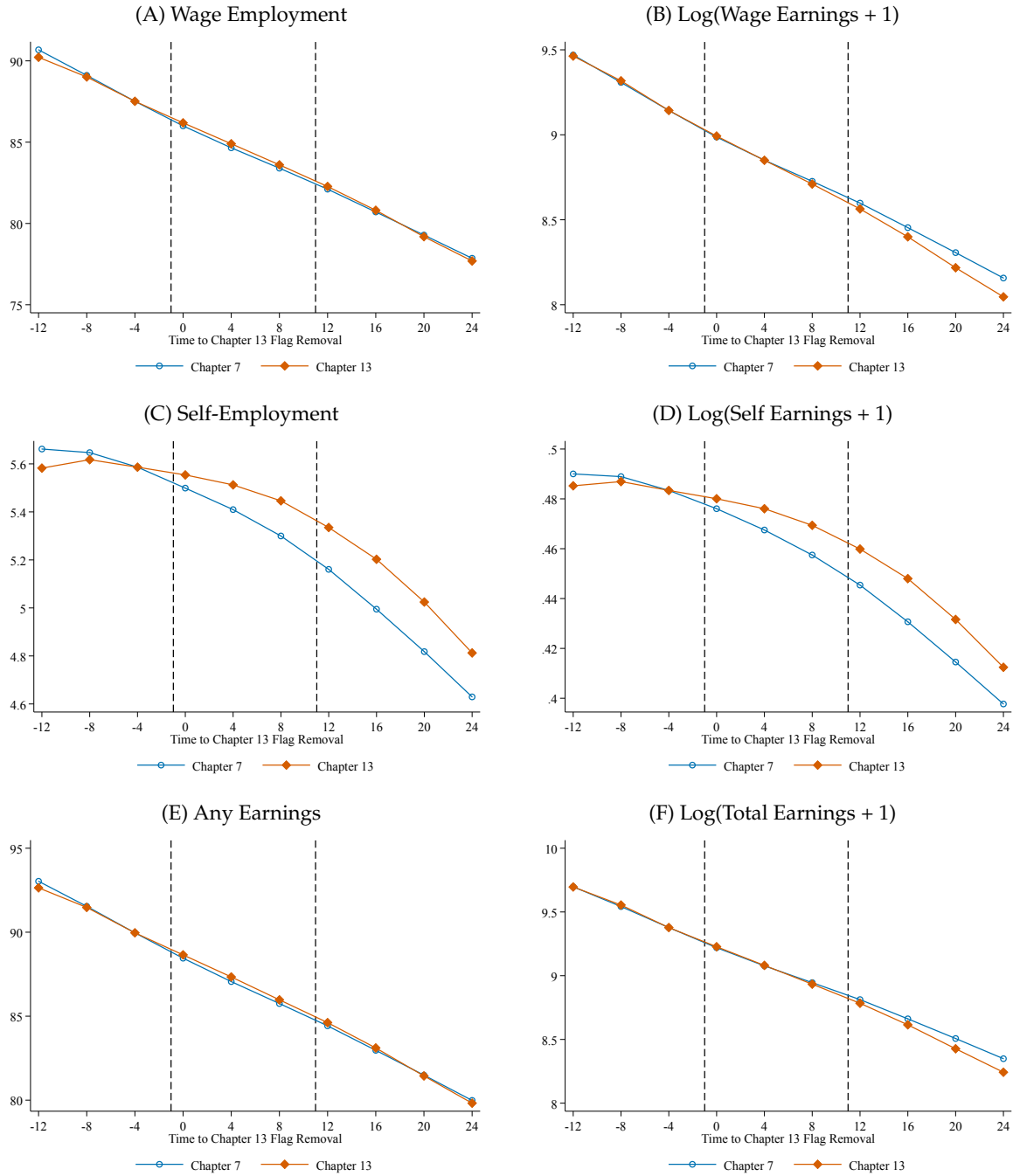
Note: This figure plots the average labor market outcomes for Chapter 13 and Chapter 7 bankruptcy filers. The horizontal axis denotes time, in quarters, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the year of Chapter 13 flag removal 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

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Figure 6: Employment Results by Gender × Race × Age × State
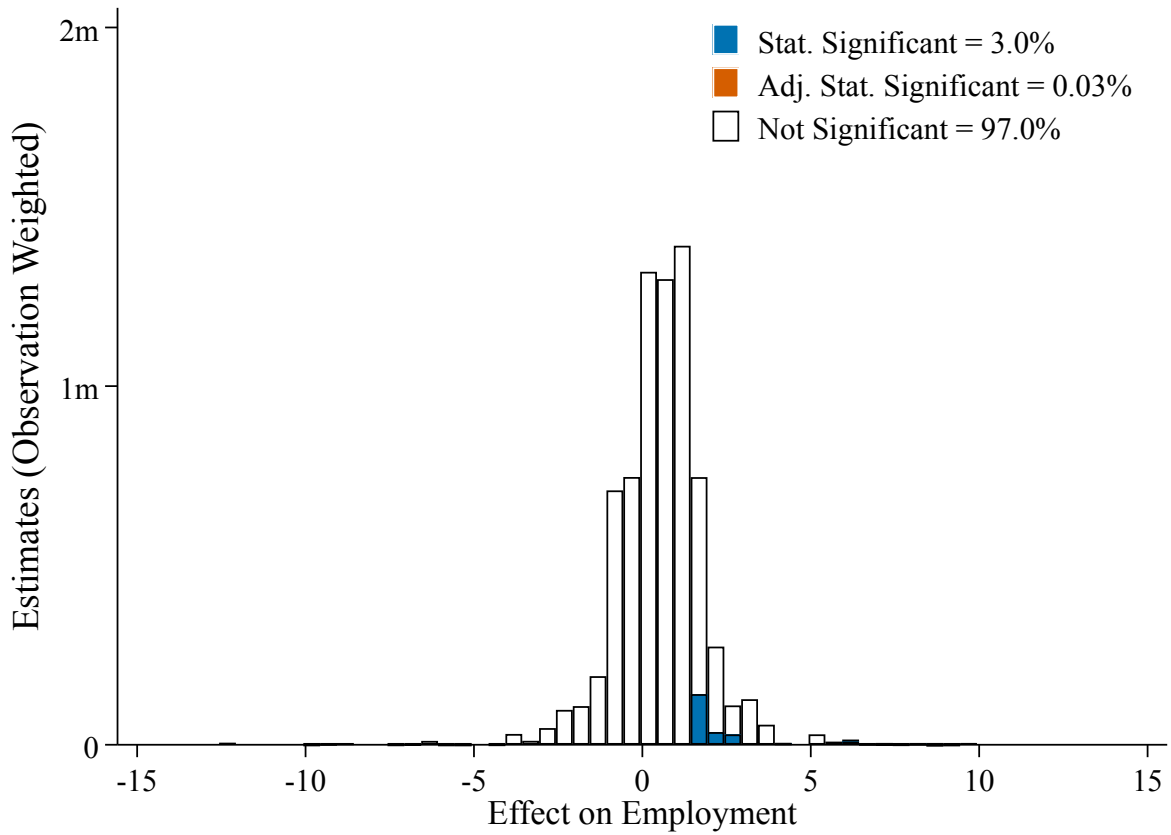
Note: This figure plots the distribution of difference-in-differences estimates for employment at three years at the gender × race × age × state level. We weight each estimate by the number of observations. We also report the fraction of estimates that are statistically significant at the 1 percent level 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. See Appendix Table A12 for a list of the coefficients.

Figure 7: *Ex Post* Informativeness of Removed Bankruptcy Flags

(A) Cumulative Credit Card Delinquency

(B) Cumulative Mortgage Delinquency

(C) Probability of Same Job

(D) Probability of Employment
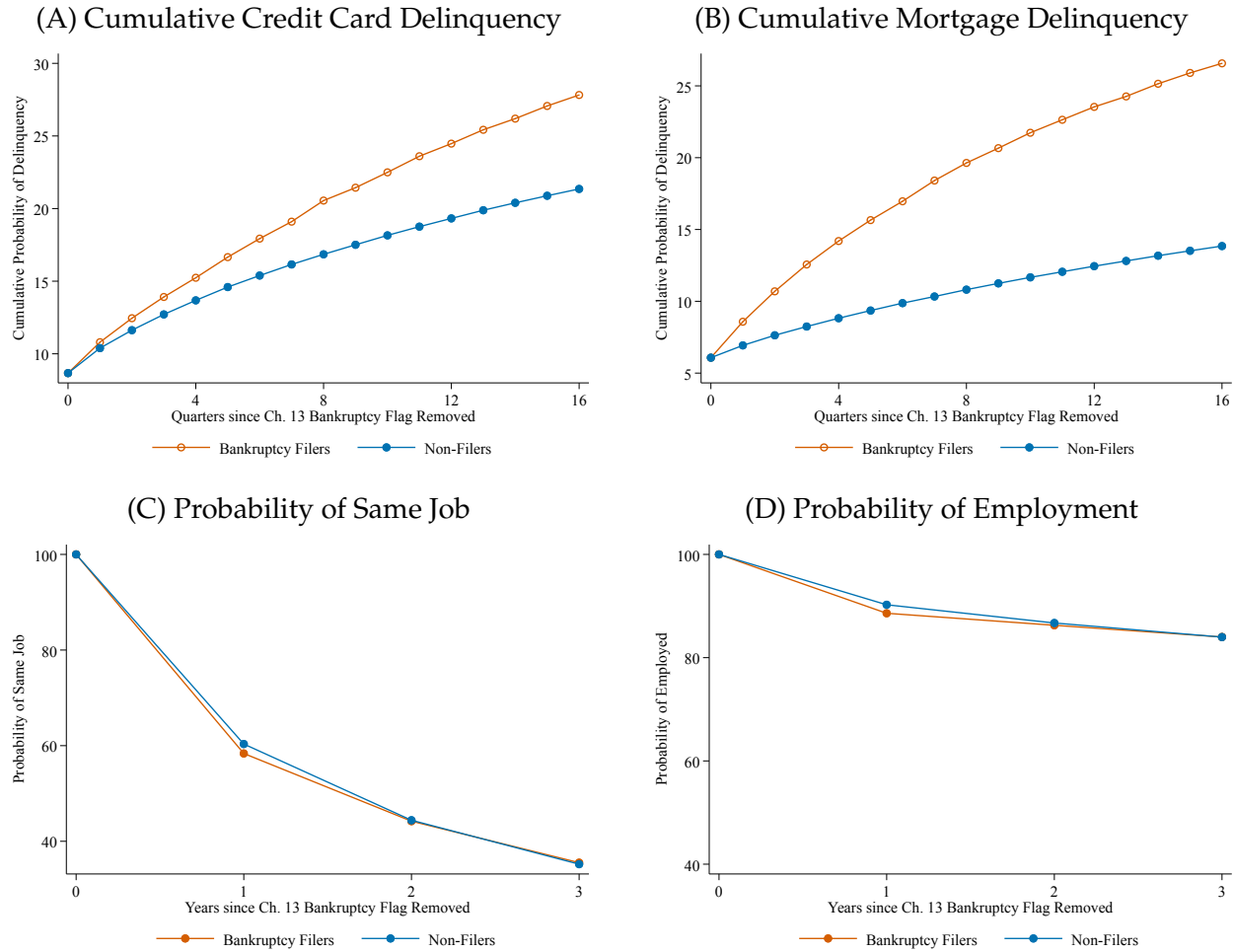
Note: These figures plot outcomes for Chapter 13 filers following the removal of their bankruptcy flag, and use non-filers as a control group for these filers. Observations from states that enacted a ban on using credit reports in employment hiring are included in the figure. Figure A reports the cumulative probability of having a credit card delinquency in the next 16 quarters following the bankruptcy flag removal. These probabilities are estimated by randomly matching a sample of non-filers to each bankruptcy flag removal year-quarter cohort, and non-parametrically controlling for the flag removal year-quarter interacted with credit score after flag removal. Non-filer probabilities are reported as the mean for the non-filer group over time. Bankruptcy filer probabilities are reported as the mean for the non-filer group plus the estimated coefficient from the previously described regression. Figure B reports the same analysis on mortgage delinquencies. Figure C uses a sample of Chapter 13 bankruptcy filers and randomly selected non-filers who both start new jobs the first full year after bankruptcy flag removal. The figure reports the estimated probability of remaining with the same employer, controlling for bankruptcy-cohort-year fixed effects interacted with industry-by-wage-decile fixed effects. The regressions also control for sex, race and age. The non-filer probabilities are reported as the mean for the non-filer group over time, while the bankruptcy filer probabilities are reported as the mean for the non-filer group plus the estimated coefficient from the previously described regression. Figure D reports the same analysis on the overall probability of employment.

Table 1: Summary Statistics

	Chapter 13 Filers		Chapter 7 Filers		Pooled	
	Mean	SD	Mean	SD	Mean	SD
Credit Report Data:	(1)	(2)	(3)	(4)	(5)	(6)
Credit Score	595.23	40.47	599.68	20.89	599.22	23.76
Implied Probability of Default (%)	32.13	11.51	30.55	6.60	30.72	7.29
Credit Card Limits	3,353.19	3,292.13	4,416.81	1,878.10	4,305.01	2,098.14
Credit Card Balance	2,043.43	2,069.28	2,719.85	1,432.23	2,648.74	1,526.04
Mortgage Loan (%)	41.35	21.94	33.07	10.07	33.94	12.15
Log(Mortgage Balance + 1)	4.65	2.51	3.72	1.16	3.82	1.39
Labor Market Data:						
Wage Employment	86.30	28.73	80.91	32.80	81.51	32.41
Self-Employment	5.52	17.94	6.47	19.36	6.36	19.20
Any Employment	88.72	25.92	83.87	30.38	84.41	29.96
Wages Earnings	38,641.20	30,053.77	32,965.96	31,309.00	33,599.04	31,222.64
Self-Employment Earnings	610.84	3,517.80	698.21	3,432.59	688.47	3,442.31
Total Earnings	37,935.87	24,018.73	32,582.01	24,117.14	33,179.24	24,165.04
Log(Wages + 1)	9.01	3.18	8.31	3.59	8.39	3.55
Log(Self-Employment Earnings + 1)	0.48	1.60	0.56	1.73	0.55	1.72
Log(Earnings + 1)	9.24	2.89	8.60	3.34	8.67	3.29

Note: This table reports summary statistics. The Chapter 13 sample includes individuals who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. 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. This quarterly CCP data sample includes 249,654 unique Chapter 7 filers and 29,328 unique Chapter 13 filers for a total of 278,982 unique filers. The SSA sample includes 3,132,593 unique Chapter 7 filers and 393,321 unique Chapter 13 filers for a total of 3,525,914 filers. Credit Score is the Equifax Risk Score 3.0 measure, described in the text. Implied Probability of Default is the estimated probability of having a 90+ day delinquent loan two years later. Credit Card Limits and Credit Card Balance are calculated as the sum of bank card and retail card lines of credit. Mortgage Loan is an indicator for having a mortgage loan. Employment is an indicator for non-zero wage earnings, self-employment is an indicator for non-zero self-employment earnings, and any employment is an indicator for non-zero wage or self-employment earnings. All indicator measures are scaled by 100 to calculate percentages.

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

	Mean at $t = -1$	Difference-in-Differences Estimates			
		1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
Credit Score	594.07	9.43	5.50	2.19	5.71
	(36.80)	(0.36)	(0.53)	(0.66)	(0.45)
		[0.00]	[0.00]	[0.00]	[0.00]
Implied Probability of Default	32.34	−2.58	−1.35	−0.29	−1.41
	(10.81)	(0.11)	(0.16)	(0.19)	(0.13)
		[0.00]	[0.00]	[0.13]	[0.00]

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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 278,982 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. 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-Differences Estimates			
	$t = -1$	1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
Credit Card Limits	2,784.10	336.25	957.29	1,328.27	873.51
	(2,382.75)	(27.51)	(63.72)	(85.83)	(56.16)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Balance	1,819.01	126.38	528.20	780.93	478.25
	(1,639.17)	(19.88)	(39.00)	(52.89)	(33.97)
		[0.00]	[0.00]	[0.00]	[0.00]

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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 278,982 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. 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-Differences Estimates | | | |
| | $t = -1$ | 1 Year | 2 Years | 3 Years | Pooled |
	(1)	(2)	(3)	(4)	(5)
Had Mortgage Loan	41.01	1.99	1.96	1.26	1.74
	(21.86)	(0.26)	(0.31)	(0.35)	(0.28)
		[0.00]	[0.00]	[0.00]	[0.00]
Log(Mortgage Balance + 1)	4.61	0.23	0.23	0.14	0.20
	(2.50)	(0.03)	(0.04)	(0.04)	(0.03)
		[0.00]	[0.00]	[0.00]	[0.00]

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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 278,982 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. 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 $t = -1$	Difference-in-Difference Estimates			
		1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
Wage Employment	87.51	0.17	0.23	0.19	0.20
	(30.68)	(0.11)	(0.12)	(0.11)	(0.10)
		[1.00]	[1.00]	[1.00]	[1.00]
Self-Employment	5.59	0.03	0.07	0.10	0.07
	(9.22)	(0.05)	(0.05)	(0.05)	(0.04)
		[1.00]	[1.00]	[1.00]	[1.00]
Any Employment	89.96	0.17	0.24	0.17	0.19
	(25.34)	(0.12)	(0.12)	(0.11)	(0.10)
		[1.00]	[1.00]	[1.00]	[1.00]
Log(Wages + 1)	9.14	0.00	−0.00	−0.02	−0.01
	(3.28)	(0.01)	(0.01)	(0.01)	(0.01)
		[1.00]	[1.00]	[1.00]	[1.00]
Log(Self-Employment Earnings + 1)	0.48	0.00	0.01	0.01	0.00
	(0.86)	(0.00)	(0.00)	(0.00)	(0.00)
		[1.00]	[1.00]	[1.00]	[1.00]
Log(Earnings + 1)	9.38	0.00	−0.00	−0.02	−0.00
	(2.77)	(0.01)	(0.01)	(0.01)	(0.01)
		[1.00]	[1.00]	[1.00]	[1.00]

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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 3,525,914 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. 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	Pooled
	(1)	(2)	(3)	(4)	(5)
White Males 30-39	91.10	0.29	0.68	0.80	0.59
	(25.57)	(0.15)	(0.16)	(0.18)	(0.14)
		[1.00]	[0.00]	[0.00]	[0.00]
Non-White Males 30-39	90.51	0.30	0.29	0.27	0.28
	(27.23)	(0.19)	(0.22)	(0.24)	(0.18)
		[1.00]	[1.00]	[1.00]	[1.00]
White Males 40-54	84.60	0.18	0.41	0.37	0.32
	(33.51)	(0.14)	(0.16)	(0.15)	(0.13)
		[1.00]	[0.51]	[0.55]	[0.55]
Non-White Males 40-54	85.83	0.23	0.18	0.25	0.22
	(32.72)	(0.15)	(0.19)	(0.20)	(0.16)
		[1.00]	[1.00]	[1.00]	[1.00]
White Females 30-39	87.77	0.36	0.08	0.24	0.23
	(29.83)	(0.25)	(0.28)	(0.31)	(0.25)
		[1.00]	[1.00]	[1.00]	[1.00]
Non-White Females 30-39	93.39	0.20	0.17	0.12	0.16
	(22.93)	(0.20)	(0.20)	(0.23)	(0.17)
		[1.00]	[1.00]	[1.00]	[1.00]
White Females 40-54	85.40	0.09	0.14	0.24	0.16
	(32.95)	(0.16)	(0.19)	(0.20)	(0.16)
		[1.00]	[1.00]	[1.00]	[1.00]
Non-White Females 40-54	90.44	0.22	0.06	−0.15	0.04
	(27.54)	(0.15)	(0.17)	(0.19)	(0.15)
		[1.00]	[1.00]	[1.00]	[1.00]

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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 3,525,914 unique individuals.Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. 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: *Ex Post* Informativeness of Removed Bankruptcy Flags

	Difference-in-Difference Estimates			
	1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(5)
Pr(Credit Card Delinquency)	1.56	3.70	5.15	3.70
	(0.25)	(0.31)	(0.42)	(0.31)
	[0.00]	[0.00]	[0.00]	[0.00]
Pr(Mortgage Delinquency)	5.37	8.82	11.09	8.82
	(0.19)	(0.24)	(0.36)	(0.24)
	[0.00]	[0.00]	[0.00]	[0.00]
Pr(Same Job)	−1.98	−0.20	0.32	−0.62
	(0.27)	(0.29)	(0.29)	(0.25)
	[0.00]	[1.00]	[1.00]	[0.10]
Pr(Employed)	−1.65	−0.44	0.02	−0.69
	(0.13)	(0.17)	(0.19)	(0.13)
	[0.00]	[0.07]	[1.00]	[0.00]
Log(Wage+1) Cond. Same Job	−20.26	−4.26	−0.40	−8.30
	(2.74)	(3.04)	(3.06)	(2.59)
	[0.00]	[1.00]	[1.00]	[0.02]
Log(Wage+1)	−12.82	−1.39	1.53	−4.23
	(1.32)	(1.67)	(1.91)	(1.37)
	[0.00]	[1.00]	[1.00]	[0.02]

Note: The table presents estimates of the effect of a removed Chapter 13 bankruptcy flag compared to a sample of non-filers. The sample is constructed by matching the sample of Chapter 13 bankruptcy filers in the first quarter after flag removal and a random sample of non-filers. Observations from states that enacted a ban on using credit reports in employment hiring are included. We then follow this sample over time to estimate the association between a hidden bankruptcy flag. For credit outcomes, we control non-parametrically for credit scores from the first quarter after flag removal, interacted with time fixed effects. For labor outcomes, we control for a fully interacted set of controls wage decile and industry in the first year after flag removal, as well as gender, race, and age. Column 1 reports the difference between the two groups after 4 quarters, Column 2 after 8 quarters, and Column 3 after 12 quarters. The regressions with credit outcomes include a panel of 1,001,846 unique individuals. The regressions with labor outcomes include a panel of 1,253,959 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise *p*-values are in brackets. 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

A Heterogeneity in Effect on Credit Scores

In this section, we examine heterogeneity by predicted pre-flag removal credit score.[29] In particular, Appendix Figure A16 plots average credit scores for Chapter 13 filers and Chapter 7 filers for different quartiles of the predicted pre-flag removal credit score, and Appendix Table A2 shows estimates from our corresponding parametric specification that pools effects by year. The degree of reversion to pre-removal credit scores is decreasing sharply in predicted credit score. For the lowest predicted credit score quartile, average credit scores for the treated Chapter 13 filers drop below the average credit scores for the Chapter 7 control group two to three years post-removal. Conversely, for the highest predicted credit score quartile, 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.

For completeness, Appendix Figure A17 and Appendix Table A2 show the analogous plots and parameter estimates with the implied probability of default as the dependent variable. Other than the inverse relationship, the patterns are qualitatively very similar.

B Event Study Estimates of Chapter 13 Flag Removal

In our baseline difference-in-differences specification, we construct counterfactual outcomes for Chapter 13 filers (the "treatment" group) using outcomes for Chapter 7 filers (the "control" group). In this section, we present results from event-study specifications, where the counterfactual is based on a linear trend estimated using the outcomes of Chapter 13 filers in pre-flag removal period. Letting i indicate filing groups, s indicate calendar-time, and t indicate event-time, our event-study regression specification takes the form:

$$y_{it} = \alpha_i + \alpha_{s(i,t)} + \delta \cdot t + \sum_{t=0}^{t=8} \beta_t + \varepsilon_{it},$$

where α_i are filing group fixed effects, $\alpha_{s(i,t)}$ are calendar-time fixed effects, δ captures the pre-flag removal linear trend, and β_t are the key coefficients of interest, showing the change in the outcome

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[29]In our setting, segmenting the data by actual pre-flag removal credit score is problematic because it is difficult to separate changes in credit scores due to mean reversion (i.e., low credit scores rising and high credit scores falling) and due to heterogeneity in the causal effect of flag removals. For example, mean reversion occurs in our setting when an individual accidentally misses a payment, has their score lowered, and then catches up on the overdue payment, and has their score immediately recover. In this scenario, we would mistakenly attribute the rise in credit scores to a larger treatment effect. To overcome this issue, we split the data on stable characteristics that are predictive of credit scores but do not suffer from mean reversion. Specifically, we segment the data by first running a linear regression of pre-flag removal credit score on fully interacted fixed effects for age group and state of residence, and then bin the data based on the predicted credit scores from this regression.

relative to the pre-existing linear trend. We drop Chapter 7 filers because they do not play any role in the analysis. The identifying assumption is that, in the absence of bankruptcy flag removal, outcomes would have proceeded on their pre-existing linear trend, δt.

Figure A18 shows trend plots for credit scores and the implied probability of default, with the linear counterfactual from the event-study specification added to both figures. Table A7 shows the corresponding parameter estimates at one, two, and three years after flag removal, along with an estimate that combines the effect across the three years. At one year, the event study estimates are roughly similar to the baseline difference-in-differences estimates shown in Table 2. At longer time horizons, however, the linear trends indicate negative effects on credit scores (and positive effects on the implied probability of default), while the baseline difference-in-differences results indicate positive effects on credit scores (and negative effects on the implied probability of default). The event study estimates yield the incorrect sign in large part because these estimates do not account for the reduction in slope for Chapter 7 outcomes discussed in footnote 19. In contrast, our baseline difference-in-differences estimates account for this slope change by using Chapter 7 filers to construct counterfactual outcomes.

Figures A19, A20, A21, A22, and A23 show trend plots for the other main outcomes. Tables A8, A9, A10, and A11 show the corresponding parameter estimates. The event study design produces a counterfactual that is very similar to that from the difference-in-differences specification for some outcomes (e.g., Panel A of A19). In these case, both approaches yield comparable results. In other cases, the counterfactuals, and thus the parameter estimates, are very different (e.g., Panel B of A20).

C   Additional Evidence on Labor Supply versus Labor Demand

We further investigate the potential for offsetting labor supply and demand effects by exploiting heterogeneity across individuals in the effect of flag removal on credit access. The first form of heterogeneity we use is based on pre-flag removal credit card utilization. It is plausible that individuals with higher pre-flag removal credit card utilization are relatively more credit constrained, and should therefore experience larger increases in credit access and larger decreases in labor supply compared to individuals with lower pre-flag removal credit card utilization. Thus, if our zero labor market effect is explained by a reduction of labor supply, the employment effect should be downward sloping in pre-flag removal credit limits, with larger employment effects for individuals who had low pre-flag removal utilization (and were never credit constrained), and smaller employment effects for individuals who had high levels of pre-flag removal utilization (and were most likely to have reduced their labor supply in response to flag removal).

Panel A of Appendix Figure A24 shows results from this heterogeneity analysis. The plot is constructed by estimating the effect of flag removal for groups of individuals defined by the full interaction of state of residence and five-year age bins. We then show a binned scatter plot of the employment effect against the average pre-flag removal utilization rate for each group. The best fit line is based on the underlying group-level data, with each group weighted by the number of underlying observations and standard errors clustered at the state level. There is substantial heterogeneity in pre-flag removal credit card utilization, with rates varying from 65 percent to more than 85 percent across the binned groups. However, the employment response is completely homogeneous across these groups,

with the slope of the best fit line statistically indistinguishable from zero.

The second form of heterogeneity we use arises from variation in the size of the effect of flag removal on credit limits. Individuals who receive larger increases in credit limits experience a larger increase in credit access, and should therefore reduce their labor supply more than individuals who receive smaller increases in credit limits. If our zero labor market effects are explained by a reduction of labor supply, the employment effect should be negatively correlated with the credit limit effect, with larger employment effects for individuals who had small credit limit effects (and thus were mainly exposed to an increase in employers' labor demand) and smaller employment effects for individuals who had larger credit limit effects.

Panel B of Appendix Figure A24 plots the employment effect against the effect on credit limits, constructed in an analogous manner to Panel A. While there is substantial variation in the change in credit limits across groups, with the size of the effect varying by more than $6,000, there is no evidence that the employment effect is negatively correlated with the credit limit effect, with the slope of the best fit line statistically indistinguishable from zero. Taken together, this evidence suggests that an offsetting labor supply effect cannot explain the zero effect for labor market outcomes, and provides evidence that these estimates should be interpreted as a zero average effect of flag removal employers' labor demand.[30]

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[30]This analysis presumes that the labor market effect is relatively constant across the changes in access to credit. If the effect on labor demand is correlated with the effect on credit access, the results could still be consistent with offsetting labor supply and demand effects.

Figure A1: Bankruptcy Flag Removal Timing
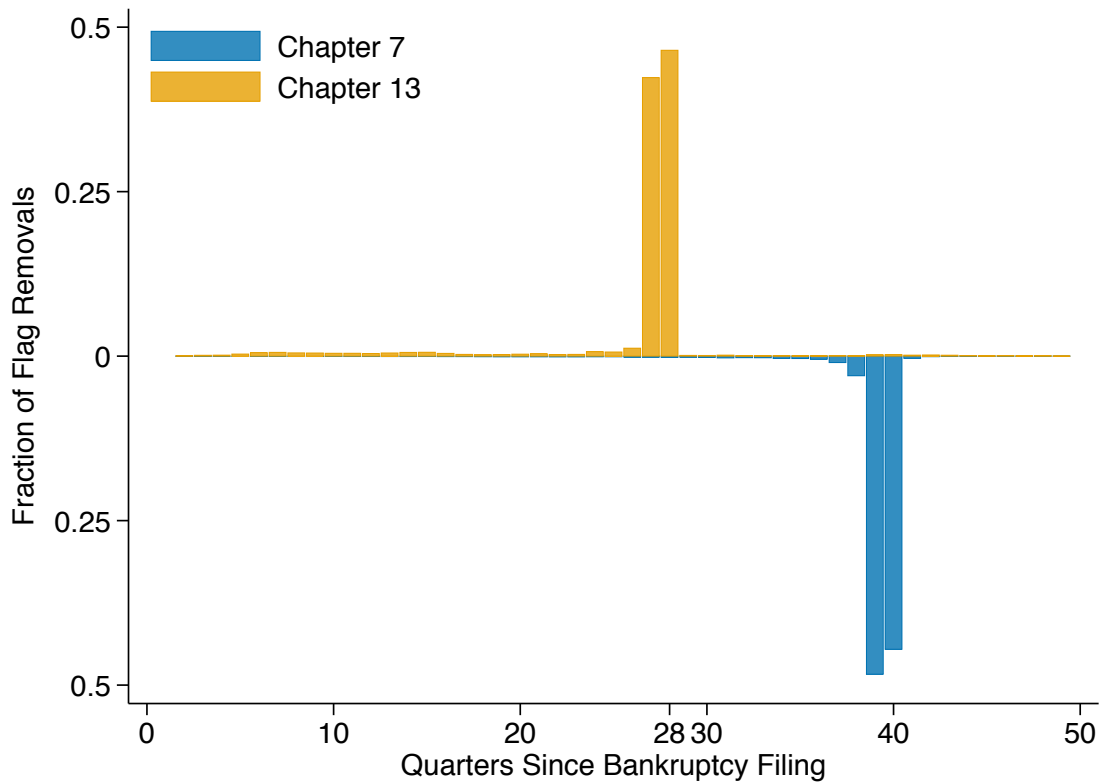
Note: This figure shows the length of time which a bankruptcy flag remains on the credit report data. For each bankruptcy filer in our credit report dataset, we identify when the flag was added to the credit report, and then track the number of quarters until flag removal. The New York Fed CCP data begins in 1999q1, and so this data will reflect filings from 2006 and onwards.

Figure A2: Effect of Chapter 13 Flag Removal on Credit Scores
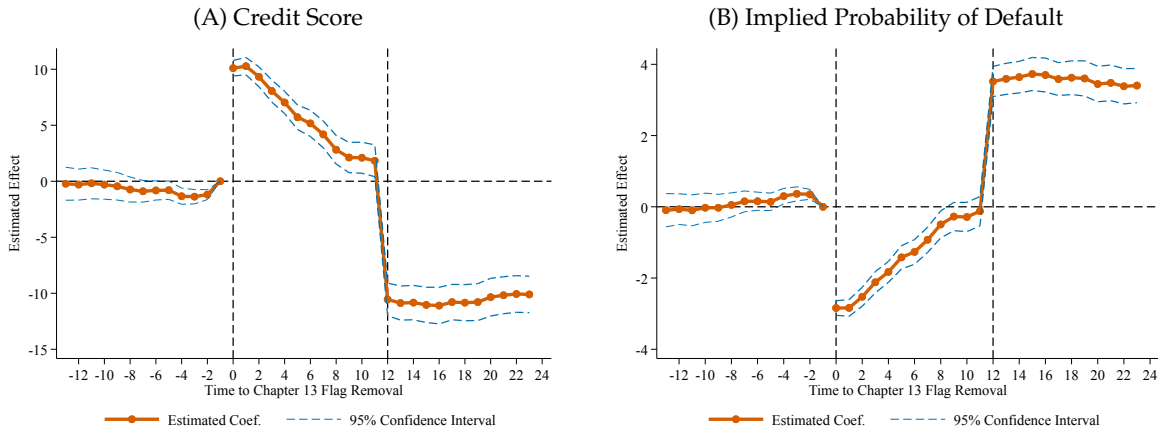
(A) Credit Score

(B) Implied Probability of Default

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 of 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A3: Effect of Chapter 13 Flag Removal on Credit Card Outcomes
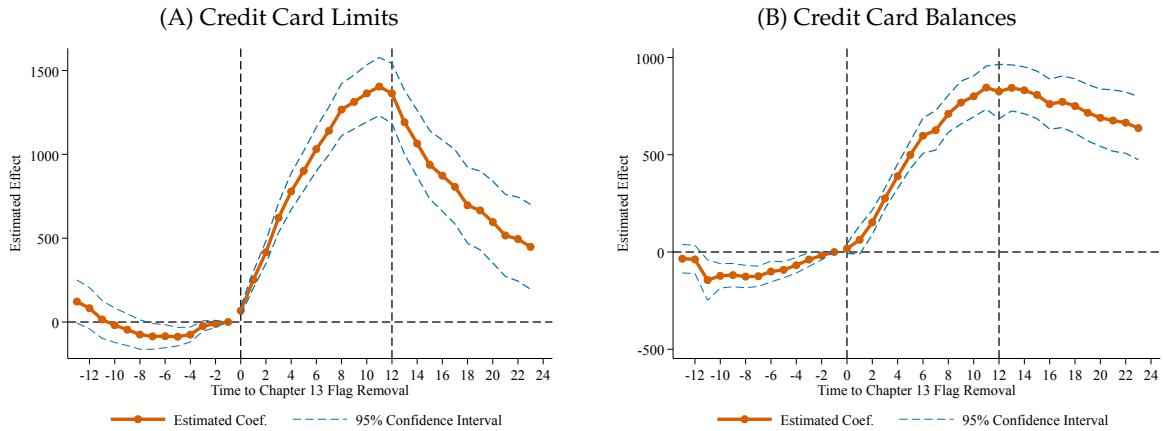
(A) Credit Card Limits

(B) Credit Card Balances

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 of 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. 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: Credit Card Limits by Pre-Flag Removal Credit Score

(A) Bottom Quartile

(B) 2nd Quartile

(C) Third Quartile

(D) Top Quartile
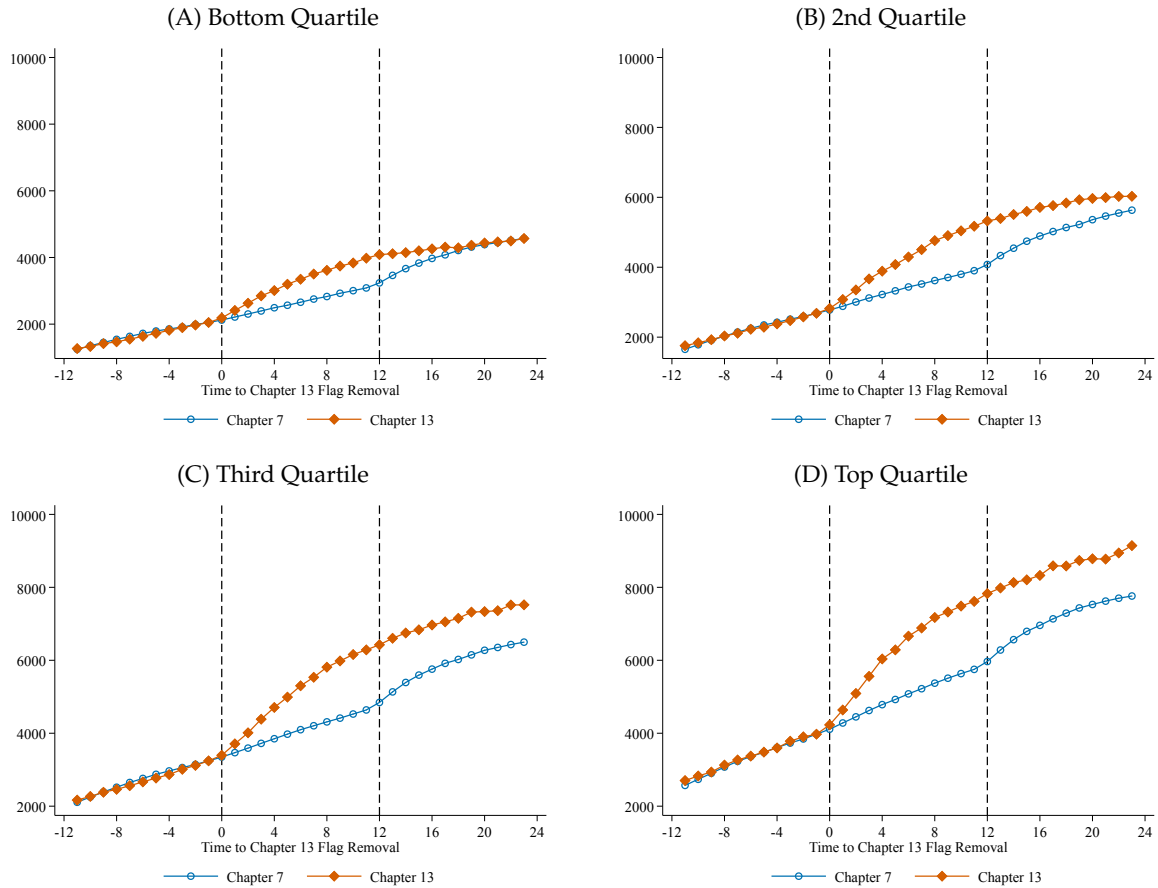
Note: This figure plots average credit card limits for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. In order to split the sample into quartiles, we construct predicted pre-removal credit scores from a linear regression of observed pre-flag removal credit score 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A5: Credit Card Balance by Pre-Flag Removal Credit Score

(A) Bottom Quartile

(B) 2nd Quartile

(C) Third Quartile

(D) Top Quartile
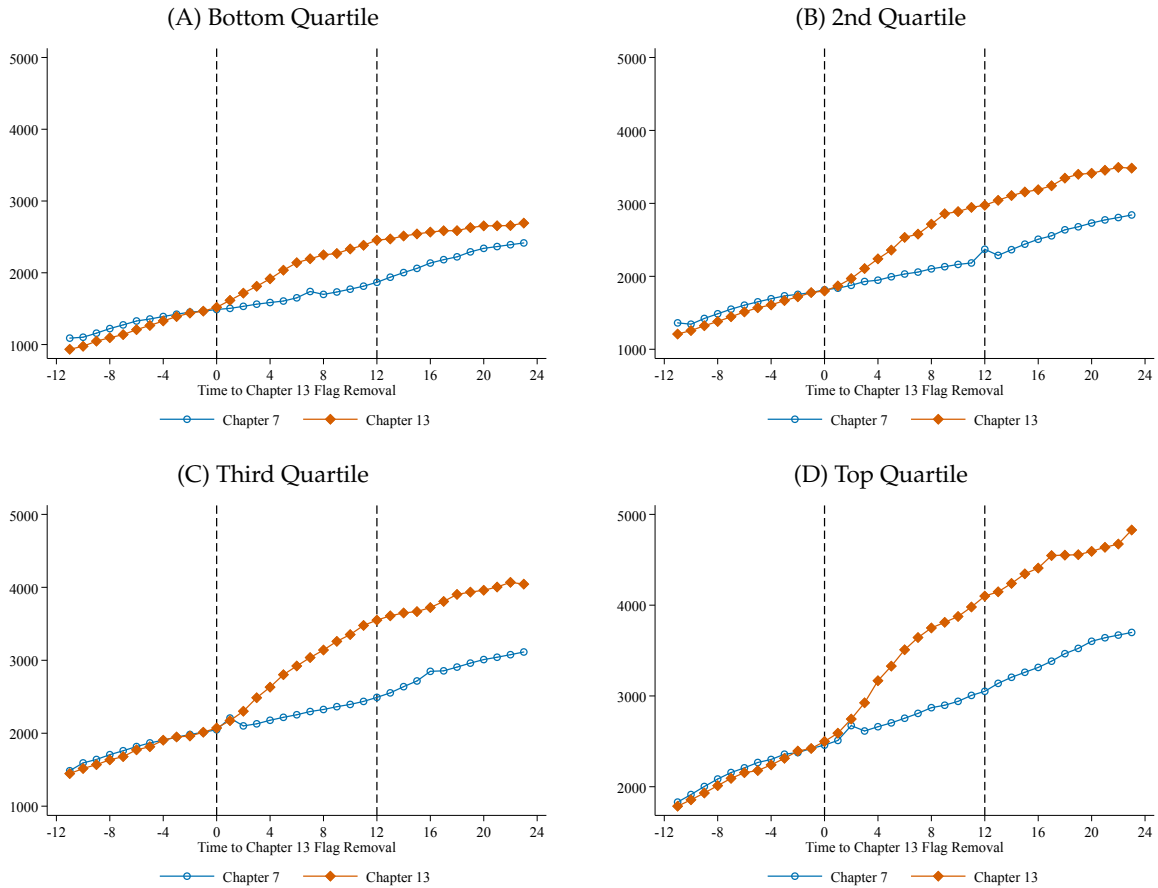
Note: This figure plots average credit card balances for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. In order to split the sample into quartiles, we construct predicted pre-removal credit scores from a linear regression of observed pre-flag removal credit score 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

**Figure A6:** Effect of Chapter 13 Flag Removal on Mortgage Outcomes
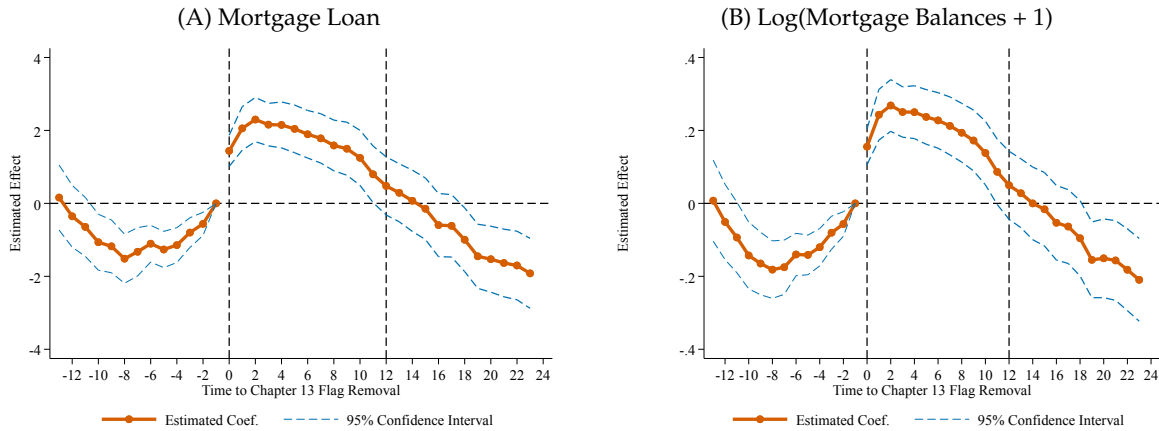
Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on mortgage outcomes. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical lines show the quarter of Chapter 13 flag removal and the quarter of 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. 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: Dose-Response
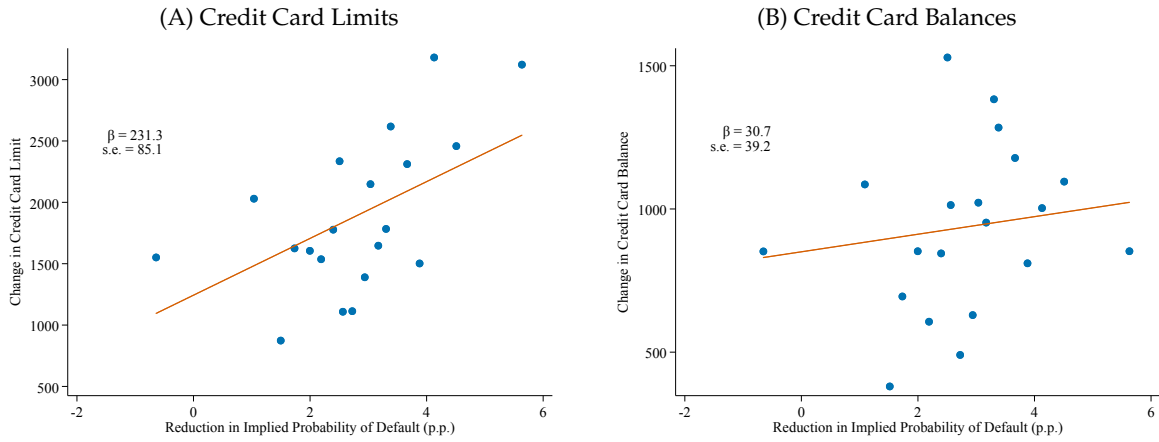
(A) Credit Card Limits

β = 231.3
s.e. = 85.1

(B) Credit Card Balances

β = 30.7
s.e. = 39.2

Note: This figure shows binned scatter plots of the estimated coefficients from difference-in-differences regressions conducted at the age group-by-state group level. Panel A plots the dollar increase in credit card limits against the estimated reduction in the implied probability of default. Panel B plots the dollar increase in credit card balances against the estimated reduction in the implied probability of default. The solid line shows the best linear fit estimated on the underlying age-by-state level data weighted by the number of observations within each group. The coefficients show the estimated slope of the best-fit line, with standard errors clustered at the state level reported below. The effects on employment and credit limits are at one year after flag removal. See the text for additional details on the specification.

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Figure A8: Mortgage Loan by Pre-Flag Removal Credit Score
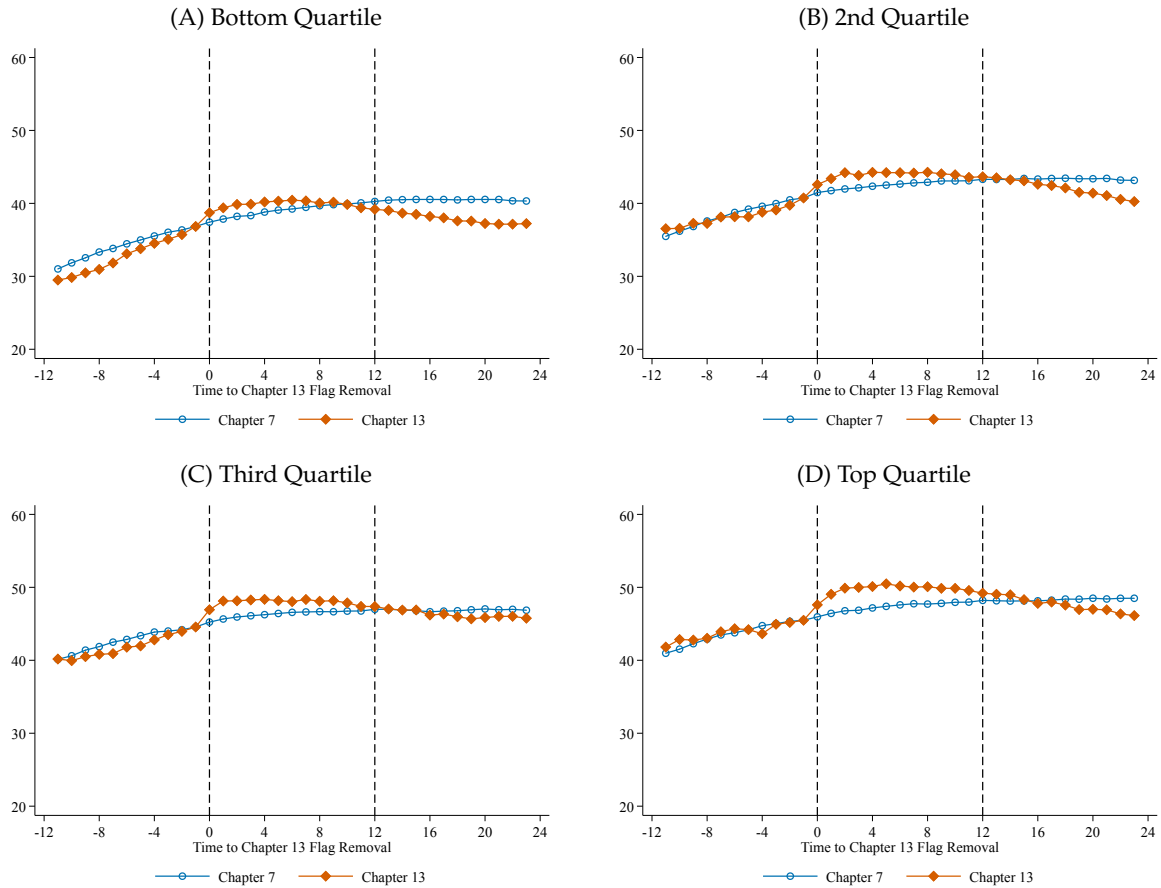
Note: This figure plots average fraction of individuals with a mortgage for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. In order to split the sample into quartiles, we construct predicted pre-removal credit scores from a linear regression of observed pre-flag 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A9: Net Change in Fraction with Mortgage

(A) All Years

(B) Early (2002-2007)

(C) Late (2008-2011)
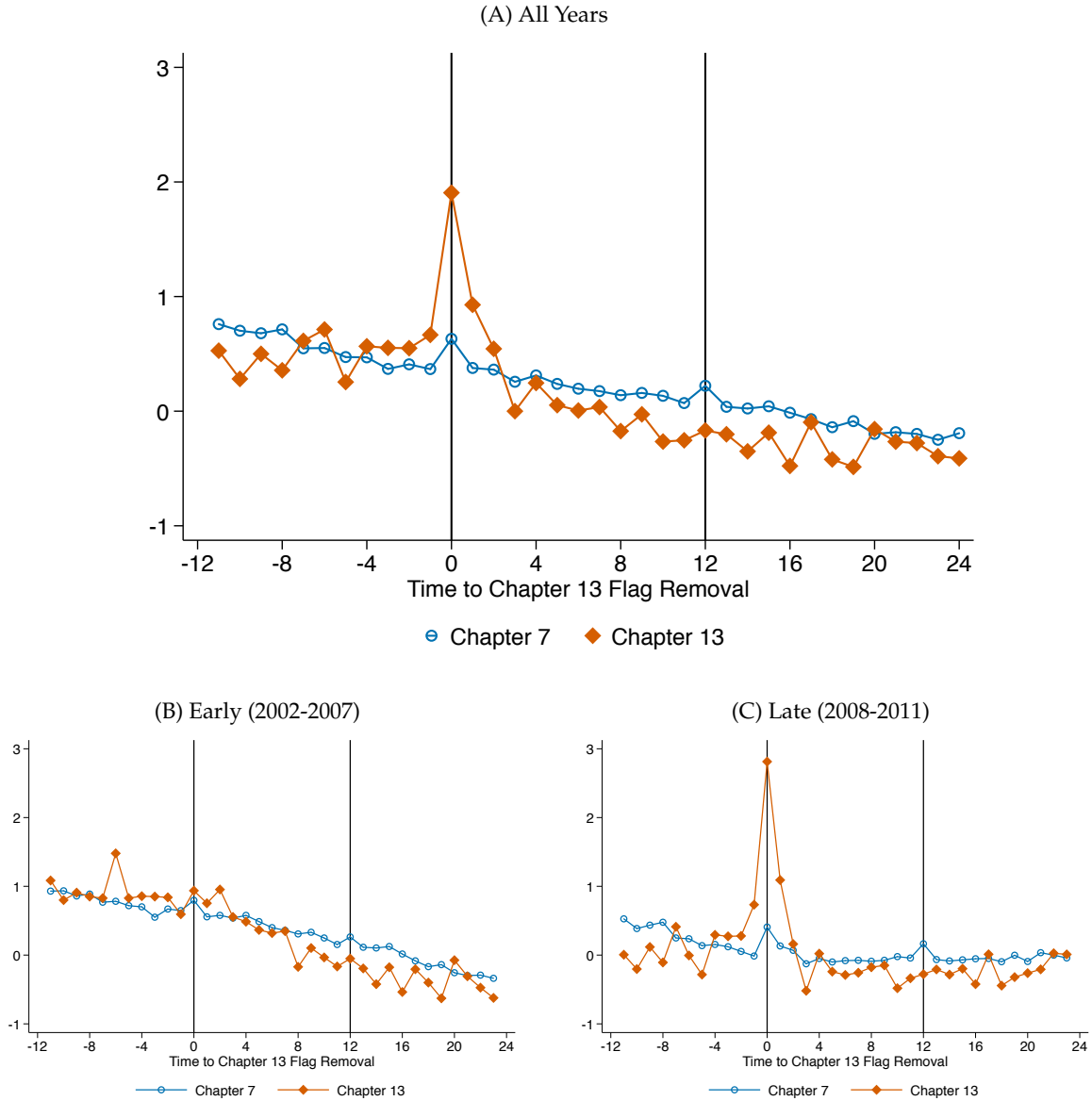
Note: This figure plots the net change in the stock of individuals with a mortgage loan. The net change is calculated as the difference between two objects: the inflow of mortgages, which is the fraction of individuals who go from having no mortgage in the previous quarter to having a mortgage in this quarter; and the outflow of mortgages, which is the fraction of individuals who go from having a mortgage in the previous quarter to having a mortgage in this quarter. Observations from states that enacted a ban on using credit reports in employment hiring are included in this figure. Panel A plots this net change for the entire sample. Panel B plots this change for individuals whose flag is removed between 2002 and 2007, inclusive. Panel C plots this change for individuals whose flag is removed between 2008 and 2011, inclusive.

**Figure A10:** 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 quarters, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical line shows the year of Chapter 13 flag removal. 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Figure A11: PSID Employment Trends

(A) Hours Worked > 0

(B) Labor Income > 0
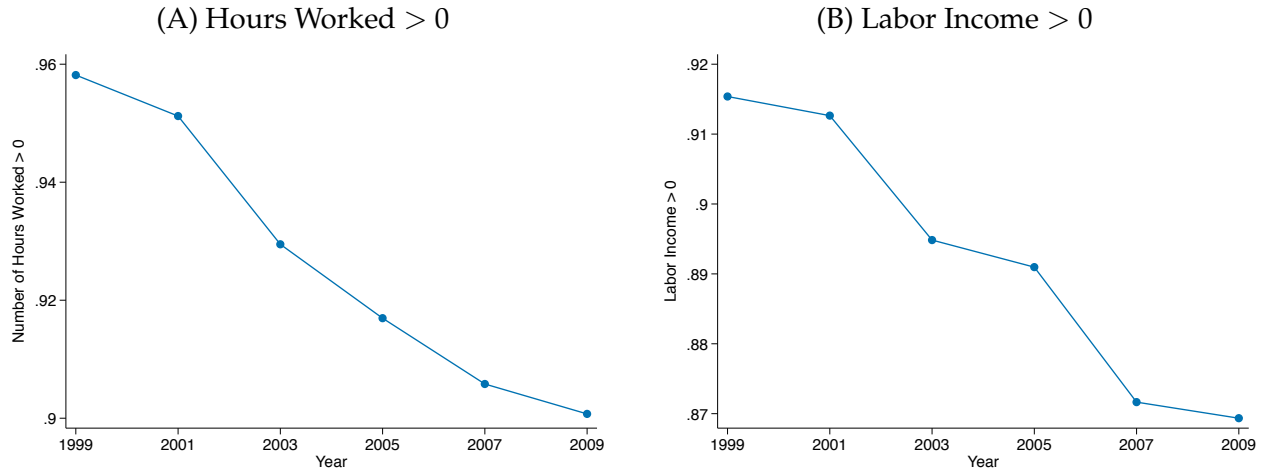
Note: This figure plots the outcomes of all heads of household who are between ages 30 and 55 in 2003. Panel A reports the average by year of an indicator for hours worked greater than zero. Panel B reports the average by year of an indicator for labor income greater than zero. Outcomes are weighted by the PSID weights.

Figure A12: Effect of Chapter 13 Flag Removal on Labor Market Outcomes by Period
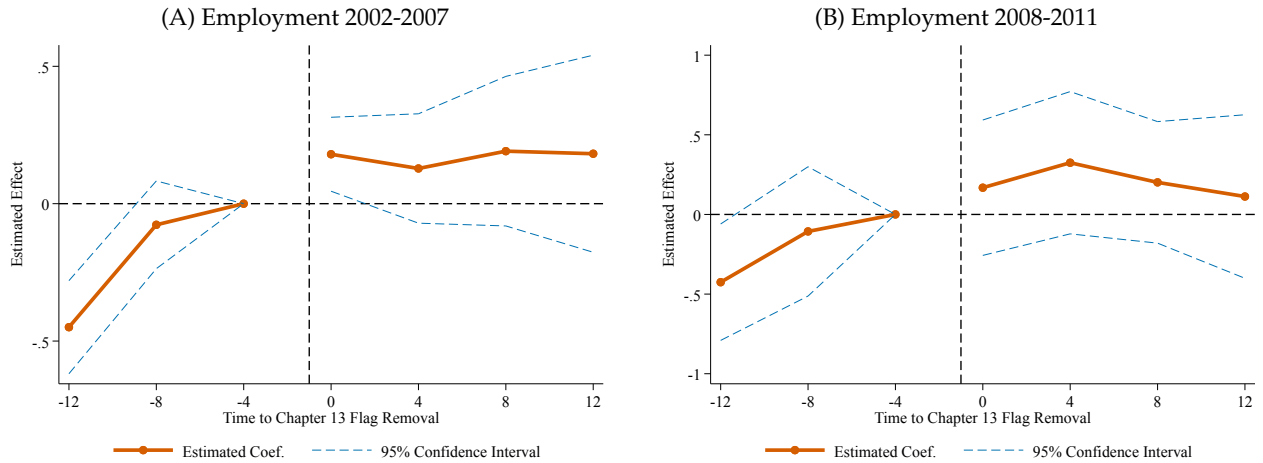
(A) Employment 2002-2007

(B) Employment 2008-2011

Note: This figure plots difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes for separate time periods, 2002-2007 and 2008-2011. The horizontal axis denotes time, in quarters, relative to the year of Chapter 13 flag removal, which occurs 7 years after filing. The dashed vertical line shows the year of Chapter 13 flag removal. 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. 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 quarters, 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 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 year prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A14: 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 quarters, 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 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 year prior to flag removal. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A15: State Difference-in-Differences

(A) Employment Trends (Chapter 7)

(B) Employment Trend (Chapter 13)

(C) Employment Effects (Chapter 7)

(D) Employment Effects (Chapter 13)
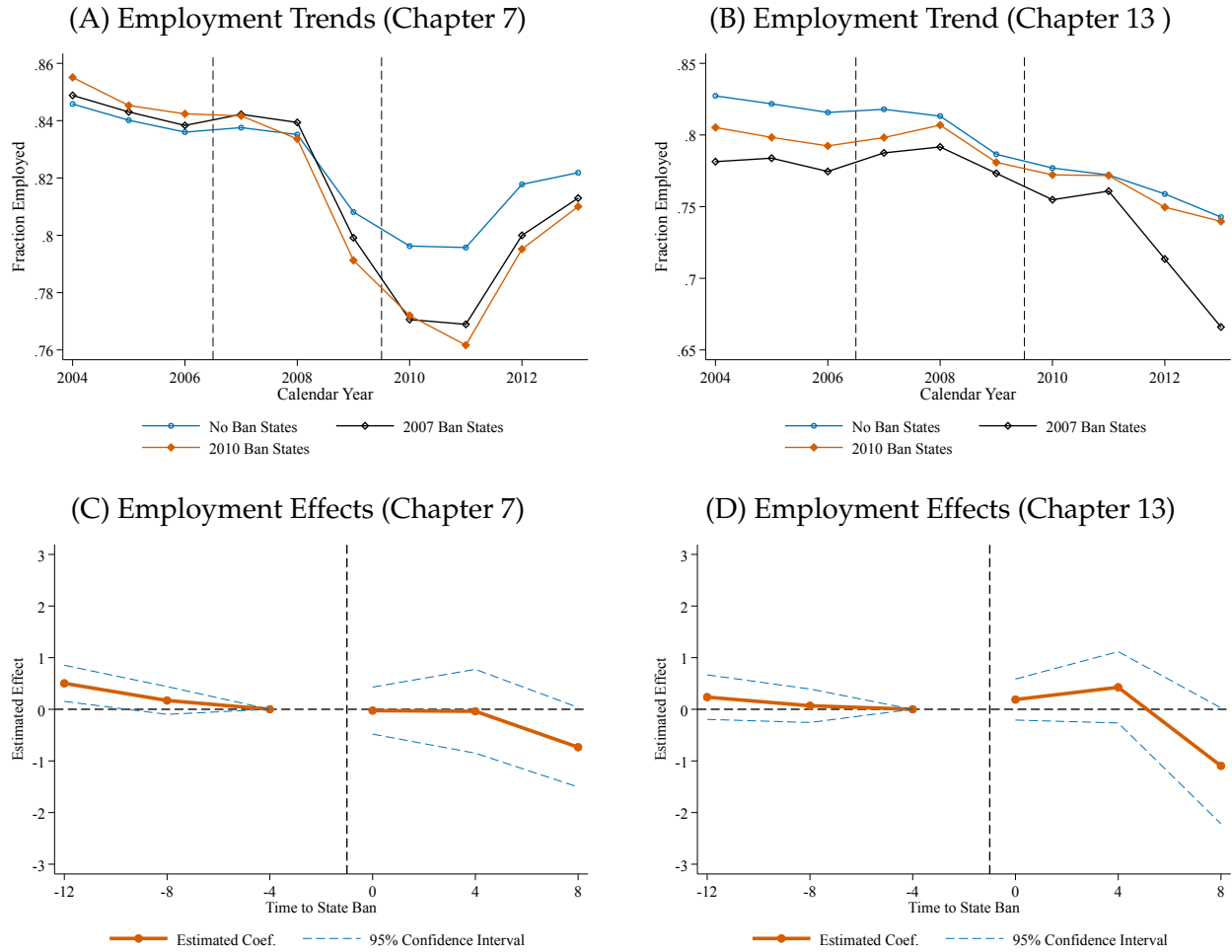
Note: These figures study the effect of state bans on employment outcomes for individuals who had filed for Chapter 7 or Chapter 13 bankruptcy 4 to 6 years before. Panels A and B plot employment for these filers in three groups: in states that banned the use of credit reports in employment in 2007 (Washington), banned the use of credit reports in employment in 2010 (Oregon) or never banned the use of credit reports. See Appendix Table A1 for a full list of states. Panels C and D plot the non-parametric difference-in-differences estimates from these samples relative to the state ban, with the year previous to the ban normalized to 0. The dashed lines are 95 percent confidence intervals from standard errors clustered at the 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 A16: Credit Score Trends by Pre-Flag Removal Credit Score

(A) Bottom Quartile

(B) Second Quartile

(C) Third Quartile

(D) Top Quartile

Note: This figure plots average credit scores for Chapter 13 and Chapter 7 bankruptcy filers by predicted pre-flag removal credit score. In order to split the sample into quartiles, we construct predicted pre-removal credit scores from a linear regression of observed 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A17: Implied Probability of Default 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. In order to split the sample into quartiles, we construct predicted pre-removal credit scores from a linear regression of observed 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 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. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A18: Credit Score Trends with Linear Counterfactual

(A) Credit Score

(B) Implied Probability of Default

Note: This figure plots average credit scores and the implied probability of default for Chapter 13 and Chapter 7 bankruptcy filers and a linear counterfactual line for Chapter 13 filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The vertical line shows the quarter of Chapter 13 flag removal. 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A19: Credit Card Trends with Linear Counterfactual

(A) Credit Card Limits

(B) Credit Card Balances

Note: This figure plots the average credit card outcomes for Chapter 13 and Chapter 7 bankruptcy filers and a linear counterfactual line for Chapter 13 filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The vertical line shows the quarter of Chapter 13 flag removal. 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A20: Mortgage Trends with Linear Counterfactual
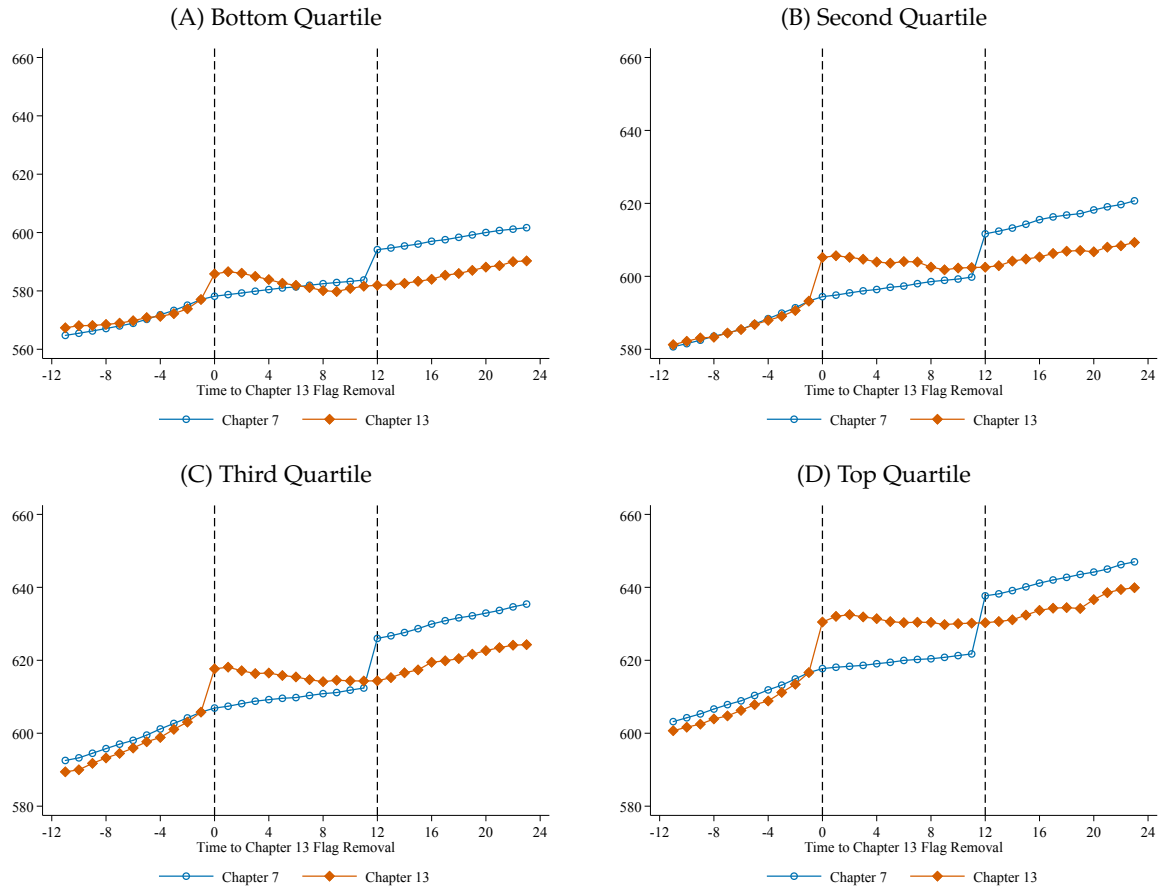
(A) Mortgage Loan

(B) Log(Mortgage Balances + 1)

Note: This figure plots the average mortgage outcomes for Chapter 13 and Chapter 7 bankruptcy filers and a linear counterfactual line for Chapter 13 filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The vertical line shows the quarter of Chapter 13 flag removal. 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.
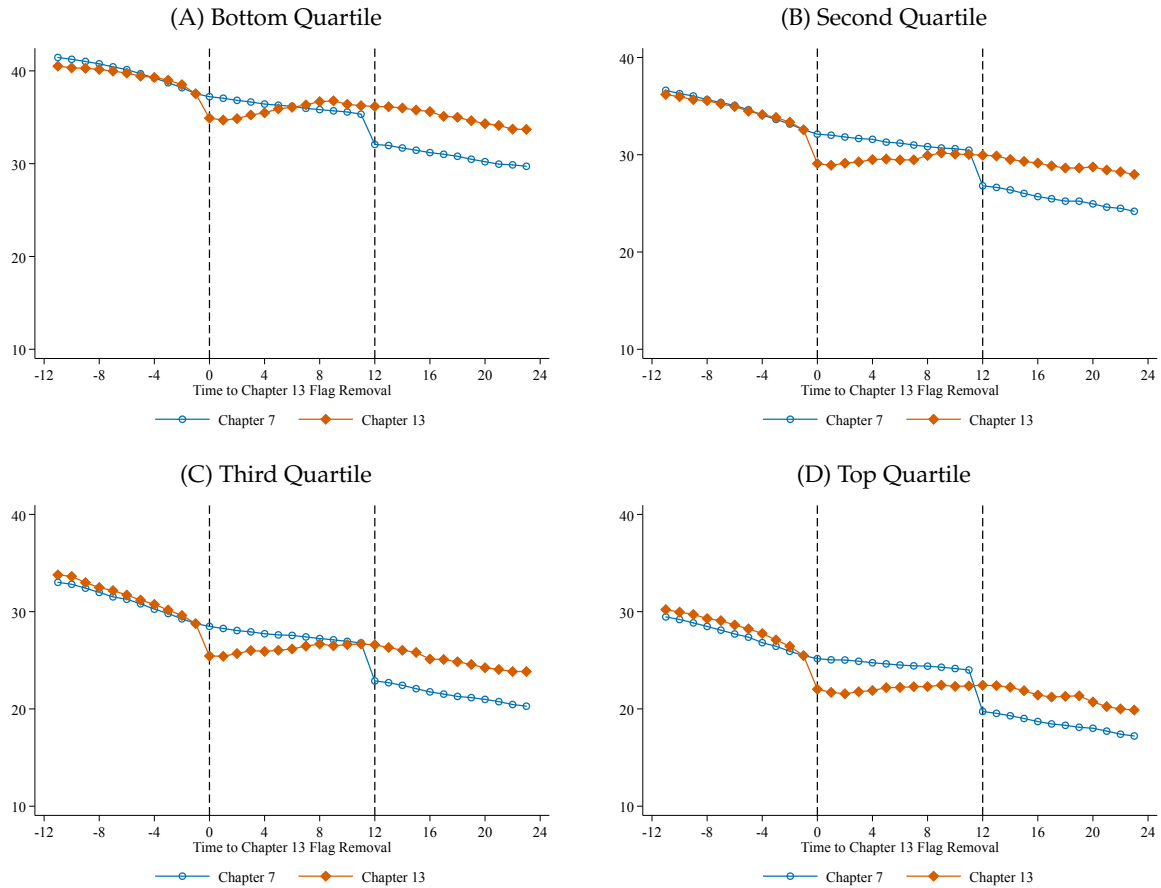
Figure A21: Labor Market Trend with Linear Counterfactual

Note: This figure plots the average labor market outcomes for Chapter 13 and Chapter 7 bankruptcy filers and a linear counterfactual line for Chapter 13 filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The vertical line shows the quarter of Chapter 13 flag removal. 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A22: Employment Transitions Trends with Linear Counterfactual

(A) Change Job

Chapter 13 — Linear Counterfactual
Chapter 7

(B) Same Job

Chapter 13 — Linear Counterfactual
Chapter 7

(C) Start Job

Chapter 13 — Linear Counterfactual
Chapter 7

(D) End Job

Chapter 13 — Linear Counterfactual
Chapter 7

(E) No Job

Chapter 13 — Linear Counterfactual
Chapter 7

Note: This figure plots the average labor market outcomes of bankruptcy filers to the event of bankruptcy flag removal and a linear counterfactual line for Chapter 13 filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The vertical line shows the quarter of Chapter 13 flag removal. 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A23: Employment by Industry Trends with Linear Counterfactual

Note: This figure plots the average labor market outcomes of bankruptcy filers to the event of bankruptcy flag removal and a linear counterfactual line for Chapter 13 filers. The horizontal axis denotes time, in quarters, relative to the quarter of Chapter 13 flag removal, which occurs 7 years after filing. The vertical line shows the quarter of Chapter 13 flag removal. 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 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. See the Table 1 notes for additional details on the outcome measures and sample.

Figure A24: Labor Supply versus Labor Demand
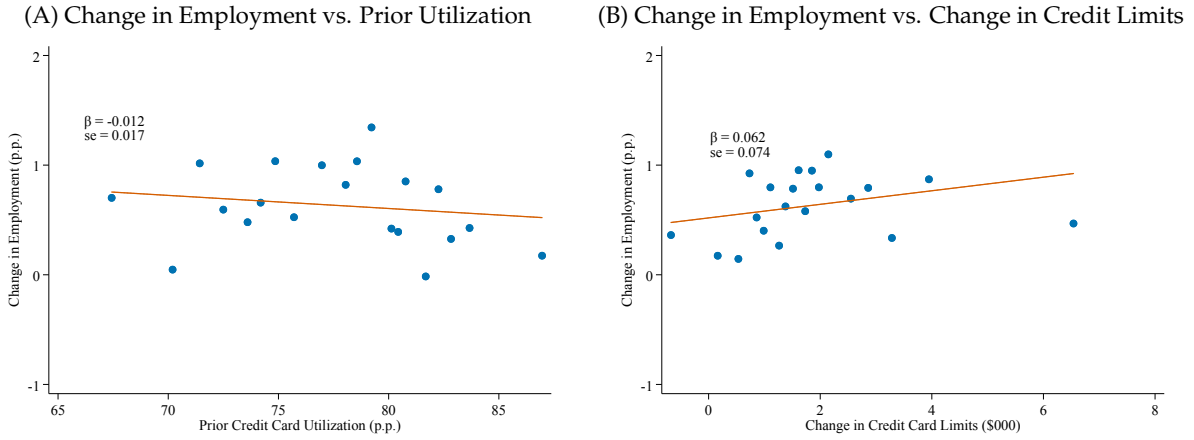
(A) Change in Employment vs. Prior Utilization

(B) Change in Employment vs. Change in Credit Limits

Note: This figure shows binned scatter plots of the estimated coefficients from difference-in-differences regressions conducted at the age group-by-state group level. Panel A plots the percentage change in employment against the average credit card utilization rates in $t = -4$ for that age group-by-state bin. Panel B plots the percentage change in employment against the change in credit limits. The solid line shows the best linear fit estimated on the underlying age-by-state level data weighted by the number of observations within each group. The coefficients show the estimated slope of the best-fit line, with standard errors clustered at the state level reported below. The effects on employment and credit limits are at one year after flag removal. See the text for additional details on the specification.

Table A1: State Credit Check Bans

State	Year of Ban
Washington	2007
Hawaii	2009
Oregon	2010
Illinois	2011
Maryland	2011
California	2012
Connecticut	2012
Vermont	2012
Colorado	2013
Nevada	2013
Delaware	2014

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) and Friedberg, Hynes, and Pattison (2017) for additional details.

Table A2: Credit Results by Pre-Flag Removal Credit Score

	Mean at $t = -1$	Difference-in-Differences Estimates			
		1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
Score Quartile 1:					
Credit Score	577.05	7.00	1.47	−2.33	2.05
	(33.53)	(0.62)	(0.89)	(0.96)	(0.75)
		[0.00]	[0.51]	[0.17]	[0.09]
Implied Probability of Default	37.54	−2.06	−0.35	0.87	−0.51
	(9.85)	(0.19)	(0.27)	(0.28)	(0.22)
		[0.00]	[0.77]	[0.04]	[0.18]
Credit Card Limits	2,049.53	264.72	673.70	889.77	609.22
	(1,798.60)	(23.44)	(51.49)	(78.97)	(47.34)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Balance	1,465.60	145.10	437.87	584.69	389.10
	(1,352.55)	(21.51)	(42.51)	(51.02)	(32.91)
		[0.00]	[0.00]	[0.00]	[0.00]
Had Mortgage Loan	36.84	1.61	1.45	0.42	1.16
	(21.28)	(0.33)	(0.47)	(0.51)	(0.40)
		[0.00]	[0.04]	[0.77]	[0.07]
Score Quartile 2:					
Credit Score	593.21	10.12	6.94	3.30	6.79
	(34.24)	(0.58)	(0.89)	(1.06)	(0.75)
		[0.00]	[0.00]	[0.04]	[0.00]
Implied Probability of Default	32.57	−2.84	−1.84	−0.65	−1.78
	(10.14)	(0.19)	(0.28)	(0.33)	(0.24)
		[0.00]	[0.00]	[0.29]	[0.00]
Credit Card Limits	2,682.25	311.41	889.94	1,330.30	843.26
	(2,159.47)	(39.88)	(78.33)	(99.89)	(67.66)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Balance	1,777.18	85.03	459.71	774.60	439.39
	(1,494.69)	(28.52)	(59.61)	(80.44)	(50.53)
		[0.05]	[0.00]	[0.00]	[0.00]
Had Mortgage Loan	40.74	1.84	2.02	1.45	1.77
	(21.26)	(0.36)	(0.44)	(0.52)	(0.41)
		[0.00]	[0.00]	[0.08]	[0.00]
Score Quartile 3:					
Credit Score	605.79	9.55	5.97	2.86	6.13

	(32.66)	(0.60)	(0.95)	(1.16)	(0.80)
		[0.00]	[0.00]	[0.17]	[0.00]
Implied Probability of Default	28.77	−2.56	−1.46	−0.42	−1.48
	(9.51)	(0.19)	(0.27)	(0.34)	(0.23)
		[0.00]	[0.00]	[0.77]	[0.00]
Credit Card Limits	3,242.97	363.48	1,145.02	1,649.26	1,052.04
	(2,481.76)	(46.71)	(100.55)	(123.07)	(83.70)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Balance	2,013.85	149.77	639.27	967.12	585.07
	(1,554.56)	(42.87)	(58.66)	(83.83)	(53.51)
		[0.01]	[0.00]	[0.00]	[0.00]
Had Mortgage Loan	44.56	2.23	1.97	1.47	1.89
	(21.26)	(0.48)	(0.57)	(0.64)	(0.53)
		[0.00]	[0.01]	[0.18]	[0.01]

Score Quartile 4:

Credit Score	616.63	13.57	11.09	9.06	11.24
	(36.64)	(0.85)	(1.26)	(1.40)	(1.03)
		[0.00]	[0.00]	[0.00]	[0.00]
Implied Probability of Default	25.48	−3.28	−2.45	−1.86	−2.53
	(10.26)	(0.25)	(0.36)	(0.39)	(0.30)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Limits	3,973.97	556.81	1,556.29	1,962.75	1,357.59
	(3,124.91)	(61.06)	(142.38)	(184.13)	(121.54)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Balance	2,421.30	148.47	729.72	995.44	624.08
	(2,332.66)	(60.77)	(89.96)	(115.57)	(79.25)
		[0.17]	[0.00]	[0.00]	[0.00]
Had Mortgage Loan	45.48	2.75	3.02	2.41	2.73
	(23.59)	(0.58)	(0.68)	(0.77)	(0.63)
		[0.00]	[0.00]	[0.04]	[0.00]

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal by predicted pre-flag removal credit score. In order to split the sample into quartiles, we construct predicted pre-removal credit scores from a linear regression of observed 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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 278,982 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise *p*-values are in brackets. 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: Labor Results by Time Period

| | Mean at | Difference-in-Differences Estimates | | | |
	$t = -1$	1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
Wage Employment 2002-2007	88.39	0.18	0.13	0.19	0.17
	(29.73)	(0.07)	(0.10)	(0.14)	(0.08)
		[0.03]	[0.32]	[0.32]	[0.12]
Wage Employment 2008-2011	86.44	0.17	0.32	0.20	0.23
	(31.79)	(0.21)	(0.22)	(0.19)	(0.18)
		[0.63]	[0.59]	[0.63]	[0.63]

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market outcomes in two time periods: 2002-2007 and 2008-2011. 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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime in 2002-2011. The regressions include a panel of 3,525,914 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A4: Effect of Chapter 13 Flag Removal on Labor Market Outcomes

| | Mean at | Difference-in-Difference Estimates | | | |
| | $t = -1$ | 1 Year | 2 Years | 3 Years | Pooled |
	(1)	(2)	(3)	(4)	(5)
Same Job	69.56	−0.80	−1.16	−1.65	−1.20
	(28.40)	(0.11)	(0.12)	(0.13)	(0.10)
		[0.00]	[0.00]	[0.00]	[0.00]
Change Job	16.29	0.82	1.31	1.69	1.27
	(8.81)	(0.14)	(0.13)	(0.12)	(0.11)
		[0.00]	[0.00]	[0.00]	[0.00]
No Job	9.29	−0.24	−0.35	−0.47	−0.35
	(26.89)	(0.05)	(0.06)	(0.08)	(0.06)
		[0.00]	[0.00]	[0.00]	[0.00]
End Job	3.17	0.06	0.12	0.28	0.16
	(12.22)	(0.09)	(0.10)	(0.07)	(0.07)
		[1.00]	[1.00]	[0.01]	[1.00]
Start Job	1.68	0.15	0.09	0.15	0.13
	(7.45)	(0.04)	(0.04)	(0.04)	(0.03)
		[0.03]	[1.00]	[0.05]	[0.01]

Note: This table reports difference-in-differences estimates of the effect of Chapter 13 bankruptcy flag removal on labor market job transitions. 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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 3,525,914 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A5: Effect of Chapter 13 Flag Removal on Labor Market Outcomes

| | Mean at | Difference-in-Difference Estimates | | | |
| | $t = -1$ | 1 Year | 2 Years | 3 Years | Pooled |
	(1)	(2)	(3)	(4)	(5)
Missing	24.63	−0.03	−0.03	0.10	0.01
	(27.75)	(0.14)	(0.13)	(0.15)	(0.12)
		[1.00]	[1.00]	[1.00]	[1.00]
Construction	4.07	0.01	0.06	0.08	0.05
	(4.54)	(0.03)	(0.03)	(0.04)	(0.03)
		[1.00]	[1.00]	[1.00]	[1.00]
Manufacturing	14.97	−0.33	−0.55	−0.77	−0.55
	(10.09)	(0.08)	(0.10)	(0.12)	(0.09)
		[0.00]	[0.00]	[0.00]	[0.00]
Transportation	6.92	−0.01	−0.02	−0.03	−0.02
	(5.39)	(0.03)	(0.03)	(0.04)	(0.03)
		[1.00]	[1.00]	[1.00]	[1.00]
Wholesale	4.10	0.01	−0.00	−0.03	−0.01
	(3.68)	(0.02)	(0.03)	(0.03)	(0.03)
		[1.00]	[1.00]	[1.00]	[1.00]
Retail	6.71	0.16	0.24	0.34	0.25
	(4.98)	(0.03)	(0.04)	(0.04)	(0.03)
		[0.00]	[0.00]	[0.00]	[0.00]
Finance	4.21	0.03	0.08	0.08	0.06
	(4.13)	(0.03)	(0.04)	(0.04)	(0.03)
		[1.00]	[1.00]	[1.00]	[1.00]
Services	25.60	0.25	0.41	0.54	0.40
	(15.17)	(0.08)	(0.07)	(0.10)	(0.07)
		[0.07]	[0.00]	[0.00]	[0.00]
Public	7.52	−0.08	−0.17	−0.29	−0.18
	(5.92)	(0.03)	(0.04)	(0.05)	(0.04)
		[0.72]	[0.00]	[0.00]	[0.00]

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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 3,525,914 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A6: Employment Results and State Bans on Credit Checks for Employment

| | Difference-in-Difference Estimates | | |
| | 1 Year | 2 Years | 3 Years |
	(1)	(2)	(3)
Chapter 7:			
Wage Employment	−0.03	−0.04	−0.73
	(0.23)	(0.41)	(0.39)
	[1.00]	[1.00]	[1.00]
Self-Employment	−0.00	−0.05	−0.21
	(0.02)	(0.06)	(0.15)
	[1.00]	[1.00]	[1.00]
Any Employment	−0.07	−0.05	−0.92
	(0.20)	(0.37)	(0.29)
	[1.00]	[1.00]	[0.23]
Log(Wages + 1)	0.00	−0.00	−0.08
	(0.03)	(0.04)	(0.04)
	[1.00]	[1.00]	[1.00]
Log(Self-Employment Earnings + 1)	−0.00	−0.01	−0.02
	(0.00)	(0.01)	(0.02)
	[1.00]	[1.00]	[1.00]
Log(Earnings + 1)	−0.00	−0.01	−0.10
	(0.02)	(0.04)	(0.03)
	[1.00]	[1.00]	[0.16]
Chapter 13:			
Wage Employment	0.19	0.43	−1.10
	(0.20)	(0.35)	(0.56)
	[1.00]	[1.00]	[1.00]
Self-Employment	−0.05	−0.08	−0.11
	(0.04)	(0.09)	(0.16)
	[1.00]	[1.00]	[1.00]
Any Employment	0.06	0.31	−1.13
	(0.13)	(0.27)	(0.36)
	[1.00]	[1.00]	[0.22]
Log(Wages + 1)	0.02	0.04	−0.11
	(0.02)	(0.04)	(0.04)
	[1.00]	[1.00]	[1.00]
Log(Self-Employment Earnings + 1)	−0.00	−0.01	−0.02
	(0.01)	(0.01)	(0.02)
	[1.00]	[1.00]	[1.00]

Log(Earnings + 1)	0.01	0.03	−0.12
	(0.02)	(0.03)	(0.03)
	[1.00]	[1.00]	[0.01]

All:

Wage Employment	0.00	0.02	−0.75
	(0.21)	(0.40)	(0.39)
	[1.00]	[1.00]	[1.00]
Self-Employment	−0.01	−0.05	−0.20
	(0.02)	(0.05)	(0.14)
	[1.00]	[1.00]	[1.00]
Any Employment	−0.05	−0.00	−0.92
	(0.18)	(0.36)	(0.28)
	[1.00]	[1.00]	[0.15]
Log(Wages + 1)	0.00	0.00	−0.08
	(0.02)	(0.04)	(0.04)
	[1.00]	[1.00]	[1.00]
Log(Self-Employment Earnings + 1)	−0.00	−0.01	−0.02
	(0.00)	(0.01)	(0.01)
	[1.00]	[1.00]	[1.00]
Log(Earnings + 1)	−0.00	−0.00	−0.10
	(0.02)	(0.04)	(0.03)
	[1.00]	[1.00]	[0.07]

Note: This table reports difference-in-differences estimates of the effect of state bans on credit checks for employment for individuals who had filed for bankruptcy 4 to 6 years before. Columns 1-3 report coefficients on the effect of flag removal for years 1-3 after the ban. The sample in Panel A includes Chapter 7 filers who were age 30-54 7 years after bankruptcy filing and successfully completed the bankruptcy process. The sample in Panel B includes Chapter 13 filers who were age 30-54 7 years after bankruptcy filing and successfully completed the bankruptcy process. Panel C combines both samples. The regressions include 2,043,905 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A7: Event Study Estimates of Chapter 13 Flag Removal on Credit Scores

| | Mean at | Event Study Estimates | | | |
| | $t = -1$ | 1 Year | 2 Years | 3 Years | Pooled |
	(1)	(2)	(3)	(4)	(5)
Credit Score	594.07	6.94	−0.03	−5.10	0.60
	(36.80)	(0.60)	(0.87)	(1.23)	(0.84)
		[0.00]	[1.00]	[0.00]	[1.00]
Implied Probability of Default	32.34	−1.75	0.35	1.96	0.19
	(10.81)	(0.19)	(0.27)	(0.37)	(0.26)
		[0.00]	[1.00]	[0.00]	[1.00]

Note: This table reports event study 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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 29,328 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A8: Event Study Estimates of Chapter 13 Flag Removal on Credit Card Outcomes

| | Mean at $t = -1$ | Event Study Estimates | | | |
| | | 1 Year | 2 Years | 3 Years | Pooled |
	(1)	(2)	(3)	(4)	(5)
Credit Card Limits	2,784.10	794.24	1,391.87	1,650.02	1,278.71
	(2,382.75)	(53.71)	(89.64)	(118.53)	(83.53)
		[0.00]	[0.00]	[0.00]	[0.00]
Credit Card Balance	1,819.01	259.18	596.52	740.94	532.21
	(1,639.17)	(32.95)	(62.22)	(82.00)	(56.71)
		[0.00]	[0.00]	[0.00]	[0.00]

Note: This table reports event study 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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 29,328 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A9: Event Study Estimates of Chapter 13 Flag Removal on Mortgage Outcomes using Linear Counterfactual

| | Mean at | Event Study Estimates | | | |
| | $t = -1$ | 1 Year | 2 Years | 3 Years | Pooled |
	(1)	(2)	(3)	(4)	(5)
Had Mortgage Loan	41.01	1.89	0.34	−2.31	−0.03
	(21.86)	(0.44)	(0.57)	(0.74)	(0.56)
		[0.00]	[1.00]	[0.02]	[1.00]
Log(Mortgage Balance + 1)	4.61	0.18	−0.03	−0.40	−0.08
	(2.50)	(0.05)	(0.07)	(0.09)	(0.07)
		[0.01]	[1.00]	[0.00]	[1.00]

Note: This table reports event study 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. Column 5 reports coefficients on the effect of flag removal from quarters 0-11, which pools columns 2-4. The sample includes Chapter 13 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 29,328 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A10: Event Study Estimates of Chapter 13 Flag Removal on Labor Market Outcomes using Linear Counterfactual

	Mean at	Linear Estimates			
	$t = -1$	1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
Wage Employment	87.51	0.04	−0.05	−0.13	−0.05
	(30.68)	(0.09)	(0.12)	(0.15)	(0.11)
		[1.00]	[1.00]	[1.00]	[1.00]
Self-Employment	5.59	0.00	−0.00	−0.02	−0.01
	(9.22)	(0.05)	(0.08)	(0.11)	(0.07)
		[1.00]	[1.00]	[1.00]	[1.00]
Any Employment	89.96	0.05	−0.03	−0.14	−0.04
	(25.34)	(0.10)	(0.13)	(0.15)	(0.12)
		[1.00]	[1.00]	[1.00]	[1.00]
Log(Wages + 1)	9.14	0.01	0.01	0.01	0.01
	(3.28)	(0.01)	(0.01)	(0.02)	(0.01)
		[1.00]	[1.00]	[1.00]	[1.00]
Log(Self-Employment Earnings + 1)	0.48	0.00	0.00	−0.00	0.00
	(0.86)	(0.00)	(0.01)	(0.01)	(0.01)
		[1.00]	[1.00]	[1.00]	[1.00]
Log(Earnings + 1)	9.38	0.01	0.01	0.01	0.01
	(2.77)	(0.01)	(0.02)	(0.02)	(0.01)
		[1.00]	[1.00]	[1.00]	[1.00]

Note: This table reports event study 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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 393,321 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A11: Event Study Estimates of Employment Results by Filer Characteristics using Linear Counterfactual

	Mean at $t = -1$	Linear Estimates			
		1 Year	2 Years	3 Years	Pooled
	(1)	(2)	(3)	(4)	(5)
White Males 30-39	91.10	0.08	0.29	0.39	0.25
	(25.57)	(0.15)	(0.22)	(0.30)	(0.21)
		[1.00]	[1.00]	[1.00]	[1.00]
Non-White Males 30-39	90.51	0.17	0.16	0.22	0.18
	(27.23)	(0.19)	(0.29)	(0.37)	(0.26)
		[1.00]	[1.00]	[1.00]	[1.00]
White Males 40-54	84.60	−0.01	−0.08	−0.34	−0.14
	(33.51)	(0.13)	(0.19)	(0.24)	(0.18)
		[1.00]	[1.00]	[1.00]	[1.00]
Non-White Males 40-54	85.83	−0.04	−0.25	−0.41	−0.23
	(32.72)	(0.15)	(0.24)	(0.31)	(0.22)
		[1.00]	[1.00]	[1.00]	[1.00]
White Females 30-39	87.77	0.57	0.52	1.01	0.70
	(29.83)	(0.32)	(0.49)	(0.64)	(0.47)
		[1.00]	[1.00]	[1.00]	[1.00]
Non-White Females 30-39	93.39	0.18	0.27	0.52	0.33
	(22.93)	(0.21)	(0.26)	(0.37)	(0.26)
		[1.00]	[1.00]	[1.00]	[1.00]
White Females 40-54	85.40	−0.11	−0.39	−0.50	−0.33
	(32.95)	(0.17)	(0.25)	(0.32)	(0.23)
		[1.00]	[1.00]	[1.00]	[1.00]
Non-White Females 40-54	90.44	−0.04	−0.38	−0.67	−0.36
	(27.54)	(0.15)	(0.23)	(0.31)	(0.22)
		[1.00]	[1.00]	[1.00]	[1.00]

Note: This table reports event study 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. Column 5 reports the pooled estimates from years 1 through 3. The sample includes Chapter 13 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. The regressions include a panel of 393,321 unique individuals. Standard errors clustered at the chapter-by-cohort-year-by-state level are in parentheses. Family-wise p-values are in brackets. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample.

Table A12: Employment Results by Gender × Race × Age × State

Gender	Race	Age	State	Estimate	Adj. p-value
Female	Non-white	40 and Older	RI	7.26	[0.00]
Female	Non-white	40 and Older	NM	6.37	[0.18]
Female	Non-white	Under 40	WY	8.75	[0.33]
Male	Non-white	40 and Older	ID	8.32	[0.74]
Female	Non-white	Under 40	DC	4.01	[0.99]
Male	White	Under 40	VA	2.77	[1.00]
Male	White	40 and Older	UT	2.06	[1.00]
Female	Non-white	40 and Older	ID	6.19	[1.00]
Male	White	40 and Older	IN	1.61	[1.00]
Female	Non-white	40 and Older	SC	1.67	[1.00]
Male	Non-white	40 and Older	UT	3.13	[1.00]
Female	White	40 and Older	VA	1.81	[1.00]
Male	White	40 and Older	NY	0.94	[1.00]
Female	Non-white	Under 40	RI	5.14	[1.00]
Female	White	40 and Older	IN	1.79	[1.00]
Male	White	Under 40	OK	2.31	[1.00]
Female	White	40 and Older	ME	5.33	[1.00]
Female	White	40 and Older	IA	2.62	[1.00]
Female	White	40 and Older	KY	1.97	[1.00]
Female	Non-white	40 and Older	AL	−1.48	[1.00]
Female	White	40 and Older	SC	−2.25	[1.00]
Male	White	Under 40	IN	1.07	[1.00]
Female	Non-white	Under 40	MA	−12.57	[1.00]
Male	Non-white	Under 40	OH	1.76	[1.00]
Male	Non-white	40 and Older	VA	1.65	[1.00]
Male	White	Under 40	SC	1.81	[1.00]
Female	White	Under 40	MN	3.28	[1.00]
Male	Non-white	40 and Older	AK	9.75	[1.00]
Male	White	Under 40	NE	2.97	[1.00]
Male	White	Under 40	PA	1.84	[1.00]
Male	White	40 and Older	OK	1.63	[1.00]
Male	Non-white	Under 40	VA	−1.45	[1.00]
Male	Non-white	40 and Older	MA	−1.85	[1.00]
Female	White	Under 40	WI	3.17	[1.00]
Male	White	40 and Older	RI	5.09	[1.00]
Female	Non-white	Under 40	TN	−1.20	[1.00]
Male	White	Under 40	AL	1.48	[1.00]

Female	Non-white	40 and Older	FL	1.53	[1.00]
Female	Non-white	Under 40	WV	4.96	[1.00]
Female	White	Under 40	UT	2.80	[1.00]
Female	White	40 and Older	AR	1.12	[1.00]
Male	Non-white	Under 40	FL	2.16	[1.00]
Male	White	40 and Older	MT	2.54	[1.00]
Male	Non-white	40 and Older	IA	−8.81	[1.00]
Male	White	Under 40	OH	0.98	[1.00]
Male	Non-white	Under 40	DE	−7.04	[1.00]
Female	Non-white	40 and Older	MT	9.83	[1.00]
Male	Non-white	Under 40	WI	2.04	[1.00]
Male	White	40 and Older	SC	1.30	[1.00]
Male	White	Under 40	UT	1.70	[1.00]
Female	Non-white	40 and Older	IA	6.58	[1.00]
Male	White	Under 40	AK	2.22	[1.00]
Male	Non-white	Under 40	TX	1.51	[1.00]
Female	Non-white	40 and Older	TX	−0.95	[1.00]
Male	White	40 and Older	TX	−0.63	[1.00]
Male	White	40 and Older	OH	0.94	[1.00]
Female	White	40 and Older	RI	−6.45	[1.00]
Female	White	40 and Older	OK	−2.18	[1.00]
Female	Non-white	Under 40	NY	1.98	[1.00]
Male	White	40 and Older	GA	−1.57	[1.00]
Male	Non-white	Under 40	SC	−1.96	[1.00]
Male	Non-white	40 and Older	IN	1.54	[1.00]
Male	Non-white	Under 40	DC	−9.45	[1.00]
Male	White	40 and Older	VA	1.00	[1.00]
Female	White	Under 40	IA	3.92	[1.00]
Female	White	40 and Older	MN	3.38	[1.00]
Female	White	Under 40	TX	1.87	[1.00]
Male	Non-white	Under 40	MO	1.28	[1.00]
Female	Non-white	Under 40	AR	−2.13	[1.00]
Male	White	Under 40	ID	1.95	[1.00]
Female	White	Under 40	LA	−2.45	[1.00]
Female	Non-white	Under 40	NE	4.35	[1.00]
Male	Non-white	Under 40	NY	3.23	[1.00]
Male	Non-white	40 and Older	WY	9.83	[1.00]
Female	White	40 and Older	PA	0.85	[1.00]
Male	White	40 and Older	AR	−0.68	[1.00]
Male	Non-white	Under 40	NE	5.85	[1.00]
Male	White	Under 40	WY	3.69	[1.00]

Female	Non-white	40 and Older	KS	3.12	[1.00]
Male	White	40 and Older	MI	1.12	[1.00]
Female	Non-white	40 and Older	AK	2.68	[1.00]
Female	White	Under 40	GA	1.94	[1.00]
Male	Non-white	Under 40	TN	0.83	[1.00]
Male	Non-white	40 and Older	NM	3.30	[1.00]
Male	Non-white	40 and Older	TX	0.96	[1.00]
Female	White	40 and Older	MI	1.12	[1.00]
Female	White	Under 40	VA	3.53	[1.00]
Male	Non-white	40 and Older	DC	2.70	[1.00]
Male	Non-white	Under 40	RI	9.83	[1.00]
Female	White	Under 40	ND	1.95	[1.00]
Female	White	40 and Older	UT	1.60	[1.00]
Male	White	Under 40	DE	2.55	[1.00]
Male	White	40 and Older	NM	−1.90	[1.00]
Male	Non-white	40 and Older	KS	1.48	[1.00]
Female	Non-white	Under 40	DE	5.83	[1.00]
Male	Non-white	Under 40	GA	−1.25	[1.00]
Male	White	40 and Older	DC	7.92	[1.00]
Male	White	Under 40	WV	−3.76	[1.00]
Male	White	Under 40	MS	1.06	[1.00]
Female	Non-white	Under 40	ME	5.62	[1.00]
Male	White	40 and Older	MO	0.86	[1.00]
Female	White	40 and Older	AZ	1.41	[1.00]
Female	Non-white	40 and Older	ND	3.53	[1.00]
Female	Non-white	Under 40	OK	2.11	[1.00]
Male	Non-white	40 and Older	AR	−1.25	[1.00]
Female	White	Under 40	KS	−2.67	[1.00]
Female	Non-white	40 and Older	SD	−12.57	[1.00]
Female	White	40 and Older	MO	0.81	[1.00]
Male	White	40 and Older	ME	1.47	[1.00]
Female	White	Under 40	SD	0.95	[1.00]
Male	White	Under 40	WI	1.10	[1.00]
Female	White	Under 40	AR	−1.59	[1.00]
Female	Non-white	Under 40	KY	2.74	[1.00]
Male	Non-white	40 and Older	LA	−0.59	[1.00]
Female	White	40 and Older	SD	6.03	[1.00]
Female	White	40 and Older	KS	1.25	[1.00]
Male	White	Under 40	AZ	0.92	[1.00]
Female	Non-white	40 and Older	ME	2.59	[1.00]
Male	Non-white	Under 40	MA	3.86	[1.00]

Male	Non-white	40 and Older	MT	−4.40	[1.00]
Female	Non-white	40 and Older	NY	1.18	[1.00]
Female	White	40 and Older	TN	0.92	[1.00]
Male	Non-white	Under 40	MS	−0.96	[1.00]
Female	Non-white	40 and Older	VA	0.71	[1.00]
Female	Non-white	40 and Older	AR	−0.77	[1.00]
Male	Non-white	40 and Older	GA	−1.01	[1.00]
Female	Non-white	Under 40	MI	−1.06	[1.00]
Female	White	Under 40	NY	1.12	[1.00]
Female	Non-white	Under 40	PA	−2.65	[1.00]
Female	Non-white	Under 40	UT	−3.50	[1.00]
Male	White	40 and Older	ID	−0.91	[1.00]
Female	Non-white	40 and Older	AZ	−2.20	[1.00]
Male	Non-white	40 and Older	DE	1.09	[1.00]
Female	White	40 and Older	NM	−1.55	[1.00]
Male	White	40 and Older	FL	0.84	[1.00]
Male	Non-white	Under 40	IN	1.30	[1.00]
Male	White	40 and Older	NC	−0.57	[1.00]
Male	Non-white	Under 40	AK	−12.57	[1.00]
Female	White	Under 40	MS	2.42	[1.00]
Male	White	Under 40	TX	0.45	[1.00]
Male	Non-white	40 and Older	MN	2.47	[1.00]
Male	Non-white	Under 40	MT	−5.73	[1.00]
Male	White	Under 40	NY	0.49	[1.00]
Male	Non-white	Under 40	AZ	1.33	[1.00]
Female	Non-white	Under 40	NM	5.90	[1.00]
Male	Non-white	40 and Older	ND	9.83	[1.00]
Female	White	Under 40	AK	2.16	[1.00]
Female	Non-white	40 and Older	DC	−1.45	[1.00]
Male	Non-white	Under 40	OK	1.54	[1.00]
Male	White	Under 40	DC	−6.99	[1.00]
Male	White	40 and Older	DE	1.17	[1.00]
Female	White	Under 40	SC	1.59	[1.00]
Male	Non-white	40 and Older	KY	1.16	[1.00]
Male	White	Under 40	MO	0.74	[1.00]
Female	White	Under 40	MO	−1.10	[1.00]
Male	Non-white	40 and Older	SC	−0.90	[1.00]
Male	Non-white	40 and Older	RI	−2.14	[1.00]
Female	White	40 and Older	ID	2.26	[1.00]
Female	Non-white	Under 40	TX	−0.86	[1.00]
Male	Non-white	Under 40	SD	3.35	[1.00]

Female	White	40 and Older	DE	−2.19	[1.00]
Male	White	40 and Older	SD	1.24	[1.00]
Female	White	Under 40	KY	−1.20	[1.00]
Male	White	Under 40	NM	−0.77	[1.00]
Female	White	Under 40	TN	1.01	[1.00]
Male	White	40 and Older	TN	0.34	[1.00]
Female	White	40 and Older	NE	0.86	[1.00]
Female	Non-white	40 and Older	NC	−0.61	[1.00]
Male	Non-white	Under 40	AL	−0.62	[1.00]
Male	Non-white	Under 40	KY	2.33	[1.00]
Female	White	Under 40	MA	−2.95	[1.00]
Male	Non-white	40 and Older	FL	0.75	[1.00]
Female	White	40 and Older	TX	−0.64	[1.00]
Male	Non-white	40 and Older	NY	−0.61	[1.00]
Male	Non-white	Under 40	ND	1.43	[1.00]
Male	Non-white	40 and Older	AZ	0.90	[1.00]
Female	Non-white	Under 40	VA	0.69	[1.00]
Male	White	40 and Older	LA	0.48	[1.00]
Female	Non-white	Under 40	IA	1.45	[1.00]
Female	White	Under 40	PA	0.97	[1.00]
Female	White	Under 40	NC	0.90	[1.00]
Female	Non-white	Under 40	ND	1.93	[1.00]
Female	White	40 and Older	MT	−1.92	[1.00]
Male	Non-white	Under 40	NC	−0.65	[1.00]
Male	White	Under 40	MN	0.88	[1.00]
Female	Non-white	Under 40	AK	2.72	[1.00]
Male	White	Under 40	KY	0.67	[1.00]
Female	Non-white	Under 40	MT	−12.57	[1.00]
Male	White	Under 40	MI	0.49	[1.00]
Female	Non-white	40 and Older	DE	1.57	[1.00]
Female	Non-white	40 and Older	PA	−0.57	[1.00]
Female	White	40 and Older	OH	0.46	[1.00]
Male	Non-white	Under 40	KS	1.17	[1.00]
Female	Non-white	Under 40	AL	0.38	[1.00]
Female	Non-white	Under 40	NC	0.64	[1.00]
Male	Non-white	Under 40	ME	−9.63	[1.00]
Male	Non-white	Under 40	LA	−0.51	[1.00]
Male	White	40 and Older	ND	0.88	[1.00]
Female	Non-white	Under 40	GA	0.64	[1.00]
Female	Non-white	Under 40	MO	0.74	[1.00]
Male	Non-white	Under 40	UT	1.05	[1.00]

Female	White	40 and Older	WI	0.85	[1.00]
Female	Non-white	40 and Older	MA	1.73	[1.00]
Female	White	Under 40	DC	9.25	[1.00]
Female	Non-white	Under 40	KS	1.36	[1.00]
Female	White	Under 40	ME	3.51	[1.00]
Female	White	Under 40	FL	1.14	[1.00]
Male	White	Under 40	KS	0.61	[1.00]
Female	Non-white	Under 40	MS	0.43	[1.00]
Female	Non-white	40 and Older	GA	−0.38	[1.00]
Male	Non-white	40 and Older	OH	0.40	[1.00]
Male	White	40 and Older	NE	0.50	[1.00]
Female	White	Under 40	NE	1.08	[1.00]
Female	White	Under 40	OK	−0.97	[1.00]
Female	White	40 and Older	NY	0.37	[1.00]
Male	White	40 and Older	WY	1.46	[1.00]
Male	White	Under 40	MA	0.80	[1.00]
Male	White	40 and Older	WI	0.54	[1.00]
Male	Non-white	40 and Older	AL	0.29	[1.00]
Female	Non-white	40 and Older	UT	−1.21	[1.00]
Female	White	Under 40	MT	2.12	[1.00]
Male	White	40 and Older	PA	0.20	[1.00]
Female	Non-white	40 and Older	WI	−0.64	[1.00]
Female	White	Under 40	DE	−2.52	[1.00]
Female	White	Under 40	RI	−3.12	[1.00]
Male	Non-white	40 and Older	WV	3.59	[1.00]
Male	White	40 and Older	AK	−1.56	[1.00]
Female	Non-white	Under 40	MN	3.10	[1.00]
Male	Non-white	Under 40	IA	−1.39	[1.00]
Female	White	40 and Older	FL	−0.74	[1.00]
Male	Non-white	Under 40	WY	−7.18	[1.00]
Male	White	Under 40	SD	2.59	[1.00]
Female	White	Under 40	AL	−0.58	[1.00]
Female	Non-white	40 and Older	MO	0.37	[1.00]
Female	Non-white	40 and Older	MS	0.30	[1.00]
Male	Non-white	40 and Older	PA	0.38	[1.00]
Male	White	Under 40	NC	0.29	[1.00]
Female	White	Under 40	AZ	−0.99	[1.00]
Male	Non-white	40 and Older	OK	−0.64	[1.00]
Male	Non-white	40 and Older	ME	1.31	[1.00]
Female	Non-white	Under 40	AZ	0.74	[1.00]
Female	Non-white	40 and Older	LA	0.32	[1.00]

Female	Non-white	40 and Older	WY	−5.43	[1.00]
Male	Non-white	Under 40	MN	−1.78	[1.00]
Female	White	40 and Older	AK	−2.68	[1.00]
Female	White	40 and Older	MS	−0.30	[1.00]
Male	White	40 and Older	KS	0.35	[1.00]
Male	White	40 and Older	KY	0.29	[1.00]
Female	Non-white	Under 40	ID	−1.68	[1.00]
Male	White	40 and Older	IA	−0.51	[1.00]
Female	White	40 and Older	AL	−0.27	[1.00]
Male	White	40 and Older	AZ	0.33	[1.00]
Male	Non-white	40 and Older	SD	−3.15	[1.00]
Male	White	40 and Older	MN	−0.40	[1.00]
Male	White	Under 40	ME	−1.44	[1.00]
Female	Non-white	Under 40	SD	−0.49	[1.00]
Female	Non-white	40 and Older	TN	−0.16	[1.00]
Female	Non-white	Under 40	SC	0.32	[1.00]
Female	Non-white	40 and Older	IN	0.21	[1.00]
Female	White	40 and Older	MA	−0.43	[1.00]
Female	White	Under 40	WY	−3.76	[1.00]
Male	Non-white	Under 40	MI	−0.40	[1.00]
Male	White	Under 40	IA	0.58	[1.00]
Male	Non-white	40 and Older	MS	0.23	[1.00]
Female	White	Under 40	NM	1.27	[1.00]
Female	White	40 and Older	DC	−1.55	[1.00]
Male	White	40 and Older	MA	−0.30	[1.00]
Male	Non-white	Under 40	NM	−0.74	[1.00]
Female	White	40 and Older	NC	0.15	[1.00]
Male	White	Under 40	RI	1.15	[1.00]
Female	Non-white	40 and Older	OH	0.15	[1.00]
Female	White	40 and Older	WY	0.56	[1.00]
Male	White	Under 40	LA	−0.18	[1.00]
Male	Non-white	Under 40	ID	−1.18	[1.00]
Male	White	Under 40	FL	0.24	[1.00]
Female	Non-white	40 and Older	NE	−0.53	[1.00]
Female	White	Under 40	ID	0.66	[1.00]
Female	Non-white	Under 40	IN	−0.38	[1.00]
Male	Non-white	Under 40	WV	1.86	[1.00]
Male	White	40 and Older	AL	−0.09	[1.00]
Male	White	Under 40	TN	−0.14	[1.00]
Female	Non-white	Under 40	FL	−0.27	[1.00]
Female	Non-white	40 and Older	MN	−0.47	[1.00]

Male	Non-white	Under 40	AR	0.15	[1.00]
Male	Non-white	40 and Older	MI	−0.11	[1.00]
Male	White	Under 40	GA	0.15	[1.00]
Male	White	40 and Older	WV	0.22	[1.00]
Female	White	Under 40	WV	0.78	[1.00]
Male	White	40 and Older	MS	0.10	[1.00]
Male	Non-white	40 and Older	NC	−0.08	[1.00]
Female	Non-white	40 and Older	KY	−0.16	[1.00]
Male	Non-white	40 and Older	TN	−0.06	[1.00]
Female	White	Under 40	MI	0.13	[1.00]
Female	Non-white	40 and Older	MI	0.09	[1.00]
Female	Non-white	Under 40	WI	−0.09	[1.00]
Male	White	Under 40	ND	0.50	[1.00]
Female	White	40 and Older	LA	0.09	[1.00]
Female	White	Under 40	OH	−0.10	[1.00]
Female	Non-white	Under 40	OH	0.10	[1.00]
Female	White	40 and Older	ND	0.33	[1.00]
Male	White	Under 40	MT	−0.27	[1.00]
Female	White	40 and Older	GA	−0.08	[1.00]
Male	Non-white	40 and Older	NE	0.09	[1.00]
Male	Non-white	Under 40	PA	0.12	[1.00]
Female	White	Under 40	IN	0.06	[1.00]
Female	Non-white	40 and Older	OK	−0.07	[1.00]
Female	Non-white	Under 40	LA	−0.03	[1.00]
Female	White	40 and Older	WV	−0.11	[1.00]
Male	Non-white	40 and Older	WI	0.03	[1.00]
Male	White	Under 40	AR	0.02	[1.00]
Female	Non-white	40 and Older	WV	−0.06	[1.00]
Male	Non-white	40 and Older	MO	−0.00	[1.00]

Note: This table lists each of the difference-in-differences estimates for formal sector employment at three years for 384 mutually exclusive subsamples determined by the full interaction of gender, race, age, and state of filing. The sample includes Chapter 13 and Chapter 7 filers who were age 30-54 at the time of Chapter 13 flag removal, successfully completed the bankruptcy process, and had their bankruptcy flag removed sometime between 2002-2011, and were not from states that enacted a ban on using credit reports in employment hiring at any time during our sample period. Family-wise p-values are in brackets. The only group out of the 384 we consider that has a statistically significant effect is non-white females age 40 or older living in Rhode Island. See the text for additional details on the specification and the Table 1 notes for additional details on the outcome measures and sample. See Figure 6 for a histogram of the coefficients.