Do Employer Credit Check Bans Increase Default?

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Abstract

Employers often perform credit checks on prospective employees. Twelve states have banned this practice with the goal of improving employment prospects for individuals with poor credit histories. An unintended consequence of these bans is that they reduce the incentive to repay debt. This paper provides the first causal evidence of how pre-employment credit check bans (PECCBs) affect debt repayment. I find that PECCBs increase the probability of bankruptcy by 0.9 percentage points on average, equivalent to a 17.6% increase from the mean. The probabilities of past-due accounts and collections are unaffected on average, but heterogeneity by credit score prior to treatment shows that these probabilities increase among all consumers except those with the lowest scores. These findings show that consumers are sensitive to changes in the penalty for default, especially when they are deciding whether to file bankruptcy.

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1 Introduction

How much should consumers be penalized for failing to repay their debts? Negative consequences of default include a lower credit score, which reduces access to future credit, as well as difficulty finding housing and employment. These consequences can deter strategic defaults by individuals who are capable of making their debt payments. On the other hand, a penalty that is too severe can damage prospects for financial recovery after a consumer misses debt payments due to a shock such as job loss. This paper examines how removing the barrier to employment for individuals with poor credit histories affects debt repayment. This reduced penalty for default both removes a deterrent against strategic default and reduces the barrier to regaining financial well-being for those who have defaulted in the past. The question is whether improved financial health or an increase in strategic defaults will have the dominant effect on debt repayment.

I study this question in the context of pre-employment credit check bans (PECCBs). These laws prohibit the widespread[1] practice among employers of checking a candidate's credit history during the hiring process. These credit checks influence employers' decisions: survey evidence shows that one in ten unemployed workers has been told that information in their credit report was the reason they were denied employment (Traub 2014). Eleven states and Washington, D.C. have implemented PECCBs in the hopes of improving employment prospects for individuals with poor credit histories. Both the policy discussion around these laws and the academic literature focus almost exclusively on their labor market consequences, with potential credit market effects largely ignored. However, PECCBs could have two possible effects on credit markets. First, if the laws improve employment prospects for individuals with poor credit histories, those individuals' debt repayment may improve as their income increases. Second, the reduced penalty for failing to repay debt could lead to more past-due accounts, collections, and bankruptcies. This paper provides the first causal evidence of how PECCBs affect debt repayment.

Taking advantage of variation in the adoption of PECCBs across states and over time, I use two-way fixed effects to estimate the causal effect of these policies on debt repayment using detailed, individual-level data from one of the three major credit bureaus. The main result is that on average,

1. A study by the Professional Background Screening Association found that 51% of employers use credit checks in hiring decisions (Hicks 2023).

PECCBs increase the probability of bankruptcy by 0.9 percentage points, equivalent to a 17.6% increase from the mean. The average borrower's financial situation is unchanged in terms of past-due accounts and collections: estimated effects on the probability of having any accounts 30, 60 or 90 days past due and on the probability of any collections are very close to zero and statistically insignificant, with small standard errors. The increased probability of bankruptcy is therefore not associated with an overall deterioration in financial well-being, suggesting that the effect on bankruptcy is driven by the lower penalty for default. Furthermore, the estimates show that debt repayment does not improve on average despite improved labor market prospects for individuals with poor credit histories.

Heterogeneity by credit score prior to treatment, where a higher score indicates a lower probability of default, shows that the null average effects on the probabilities of past-due accounts and collections mask substantial responses to PECCBs that vary across the credit score distribution. The probability of having an account past due decreases substantially for consumers in the lowest credit score quintile but rises in all other quintiles, with the largest increases in the middle of the credit score distribution. Similarly, the probability of any collections decreases in the lowest two credit score quintiles but increases in all other quintiles, with the largest increase in the middle quintile. By contrast, the probability of bankruptcy increases in all five credit score quintiles, with the largest increase occurring among consumers with the lowest scores and effect sizes decreasing as credit score rises. In summary, these results show that PECCBs increase the probability of bankruptcy for consumers in all credit score quintiles and increase the probabilities of past-due accounts and collections for all consumers except those with the lowest credit scores. Repeating the heterogeneity analyses for past-due accounts and collections with a sample that excludes all bankruptcy filers produces nearly identical estimates, showing that the decreased probabilities of past-due accounts and collections in the lowest credit score quintile are driven by a different group of low-scoring consumers than those who file for bankruptcy. The low-scoring consumers whose debt repayment outcomes improve are likely those who have better employment prospects under PECCBs, enabling them to make more debt payments on time. However, higher-scoring consumers increase not only their probability of bankruptcy but also their probability of having past-due accounts and collections in response to PECCBs.

The increased probability of bankruptcy could be driven by the decreased penalty for default or by a reduced ability to pay debt among consumers whose labor market prospects worsen as an unintended consequence of PECCBs. Prior work has shown that while PECCBs improve labor market outcomes for individuals with the lowest credit scores (Ballance et al. 2020), outcomes worsen for consumers who are under 22, have mediocre risk scores, or are Black (Ballance et al. 2020, Bartik and Nelson (2024)). Heterogeneity analysis based on demographic characteristics suggests that worse labor market outcomes in these groups are not the primary driver of the increase in bankruptcy. Heterogeneity by age shows that the increase in bankruptcy is largest among consumers aged 36-55, with a null effect on consumers aged 18-25. Heterogeneity by credit score shows the largest increase in bankruptcy in the lowest credit score quintile, where PECCBs improve rather than harm labor market outcomes. Heterogeneity based on the share of the county population that is Black shows the largest point estimate in counties with the highest share of Black residents, but estimates are positive for all quintiles of percent Black and the point estimate in the top quintile is not statistically different from the point estimate in the bottom quintile. Overall, these results suggests that the increase in bankruptcy is primarily due to strategic filers responding to the decreased cost of bankruptcy and not from deteriorating financial situations among those whose labor market prospects are harmed by PECCBs.

While event studies for the main outcomes suggest that the parallel trends assumption necessary for identification is plausible, it is still possible that the main result is spurious: another contemporaneous trend or policy change in the treated states besides PECCBs could be the true cause of the increase in bankruptcy. To assess this possibility, I take advantage of county-level variation in treatment intensity based on the share of the population exempt from PECCBs. Each state with a PECCB allows exemptions for certain occupations, typically those in which employees have opportunities for theft or fraud. Heterogeneity based on the percent of employees in the county who are exempt from the PECCB shows statistically significant increases in bankruptcy in the bottom three quintiles, all of which are larger in magnitude than the main result. In counties in the top quintile, with the highest percent of workers exempt from the ban, there is a statistically significant decrease in bankruptcies. This robustness check provides further evidence that the main result is driven by PECCBs and is not a spurious finding, as the probability of bankruptcy does not increase in counties with the highest shares of workers who are unaffected by the law. I also use a permutation test to assess the likelihood that the main two-way fixed effects estimate captures noise in the data and find a p-value of 0.024 for the main result, indicating a very low probability that the main result captures noise in the data rather than the effect of PECCBs.

Because the main results are estimated using a two-way fixed effects identification strategy with staggered adoption, I use the weighted stacked difference-in-differences estimator from Wing et al. (2024) to assess whether the main result is biased by the presence of heterogeneous treatment effects. The weighted stacked difference-in-differences estimate is very close to the two-way fixed effects estimate, includes the two-way fixed effects estimate in its 95% confidence interval, and is significant at the 5% level, alleviating the concern over bias from heterogeneous treatment effects.

These results provide new insights into how consumers respond to a decrease in the penalty for default. In the case of PECCBs, consumers respond to this decreased penalty by strategically filing for bankruptcy. Prior literature on bankruptcy filings discusses two competing models of individual bankruptcy filing decisions: the strategic model, in which an increase in the financial benefit of bankruptcy increases the likelihood of filing, and the nonstrategic model, in which bankruptcy filings are driven by adverse events that leave individuals unable to make their debt payments (Fay et al. 2002).[2] The findings in this paper are consistent with the strategic model of bankruptcy, as they show an increase in bankruptcy filings in response to a decrease in the cost of bankruptcy. While the likelihood of past-due accounts and collections is unaffected on average, it increases for individuals with moderate and high credit scores. The findings show that consumers are sensitive to changes in the penalty for default, particularly in the case of the decision to file bankruptcy. The findings also provide new evidence of an unintended consequence of PECCBs.

This paper contributes to the emerging literature on the effects of PECCBs by providing the first causal evidence of how these laws affect credit markets. To my knowledge, this paper is the first to examine how PECCBs affect bankruptcy filings and collections, as well as the first to estimate the average effect of PECCBs on delinquency. The literature on PECCBs has largely focused on their labor market consequences, providing insight into which groups experience labor market effects of PECCBs that might change their debt repayment behavior. Findings include improved labor market outcomes for job seekers who have experienced financial distress (Friedberg et al. 2021) or

2. Throughout the paper, descriptions of “strategic” bankruptcy filing refer to behavior that is consistent with the strategic model.

live in areas with low average credit scores (Ballance et al. 2020); worse labor market outcomes for individuals who are Black and without college degrees (Ballance et al. 2020, Bartik and Nelson 2024), under 22, or live in census tracts with mediocre credit scores (Ballance et al. 2020); and a decrease in job postings (Cortés et al. 2022). This literature largely assumes that credit markets are unaffected, with Dobbie et al. (2020) using the assumption that "these bans do not affect lending markets" in interpreting their results.

The one paper in this literature that discusses potential credit market effects of PECCBs is Corbae and Glover (2024). While Corbae and Glover (2024) primarily focus on labor market effects of these policies, they also provide a theoretical argument that PECCBs should reduce the incentive to repay debt, especially among individuals who had high credit scores prior to the ban. To motivate this assumption in their model, they provide one piece of empirical evidence: an event study in which they show that delinquency is more likely to increase among individuals with high credit scores than among those with low credit scores in response to a PECCB. Corbae and Glover (2024) do not empirically examine any other debt repayment outcomes, such as collections or bankruptcy. Furthermore, they do not provide empirical evidence on the *average* effect of PECCBs on delinquencies, only the *differential* effect of PECCBs on delinquencies for high-scoring borrowers. My results show that while delinquencies increase among high-scoring borrowers, heterogeneity across the credit score spectrum leads to a tight null effect on average. Furthermore, I show that the relationship between credit score and increased delinquency in response to PECCBs is nonlinear, with larger increases in the middle of the credit score distribution than at the top.

This paper also contributes to a small literature on how individuals, rather than firms, respond to bans on firms collecting specific information from job applicants. The idea that prospective employees may become more likely to engage in an undesirable behavior when a signal of that behavior is withheld from employers has been explored in the literature on effects of "ban-the-box" laws, which prohibit employers from asking job applicants about their criminal records. Sabia et al. (2021) finds that ban-the-box laws are associated with an increase in criminal incidents among Hispanic men, while Sherrard (2020) finds that ban-the-box laws increase recidivism among Black male ex-offenders. Both papers argue that worse labor market outcomes resulting from ban-the-box policies contribute to increased crime in these groups. While Sabia et al. (2021) notes that moral hazard may also play a role, neither paper finds an increase in criminal activity in groups whose labor market outcomes are not harmed by the policy. By contrast, in the PECCB setting I find that bankruptcy, delinquency and collections rates increase in demographic groups whose labor market outcomes are not harmed by the policy, showing that withholding a signal from employers can directly affect the behavior that is now concealed from employers by reducing the penalty for engaging in it.

Finally, this paper contributes to the literature on the causes of personal bankruptcy by providing new evidence on how consumers respond to a decrease in the cost of bankruptcy. The literature provides mixed evidence on whether consumers typically default strategically or out of necessity following events such as hospitalization or job loss. By improving labor market outcomes for individuals with poor credit histories and at the same time reducing their cost of bankruptcy, PECCBs provide an interesting new setting to study which of these forces has the dominant effect on bankruptcy filings. Using data from before a 2005 national bankruptcy reform that was intended to reduce strategic bankruptcy filings by raising the cost of bankruptcy, three papers provide evidence that the bankruptcy decision tends to be made strategically rather than out of necessity and that bankruptcy rates are highly responsive to changes in the costs and benefits of bankruptcy (Livshits et al. 2010, Fay et al. 2002, Gross and Souleles (2002)). These papers leave an open question as to whether bankruptcy decisions continued to be highly strategic after the 2005 reform.

More recent work has shown that households respond strategically to a decrease in the cost of bankruptcy by reducing precautionary savings (Braxton et al. 2024), and that consumers with higher bankruptcy costs pay more of their medical bills (Mahoney 2015). The literature using post-2005 data has found several factors that increase the likelihood of bankruptcy: job loss (Keys 2018); an increase in the assets a filer can retain in bankruptcy (Pattison and Hynes 2020, Miller 2019); increased access to credit through credit cards (Dick and Lehnert 2010) or payday loans (Skiba and Tobacman 2019); reduced access to unsecured personal loans (Danisewicz and Elard 2023); medical bills (Dobkin et al. 2018, Himmelstein et al. 2009); and strong social networks (Miller 2015). Other work has found that paid sick leave laws (Miller 2022), marriage and home ownership (Agarwal et al. 2011) reduce the likelihood of bankruptcy.

2 Background

2.1 Pre-Employment Credit Checks and Bans

In a pre-employment credit check, employers see a modified and limited version of the applicant's credit report. Employers do see credit accounts, balances, payment history, collections, credit inquiries, and past and current bankruptcies. They do not see credit scores; income; public records apart from bankruptcies; medical bills not in collections; or protected information such as birth date, marital status, religion, race, ethnicity, or information about the applicant's spouse (Akin 2024).

Employers, who have limited information about an applicant's quality, often see a pre-employment credit check as a useful signal (Bartik and Nelson 2024). A good credit history may be seen as evidence that a candidate is responsible. A credit report indicating financial distress may raise concerns that the candidate will embezzle, commit fraud or steal to repay her debts. These concerns motivate the use of credit reports for prospective employees whose roles will involve managing finances or handling money (Akin 2024). Cortés et al. (2022) find evidence that credit checks are also widely used in hiring decisions for workers who perform routine tasks or have less than a college degree.

Pre-employment credit checks may worsen job prospects for those with poor credit histories. Proponents of PECCBs have argued that individuals who are struggling to repay their debts, potentially because they are unemployed, could be trapped in financial distress if their poor credit histories prevent them from finding jobs (Traub 2014). PECCBs gained popularity in the aftermath of the Great Recession, as increased defaults during the recession raised the proportion of individuals whose employment prospects could be adversely affected by a credit check. Proponents of PECCBs have also argued that inaccurate information on credit reports could damage job prospects and that Black and Latino job applicants, who have lower average credit scores than White applicants, are disproportionately denied employment based on their credit reports (Maurer 2019). These concerns motivated the adoption of PECCBs in 11 states and Washington, D.C. between 2007 and 2017. The timeline of treatment is shown in Table 1.[3] All PECCBs provide exemptions for certain occupations, typically those in which employees have opportunities for embezzlement, fraud or theft. Prior literature has investigated whether PECCB implementation is systematically correlated with other

3. Pre-employment credit check bans have also been adopted in New York City and Chicago. I do not study the effects of these city-level bans in this paper.

policies that could affect the same group of job seekers affected by PECCBs, such as minimum wage laws and "ban-the-box" legislation, and has found no evidence of a correlation (Ballance et al. 2020).

2.2 Bankruptcy

Personal bankruptcy is a legal process that either discharges debt or creates a formal plan for repayment. Fisher (2019) examines demographic characteristics of bankruptcy filers in the United States and finds that bankruptcy filers tend to be middle-income and are more likely than the general population to be divorced, Black, middle-aged, employed, and have a terminal high school degree or some college.

Individuals can choose to file Chapter 7 bankruptcy or Chapter 13 bankruptcy. In Chapter 7, assets above a certain exemption limit are sold to repay debts. The debtor is not liable for paying any debts that remain after the proceeds from liquidating assets are distributed to lenders. In Chapter 13, individuals retain their assets but are liable for repaying debts with their wages as part of a formal repayment plan. The optimal type of bankruptcy depends on the state of residence and details of the filer's financial situation. Furthermore, filers must pass a "means test" to show that their disposable income is sufficiently low before they can qualify for Chapter 7. Individual credit reports also show Chapter 11 bankruptcies, which reorganize business debts, and Chapter 12 bankruptcies, which can only be filed by family farmers and family fishermen. In 2023, 55% of personal bankruptcies were filed under Chapter 7, and 45% were filed under Chapter 13. The 448,105 individuals who sought bankruptcy protection in 2023 had outstanding debts totaling $66 billion (United States Courts 2023).

While bankruptcy provides significant benefits for filers, it is also costly. Attorney fees are typically $1,000-2,000 for Chapter 7 bankruptcy and $3,000-5,000 for Chapter 13. Other fees, such as administrative fees and the cost of required pre- and post-filing courses, can total several hundred dollars (Safane 2024). In addition to these immediate monetary costs, bankruptcy substantially reduces future access to credit by lowering the filer's credit score by up to 200 points (Luthi 2024). Chapter 7 bankruptcy remains on the filer's credit report for 10 years, while Chapter 13 remains for seven years. Individuals with a bankruptcy on their credit report may be denied employment or may struggle to rent a home, as landlords typically prefer renters with "good" FICO credit scores of 670 or higher (Gerson 2024). Bankruptcy also carries a social stigma. Strategic individuals interested in bankruptcy will carefully consider whether these costs of filing outweigh the benefits.

2.3 Use of Bankruptcy Information in Hiring Decisions

An individual who is already employed is legally protected from discrimination based on her bankruptcy history: a current employer cannot reduce her salary, demote her or remove responsibilities solely because she filed for bankruptcy. However, protections for prospective employees are more limited. While federal, state and local government agencies cannot consider bankruptcy in hiring decisions, private employers can unless a PECCB is in place.

Private employers typically learn about an individual's bankruptcy history through a pre-employment credit check. Bankruptcy filings can also be found in court records. However, the process of obtaining a bankruptcy record from a court record system can be complex, and employers are therefore unlikely to learn about applicants' bankruptcy histories with this method (O'Neill 2024). Importantly, bankruptcy records will not appear if an employer simply Googles an applicant's name (Walker 2023); in other words, employers seeking general information about an applicant's online presence will not stumble across the individual's bankruptcy history by accident. Employers would only find the bankruptcy records by intentionally seeking them out.

When a PECCB is in place, employers cannot use an applicant's credit history to inform a hiring decision. These laws therefore prohibit not only the purchase or sale of a credit report on a prospective employee but also the search for bankruptcy records in court filing systems. For example, the PECCB in Washington, D.C. prohibits the use of "any written, oral, or other communication of information bearing on an employee's credit worthiness, credit standing, credit capacity, or credit history" in making employment decisions (D.C. Office of Human Rights 2017).

3 Data

3.1 Credit Bureau Data

I use credit bureau data that includes detailed, individual-level information on debt repayment and bankruptcy filings. This dataset provides annual snapshots of a 0.7% random sample of the population from 2005-2021. The credit bureau constructed the sample as follows: in 2005, they drew a 0.7% random sample of the population. In each subsequent year, they made two adjustments to the sample. First, they removed individuals who have had no activity for 10 years, have had only negative activity for seven years, or have had no activity except one inquiry followed by at least six months of inactivity. Second, they added new individuals to the sample to ensure that in each year, the sample continues to resemble a random sample of the U.S. population. Individuals who are added to the data remain in the sample in subsequent years unless they are removed based on one of the criteria listed above.

In addition to the steps taken by the credit bureau to construct the sample, I impose the following restrictions. First, I remove data from 2020 and 2021 due to external validity concerns raised by the Covid-19 pandemic. Second, I remove tradelines that were flagged as duplicates. Third, I remove observations identified as "fragments".[4] Fourth, I remove observations from U.S. territories and observations for which state information is missing. Fifth, I remove observations for which the consumer ID is observed only once. Sixth, I remove observations for which county-level demographic information, described below, is unavailable. After these restrictions are applied, the sample includes 28,575,143 person-year level observations.

3.2 Demographic Data

I use county-level data from the American Community Survey (ACS) to control for race, ethnicity, education, income, and county population. I also use this data to perform heterogeneity analysis based on county-level demographic characteristics.

For the years 2009-2019, I use five-year ACS estimates. From 2005-2008, only one-year ACS estimates are available, and these estimates are only provided for counties with populations of 65,000 or more. Therefore in the years 2005-2008, I use a one-year estimate if it is available and if it is not, I use the 2009 five-year estimate. ACS county-level demographic information is available for 99.9% of observations in the credit bureau data, with credit bureau data matched to ACS data using state and county FIPS codes.

4. A fragment occurs when a credit bureau creates a new credit report for an individual without realizing that individual has an existing credit report. The credit bureau combines data from fragment observations with the consumer's existing credit report, but leaves the fragment in the data as a separate observation with a flag.

3.3 Homestead Exemption Data

The homestead exemption is the amount of home equity that a bankruptcy filer can retain in Chapter 7 bankruptcy. The size of these exemptions varies considerably across states, from a $0 exemption in New Jersey and Pennsylvania to unlimited exemptions in eight states. An increase in the homestead exemption could increase the probability of filing for bankruptcy (Pattison and Hynes 2020, Agarwal et al. 2003, White 1987, Dawsey and Ausubel 2002, Edminston 2006), although other work has found null or negative effects of exemption increases on bankruptcy filings (Lefgren and McIntyre 2009, Dawsey et al. 2013). While homestead exemptions do change over time, these changes are often small adjustments designed to keep pace with inflation. The differences between states are stable over time in the sense that high-exemption states in 2005 are still high-exemption states in 2019 and low-exemption states in 2005 still have low exemptions in 2019 relative to other states. Twenty states did not change their homestead exemptions at all over this time period.

State-year level data on the size of the homestead exemption for single and joint bankruptcy filers was available in the replication data from Pattison and Hynes (2020) for the years 2005-2017 for the 50 states. I hand-collected data on homestead exemptions for the years 2018-2019 for the 50 states and for the years 2005-2019 for Washington, D.C. from state laws.

3.4 Insights from Raw Data

Table 2 shows summary statistics for the treated and control states. These statistics provide evidence that the two groups of states are similar both demographically and financially: average values of all outcomes and demographic variables in the control group are within one standard deviation of the average in the treated group, with the exception of percent high school educated.

Figures 1, 2 and 3 show trends over time in the raw data for "ever treated" states that have passed a PECCB and "never treated" states that have not. These graphs provide descriptive evidence that financial outcomes in the treated and control states tend to follow similar trends over time: Figures 1 and 2 especially show that the percent of the population with any accounts 30 days past due and the percent of the population with any collections follow similar trends in the two groups of states. Figures 1 and 2 also suggest that PECCBs had little to no effect on these two outcomes: the two groups of states continue to follow parallel trends after treatment occurs between 2008 and 2015.

By contrast, Figure 3 suggests that PECCBs may have increased the percent of the population with any bankruptcies. The treated states had a smaller percent with any bankruptcies in 2007, the last year of data before any states were treated; as treatment was implemented between 2008 and 2015, the percent with any bankruptcies in the treated states increased until it exceeded the value in the control states, and remains elevated above the control states until the end of the sample period.

4 Methodology

4.1 Main Specification

I use a two-way fixed effects identification strategy to estimate a causal effect, taking advantage of variation across states and over time in adoption of PECCBs. The main regression specification is:

$$Y_{ist} = \beta_0 + \beta_1 Ban_{st} + \beta_2 X_{it} + \beta_3 Z_{st} + \gamma_i + \theta_s + \delta_t + \epsilon_{ist} \quad (1)$$

Ban_{st} is an indicator for whether a PECCB is in effect in state s in year t. As the annual snapshot from the credit bureau is taken in June, the first treated year is the first year in which a ban was in effect by June. β_1 is the coefficient of interest. X_{it} is an individual-level control for age. Z_{st} denotes state-level controls for income, education, race, ethnicity, total population, and bankruptcy homestead exemption size.

Y_{ist} is an outcome for individual i in state s in year t. The outcomes used in the main analysis are indicators for having any accounts 30 days past due, any accounts 60 days past due, any accounts 90 days past due, any collections, and any bankruptcy. Regressions include individual, state and year fixed effects. Standard errors are clustered at the state level. A linear probability model is used to estimate the main results. A logistic regression, discussed in the Appendix, produces very similar results.

The timing of treatment varied across states. Recent advances in the two-way fixed effects literature have shown that in settings with staggered adoption, two-way fixed effects estimates can be biased by negative weights (Roth et al. 2023). I use the weighted stacked DID estimator from

Wing et al. (2024) to address this concern, with more details provided in the Robustness Checks section.[5]

4.2 Event Studies

Event studies are used to assess the plausibility of the parallel trends assumption and examine dynamic effects. The regression specification for the event studies is:

$$Y_{ist} = \beta_0 + \sum_{\substack{j=-5 \\ j\neq -1}}^{5} \beta_j \mathbb{1}\{t = j\} * EverTreated + \beta_2 X_{it} + \beta_3 Z_{st} + \gamma_i + \theta_s + \delta_t + \epsilon_{ist}$$

Variables are the same as in the main specification, except that $EverTreated$ is an indicator for whether a state has ever implemented a PECCB and j is the number of years since the ban was implemented, with negative numbers indicating the number of years prior to treatment.

5 Main Results

Table 3 shows TWFE estimates of the effects of PECCBs. Columns 1-4 show precisely estimated null effects of these laws on the probabilities of having any accounts 30 days past due, any accounts 60 days past due, any accounts 90 days past due, and any collections. The estimates and standard errors in the main specification that includes all controls are nearly identical to those from a second specification that removes the individual-level control for age and a third specification in which only fixed effects are used. Figures 4 and 5 show event studies that provide support for the parallel trends assumption for these outcomes, with statistically insignificant estimates in the pre-treatment periods. In the post-treatment period, all estimates are statistically insignificant and magnitudes tend to be close to zero, which is consistent with the finding of a null average effect.

Column 5 shows the main result of the paper: PECCBs increase the probability of bankruptcy. I estimate that PECCBs increase the probability of any bankruptcy by 0.9 percentage points. This result is statistically significant at the 5% level. The two alternative specifications produce nearly identical estimates of 0.8 and 1 percentage points, both of which are significant at the 5% level. A 0.9 percentage point increase in bankruptcy is equivalent to a 17.6% increase from the mean. Figure

5. The two-stage difference-in-differences estimator from Gardner (2022) and the multiperiod difference-in-differences estimator from de Chaisemartin and D'Haultfœuille (2020) are used as additional robustness checks, with more details provided in the Appendix.

6 shows the event study for this outcome, which provides support for the parallel trends assumption: three out of four pre-treatment estimates are statistically insignificant, and an F-test shows that the four pre-treatment estimates are jointly insignificant, with a p-value of 0.1. While all point estimates in the pre-treatment period are slightly negative, in the post-treatment period all point estimates are positive and increasing over time, which is consistent with the overall finding of an increase in bankruptcy filings.

This substantial increase in bankruptcy filings indicates a strong consumer response to the decrease in the penalty for filing for bankruptcy when PECCBs are enacted. The tight null effects on the probability of having any accounts past due or any collections indicate that the increase in bankruptcy filings does not arise from increased financial distress culminating in bankruptcy, but rather from strategic bankruptcy filings increasing in response to the lower cost of having a poor credit history. Furthermore, the null effects on non-bankruptcy financial distress measures indicate that any improvement in labor market outcomes resulting from PECCBs does not lead to better debt repayment on average.

6 Heterogeneity

6.1 Credit Score

While the main results show that PECCBs on average have tight null effects on the probabilities of past-due accounts and collections, the model in Corbae and Glover (2024) predicts an increased probability of delinquency among individuals with high credit scores in response to PECCBs. I use heterogeneity analysis by credit score to investigate whether effects of PECCBs vary across the credit score distribution. The credit score used for this analysis ranges from 280 to 850, with higher scores indicating a lower risk of failing to repay debt. The population is divided into quintiles based on credit score for this analysis. Credit score from a single year is used for the purpose of assignment to a quintile: the last year prior to treatment in the treated states and 2012 (the median year of treatment) in the control states. Scores from the last year prior to treatment are used because credit score is directly affected by changes in debt repayment resulting from PECCBs, which would create difficulty in interpreting heterogeneity based on post-PECCB scores.

The results of this heterogeneity analysis show that the null average effects on the probabilities of past-due accounts and collections mask significant responses that vary across the credit score distribution. Figures 7, 8, 9 and 10 show results of the heterogeneity analysis for four outcomes: the probability of having any accounts 30, 60 or 90 days past due and the probability of any collections. I find that the probability of having any accounts 30 days past due decreases by 2.7 percentage points for individuals in the lowest credit score quintile but increases by a statistically significant amount in all other quintiles, with the positive point estimates shrinking as credit score rises in quintiles 2 through 5. PECCBs increase the probability of having any accounts 30 days past due by 0.9 percentage points in quintile 2, but only 0.1 percentage points in quintile 5. No quintile is unaffected by the policy, with statistically significant estimates in every quintile. Results for the probability of having any accounts 60 days past due and any accounts 90 days past due show identical patterns, with slightly different point estimates.

I estimate that PECCBs decrease the probability of having any collections by 2.6 percentage points for consumers in the lowest quintile and by 1.3 percentage points in the second-lowest quintile. As with the probability of having any accounts 30, 60 or 90 days past due, point estimates in the highest three quintiles are positive and decreasing in size as credit score increases.

Figure 11 shows how the effect of PECCBs on the probability of bankruptcy varies by credit score. While bankruptcy filings increase in all quintiles, effect sizes shrink as credit score increases. For individuals in the first quintile, with the lowest credit scores, PECCBs increase the probability of filing for bankruptcy by 2.5 percentage points. This estimate is more than three times as large as the main result. Point estimates monotonically decrease as credit score rises. In the highest quintile, PECCBs increase the probability of filling for bankruptcy by only 0.1 percentage points. These results show that individuals with low credit scores are the most sensitive to a decrease in the cost of bankruptcy.

Results for all four non-bankruptcy outcomes show that on-time repayment improves for individuals in the lowest credit score quintile in response to PECCBs. This finding is not driven by past-due accounts being removed from bankruptcy filers' credit reports, as the estimates are nearly identical when all bankruptcy filers are removed from the sample.[6] Therefore, individuals in the lowest credit

6. See Appendix for details.

score quintile have heterogeneous responses to PECCBs. One group of individuals in this quintile strategically increases bankruptcy filings in response to the lower penalty for bankruptcy. A second, separate group within the same low-scoring quintile improves repayment of debt, resulting in fewer accounts past due or in collections. Improved debt repayment in this second group likely results from PECCBs improving employment outcomes among individuals with the lowest credit scores, as was found in Friedberg et al. (2021).

The increased probability of having any accounts 30 days past due, 60 days past due, 90 days past due, or in collections among individuals with higher credit scores is an unintended consequence of PECCBs. The point estimates show that the probability of failing to repay debts on time increases most among individuals at or just below the middle of the credit score distribution, with the largest increase in past-due accounts occurring in the second credit score quintile and the largest increase in collections found in the third credit score quintile. This heterogeneity analysis shows a more nuanced relationship than the positive correlation between credit score and increased delinquency predicted in Corbae and Glover (2024): the relationship between credit score and the effect of PECCBs on the probability of having an account past due is nonlinear, and while past-due accounts do increase in the highest credit score quintile, the largest increases occur in the middle of the credit score distribution rather than at the top. Furthermore, increases in bankruptcy are largest in the lowest credit score quintile and monotonically decrease as credit score rises.

6.2 Evidence on Mechanisms

The finding of an increased probability of bankruptcy could be driven by two potential mechanisms. First, the lower penalty for default when a PECCB is enacted could increase the likelihood of bankruptcy. Second, PECCBs could reduce the ability to repay debt among certain demographic groups who have been shown in the literature to face worse employment prospects as a result of PECCBs. Heterogeneity analysis by credit score and by demographic characteristics provides insight into whether the groups whose labor market outcomes worsen under PECCBs drive the overall increase in bankruptcy.

First, Ballance et al. (2020) finds that PECCBs improve labor market outcomes for individuals with the lowest risk scores and worsen labor market outcomes for those with mediocre risk scores. I find that the largest increase in the probability of bankruptcy in response to PECCBs occurs among individuals in the lowest credit score quintile, where labor market outcomes improve, rather than among consumers with mediocre credit scores.

Second, Ballance et al. (2020) find that PECCBs worsen labor market outcomes for individuals who are under 22. Figure 12 shows how the effect of PECCBs on the probability of bankruptcy varies by age. The estimated effect on consumers aged 18-25 is close to zero and statistically insignificant, with the largest increases in the probability of bankruptcy occurring among consumers aged 36-45 and 46-55. Therefore the overall increase in the probability of bankruptcy appears not to be driven by consumers in the age group where PECCBs worsen employment prospects.

Third, Ballance et al. (2020) and Bartik and Nelson (2024) provide evidence that PECCBs worsen employment prospects for Black job seekers. As race is not provided at the individual level in the credit bureau data, the percent of the county population that is Black is used to examine the relationship between race and the effect of PECCBs on bankruptcy. Counties are divided into quintiles for this analysis based on the percent of the county population that is Black in each year. This method avoids mischaracterizing counties with demographics that change substantially over time and also allows the cutoffs for each quintile to change as the national distribution shifts. Figure 13 shows the results. Estimated effects of PECCBs on bankruptcy are positive in all quintiles and statistically significant in quintiles 2, 3 and 5, with the largest point estimate in quintile 5. This estimate, a 1.4 percentage point increase in the probability of bankruptcy, is 56% larger than the average effect in the full sample. However, the estimate in the highest quintile is within the confidence interval of the estimate in the lowest quintile, and point estimates are not monotonically increasing in percent Black. While this heterogeneity analysis is limited by the lack of individual-level data on race, the findings suggest that the effects of PECCBs on employment prospects for Black individuals are not the primary mechanism driving the increased average probability of bankruptcy.

Figure 14 shows how the effects of PECCBs on the probability of bankruptcy vary by the percent of a county population that is Hispanic. Effect sizes increase as the percent of Hispanic residents rises: estimates are small or negative for the lowest three quintiles, while the probability of bankruptcy is estimated to increase by 0.9 percentage points in the fourth quintile and 1 percentage point in the fifth quintile. The estimated effects in the fourth and fifth quintiles are statistically significant, and the effect in the fifth quintile is larger in magnitude than the main result. While the literature does not provide evidence that PECCBs affect job prospects for Hispanic individuals, Miller (2015) provides an alternative explanation for why counties with larger shares of Black or Hispanic residents might experience larger increases in bankruptcy filings in response to PECCBs. Miller (2015) shows that counties with larger shares of racial and ethnic minorities generally see higher rates of bankruptcy filings due to social networks that transmit information about bankruptcy such as awareness of the benefits of bankruptcy and attorney recommendations. One possibility is that these social networks transmit information about the reduced cost of bankruptcy after a PECCB becomes effective. This mechanism may contribute to larger effect sizes in counties with higher shares of Hispanic or Black residents.

Overall, these findings suggest that the groups whose labor market outcomes are hurt by PECCBs are not the primary drivers of increased bankruptcy filings; in fact, the largest increase in the probability of bankruptcy estimated across all heterogeneity analyses is among individuals in the lowest credit score quintile, where labor market outcomes have been shown to improve. These results therefore suggest that the increase in bankruptcy is primarily a strategic response to PECCBs decreasing the cost of bankruptcy.

6.3 Additional Heterogeneity Analysis

Heterogeneity analysis by income and education provides additional insight into which types of individuals are most responsive to PECCBs. Fisher (2019) finds that in general, bankruptcy filers tend to be near the middle of the income distribution and that the typical bankruptcy filer has a terminal high school degree or some college. Heterogeneity by county-level median household income, shown in Figure 15, shows that PECCBs increase the probability of bankruptcy most in the middle quintile of the income distribution. Heterogeneity by the modal education level in the county, shown in Figure 16, shows that the largest increase in the probability of bankruptcy occurs in counties in which some college is the most common education level. These results suggest that PECCBs increase the probability of bankruptcy most in demographic groups that already have a relatively high propensity to file bankruptcy, rather than in the demographic groups where bankruptcy is rarest. However, these results are based on county-level rather than individual-level demographic data and should therefore be interpreted with caution.

Figure 17 shows the results of heterogeneity analysis by type of bankruptcy. Chapter 7 and Chapter 13 are the two common types of consumer bankruptcy. Two other types of bankruptcy occasionally appear on consumer credit reports: Chapter 11, which is typically used to reorganize business debts, and Chapter 12, which can only be filed by family farmers or family fishermen. Few individuals would qualify for Chapter 12 bankruptcy, and those who do would be unlikely to respond to employer credit check bans because they own their own businesses. Therefore Chapter 12 bankruptcy is unlikely to be affected by PECCBs. The probability of Chapter 11 bankruptcy is also unlikely to be affected because filing Chapter 11 bankruptcy as an individual is more complex and expensive than filing Chapter 7 or Chapter 13 (United States Courts (2025)), and individuals on the margin are therefore more likely to select Chapter 7 or Chapter 13 when the penalty for bankruptcy decreases. The estimates in Figure 17 confirm that the probabilities of Chapter 11 and Chapter 12 are unaffected by PECCBs, with tight null effects. The largest effect of PECCBs is on Chapter 7 bankruptcy filings, followed by Chapter 13. Effects on both of these types of personal bankruptcy are positive and statistically significant. These results provide additional evidence that PECCBs cause the overall increase in the probability of bankruptcy: a finding of substantial increases in Chapter 11 or Chapter 12 filings driving the results could raise doubts about whether PECCBs drove the increase in bankruptcy.

7 Robustness Checks

7.1 Permutation Test

While the number of observations in the sample is large, the number of treated clusters is fairly small. A small number of treated clusters raises the concern that inference may be unreliable because asymptotic assumptions typically used for inference may not hold. While a wild cluster bootstrap is often used to address this concern, it is ill-suited to this setting due to the combination of substantial variation in cluster sizes and a small number of treated clusters (MacKinnon and Webb 2017). As an alternative, I use a permutation test for inference. This type of permutation test has been used elsewhere in the literature on PECCBs (Bartik and Nelson 2024) and does not rely on asymptotic assumptions regarding the number of clusters. For each iteration of the permutation test, 12 states out of 51 are randomly selected as a placebo group of treated states. Each state in this placebo treatment group is randomly assigned a treatment year within the range of actual treatment years. The placebo treatment states are selected without replacement so that each iteration uses a group of 12 unique states, while the same placebo treatment year can be drawn for multiple placebo treatment states, mimicking the true set of treatment states and treatment years. Equation (1) is then estimated using the placebo treatment states and years. This procedure is repeated 1,000 times. Finally, a p-value is calculated based on the rank of the main result in the distribution of absolute values of estimates from the permutation test. The outcome used for the permutation test is the probability of any bankruptcy.

Figure 18 shows the distribution of estimates from the permutation test, with the red line indicating the value of the main result. The histogram has the most mass close to zero, and the mass diminishes quickly as estimates become more positive or negative. These characteristics of the distribution suggest that the main results do capture the effect of PECCBs, rather than noise in the data. The p-value from the permutation test is 0.024, which supports the conclusion that the main result is statistically significant and addresses the concern that inference may be unreliable due to the small number of treated clusters.

7.2 Heterogeneity by Percent Exempt

While I find support for the parallel trends assumption and plausible mechanisms can explain why PECCBs would increase the probability of bankruptcy, it remains possible that the main result is spurious: some other change in the treated states could have produced an increase in bankruptcy in the time period in which PECCBs were implemented. I take advantage of occupational exemptions from PECCBs to provide additional evidence that PECCBs drive the finding of increased bankruptcy. Each state that has implemented a PECCB has made certain occupations exempt, allowing employers to continue using information from an applicant's credit history for those occupations only. Exempt occupations are typically occupations that provide opportunities for embezzlement or fraud. For example, accountants and auditors are typically exempt. A list of SOC codes for exempt occupations in each treated state is shown in Table 1.

Because individual-level occupation information is not available in the credit bureau data, I use data on employment by SOC code from the Bureau of Labor Statistics. At the sub-state level, this data is only available by metropolitan statistical area (MSA) rather than county. Census Bureau county FIPS codes for each MSA were used to link the SOC employment data with the credit bureau data, which has a county identifier but does not include MSA. When an MSA spans multiple counties within a state, employment numbers are equally divided among the counties that make up the MSA. MSAs that span multiple states are excluded from the analysis. The final county-level data covers 868 counties out of 3,144 counties in the United States. In summary, this analysis has two limitations: occupation data is only available for geographic areas rather than individuals, and occupation data is not available for all geographic areas. Despite these limitations, this data is the best available and is therefore used for this robustness check.

Because the data is available only for geographic areas, the heterogeneity analysis uses variation in the share of employees in a county who are exempt from the PECCB rather than using a triple difference with individual-level variation as seen elsewhere in the literature on PECCBs (e.g., Cortés et al. (2022)). In the treated states, the percent exempt in each county is the percent of employees in the county who work in an occupation exempt from that state's ban. In the control states, the percent exempt is the percent of employees in the county who work in an occupation that is exempt from a ban in any treated state. As with other heterogeneity analyses based on county-level characteristics, counties are divided into quintiles based on the share of employment in exempt occupations in each year. Equation (1) is then estimated for each quintile. In quintile 1 almost all workers are affected by the PECCB, while in quintile 5 up to 20% of the population is exempt.

Figure 19 shows the results. Point estimates for quintiles 1-3 are all positive, statistically significant and larger in magnitude than the main result. The estimate in the fourth quintile is noisy, with a large standard error of 0.017, indicating more variation in the effect of the treatment in this quintile. While the large standard error creates difficulty in interpreting the result for the fourth quintile, this quintile has the first negative point estimate of -0.009. In the fifth quintile, where the share of individuals exempt from the ban is highest, the point estimate is negative at -0.01 and statistically significant. Overall, the pattern in Figure 19 shows that as the share of individuals affected by a PECCB decreases, the estimated increase in the probability of bankruptcy shrinks and even becomes negative in counties with the highest shares of exempt workers.

The results of this analysis help to address the concern that the increase in bankruptcy could be a spurious result rather than the effect of PECCBs. The heterogeneity analysis shows that the main finding of increased bankruptcy filings is driven by counties in which low shares of workers are exempt from the laws, and the estimates shrink as the share of workers who are unaffected by the law grows.

7.3 Robustness to Heterogeneous Treatment Effects

Recent advances in the literature on two-way fixed effects have shown that estimates can be biased in settings with staggered adoption when treatment effects are heterogeneous across groups or time periods (Roth et al. 2023). This literature especially highlights the concern that this bias can switch the sign of two-way fixed effects estimates, substantively changing researchers' interpretations of their results. I use the weighted stacked difference-in-differences (DID) estimator from Wing et al. (2024), which is unbiased in the presence of heterogeneous treatment effects in a staggered adoption setting, to address this concern.

The intuition for stacked regression is that it relies on "event time", the number of time periods since the treatment began, rather than calendar time, to transform a staggered adoption setting so that treatment occurs simultaneously in event time for all treated groups. The researcher constructs a stacked dataset in which groups that were treated at different calendar dates are aligned in event time (Cengiz et al. (2019), Butters et al. (2022), Deshpande and Li (2019)). The first step in constructing the stacked data is to create a separate dataset for each "sub-experiment", which consists of all units that were treated at the same calendar date, as well as never-treated units observed at the same calendar dates as those treated units. The sub-experiment dataset is trimmed to include a set number of time periods before and after treatment. The separate datasets for each sub-experiment are then vertically concatenated into a single dataset, which is used to estimate the stacked regression. Wing et al. (2024) introduce Q-weights to ensure that treatment and control group trends within each sub-experiment are weighted equally. With Q-weights in a weighted least squares regression, a researcher can estimate the trimmed aggregate average treatment effect on the treated (ATT), which is the causal parameter of interest.

To employ the weighted stacked DID estimator in this setting, I restrict event time in each sub-experiment dataset to include five years prior to treatment, the treatment year, and four years after treatment. Based on this rule, I drop Washington and Washington, D.C. from the analysis: Washington was treated in 2008 and only has three pre-treatment periods in the data, while Washington, D.C. was treated in 2018 and only has two post-treatment periods in the data. The ten remaining treated states are used to construct the stacked dataset following the procedure from Wing et al. (2024). The stacked dataset includes 67,983,167 observations, with the increased sample size arising from the use of the same group of never-treated states in multiple sub-experiment datasets. The weighted stacked DID regression estimated using this data is similar to the main specification, except that event time fixed effects are used instead of year fixed effects and the regression is weighted using Q-weights. Standard errors are clustered at the state level, as recommended with this method by Wing et al. (2024).

Table 4 shows the result. The weighted stacked DID estimate is a 0.6 percentage point increase in the probability of bankruptcy, which is close to the main two-way fixed effects estimate of 0.9 percentage points and includes the main two-way fixed effects estimate in its 95% confidence interval. The weighted stacked DID estimate is statistically significant at the 5% level. Figure 20 shows the weighted stacked DID event study for the probability of any bankruptcy. This event study shows point estimates that are statistically significant and close to zero in all pre-treatment periods, providing additional support for the plausibility of the parallel trends assumption. All post-treatment coefficients are positive and statistically significant.

8 Conclusion

This paper provides the first causal evidence of how consumers' debt repayment decisions change in response to PECCBs. PECCBs lower the cost of default by ensuring that a poor credit history will not be a barrier to employment. At the same time, by improving employment prospects for individuals with poor credit histories, these laws could reduce the likelihood of default by providing income to individuals struggling to repay their debts. The findings show that the effect of the lower penalty for default dominates the effect of any improvement in consumers' abilities to repay their debts, with PECCBs causing a 17.6% increase in the probability of bankruptcy. This result indicates that consumers making a strategic decision about filing for bankruptcy are sensitive to changes in the penalty for bankruptcy.

While the probabilities of having accounts 30, 60 or 90 days past due or in collections are unaffected on average, heterogeneity analysis shows increases in these outcomes for all consumers except those with the lowest credit scores, for whom the probability of these non-bankruptcy default measures decreases. For non-bankruptcy default measures, consumers with moderate to high credit scores respond to the decreased penalty for a poor credit history by increasing their probability of default. The behavior of individuals with the lowest credit scores is heterogeneous: one group of low-scoring individuals has a substantially increased probability of bankruptcy, while a separate group of low-scoring individuals improves their debt repayment. The improvement in debt repayment likely results from improved employment prospects in this group, which increase the ability to repay debt. Consumers in the middle and at the top of the credit score distribution are sensitive to the change in the cost of a poor credit history and have increased probabilities of past-due accounts and collections.

The credit market effects of PECCBs show that consumers are sensitive to changes in the cost of default. Negative consequences of a poor credit history can deter default, enabling lenders to trust that debts will be repaid. Policymakers should consider these results as they evaluate policies that reduce the cost of default, including additional state-level PECCBs. These results may also be useful to lenders predicting how default rates will respond to policies that reduce the cost of having a poor credit history. Credit reports are widely used, not only by lenders and employers but also by landlords, insurance companies, and utility companies. Exploring whether consumers respond similarly to changes in the use of credit reports by these other decision-makers is a promising avenue for future research. Future work could also explore how bankruptcy filings induced by PECCBs affect long-term financial well-being for filers, as well as lending terms and interest rates in treated states.

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9 Tables

Table 1: Treatment Timeline

State	Date Effective	Date Signed	Exempt SOC Codes
California	1/1/2012	10/9/2011	13, 23, 33
Colorado	7/1/2013	14/9/2013	13, 23, 33
Connecticut	10/1/2011	7/13/2011	13, 23, 33
Delaware	5/8/2014	5/8/2014	13, 23, 33
District of Columbia	10/1/2017	4/7/2017	11, 13, 23, 33
Hawaii	7/1/2009	7/16/2009	11, 13, 23, 33
Illinois	1/1/2011	8/10/2010	13, 23, 33
Maryland	10/1/2011	4/12/2011	13, 23, 33
Nevada	10/1/2013	6/13/2013	11, 13, 23, 33
Oregon	3/29/2010	3/29/2010	13, 23, 33
Vermont	7/1/2012	5/17/2012	13, 23, 33
Washington	7/22/2007	4/18/2007	13, 23, 33

Treatment dates and exempt SOC codes are from Cortés et al. (2022) except dates and SOC codes for Washington, D.C., which were hand-collected. Exempt SOC codes are codes for occupations exempt from PECCBs.

Table 2: Summary Statistics

	Control		Treated	
Variable	Mean	Std. Dev	Mean	Std. Dev
Credit Score (Range: 280-850)	574	281	586	281
Age	50	19	50	19
Total Past Due	$1,578	$19,325	$1,692	$14,118
Any Accounts 30 Days Past Due	0.033	0.18	0.029	0.17
Any Accounts 60 Days Past Due	0.017	0.13	0.015	0.12
Any Accounts 90 Days Past Due	0.012	0.11	0.011	0.1
Any Past Due	0.24	0.43	0.21	0.4
Any Collections	0.28	0.45	0.22	0.42
Any Bankruptcy	0.05	0.22	0.051	0.22
County Median Household Income	$64,032	$17,931	$73,459	$17,814
County Percent High School	30	7.1	24	5.6
County Percent Some College	28	4.7	29	4.7
County Percent College	18	6	20	5.4
County Percent White	78	16	71	16
County Percent Black	15	13	10	11
County Percent Hispanic	13	16	24	16
County Population	673,390	887,985	2,341,318	3,091,338

Control states are states that never passed a PECCB, while treated states implemented a PECCB. County-level demographics are from ACS 5-year estimates. Credit Score, age, and all financial outcomes are from credit bureau data. All data is from 2005-2019.

Main Results					
	Any Accounts 30 Days Past Due	**Any Accounts 60 Days Past Due**	**Any Accounts 90 Days Past Due**	**Any Collections**	**Any Bankruptcy**
Panel A: All Controls					
TWFE Estimate	0.000	-0.001	-0.001	-0.002	0.009**
	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
N	26,287,366	26,287,366	26,287,366	26,287,366	26,287,366
Panel B: Demographic + Bankruptcy Law Controls					
TWFE Estimate	0.000	0.000	-0.001	-0.001	0.008**
	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
N	28,575,143	28,575,143	28,575,143	28,575,143	28,575,143
Panel C: Fixed Effects Only					
TWFE Estimate	0.000	0.000	0.000	-0.001	0.010**
	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
N	28,575,143	28,575,143	28,575,143	28,575,143	28,575,143

*: p<0.1, **: p<0.05, ***: p<0.01. Clustered standard errors in parentheses. All regressions include year, state and individual fixed effects. Regressions in Panel A also include an individual-level age control, county-level demographic controls and state-level homestead exemption controls. Regressions in Panel B include demographic and homestead exemption controls but not the age control. Regressions in Panel C include all three sets of fixed effects but no additional controls. The age variable is unavailable for 2,287,777 individuals, leading to the smaller sample size in Panel A.

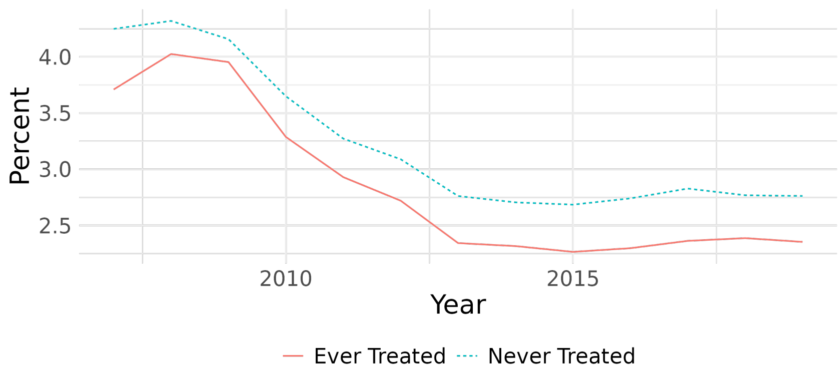
Table 3: Main Results

Weighted Stacked DID Results	
	Any Bankruptcy
Trimmed Aggregate ATT Estimate	0.006**
	(0.002)
N	67,983,167

*: p<0.1, **: p<0.05, ***: p<0.01. Standard errors clustered at the state-by-sub-experiment level in parentheses. Regression includes event time, state and individual fixed effects as well as an individual-level age control, county-level demographic controls and state-level homestead exemption controls. Regression estimated using the weighted stacked DID approach from Wing et al. (2024). The large sample sizes arises from construction of the stacked data set, described in detail in the Robustness Checks section.

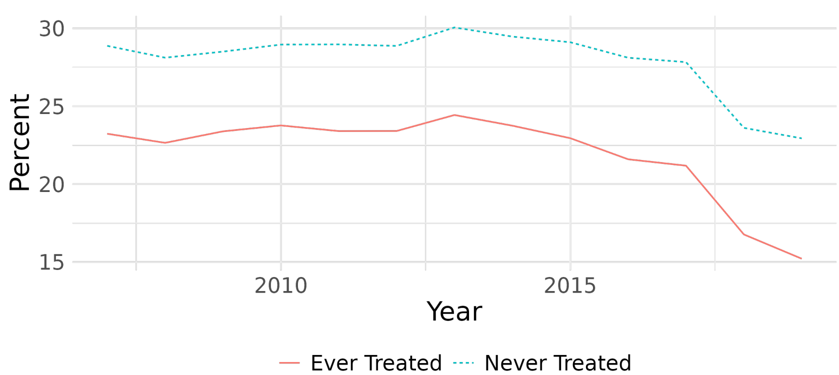
Table 4: Weighted Stacked DID Estimate

10 Figures



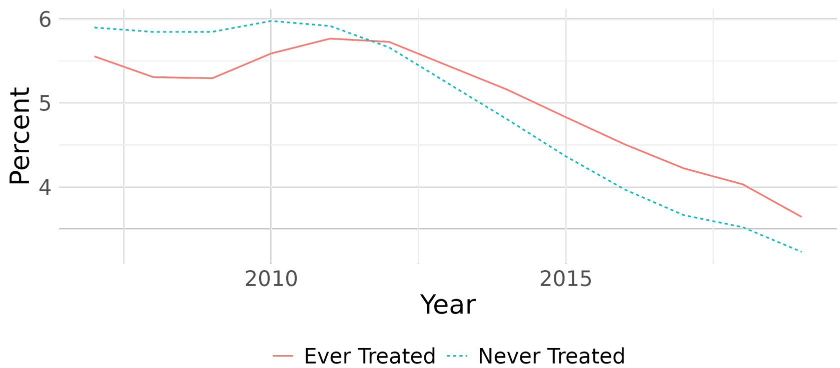
Averages calculated using credit bureau data. Ever treated states are states that ever passed a PECCB, while never treated states never passed a PECCB.

Figure 1: Percent with Any Accounts 30 Days Past Due



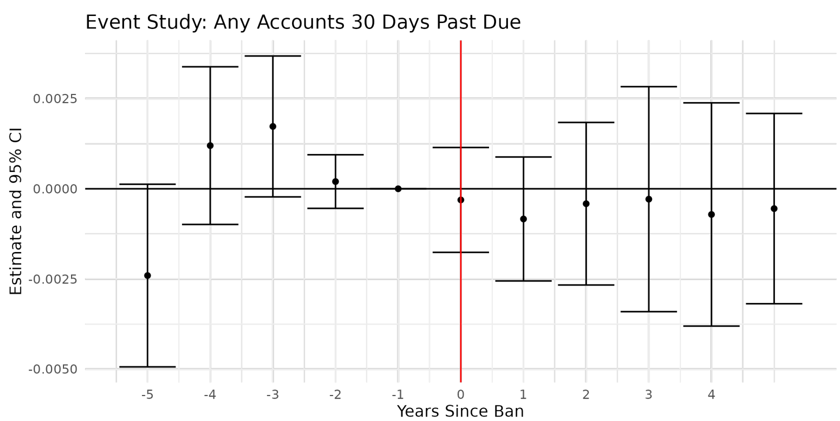
Averages calculated using credit bureau data. Ever treated states are states that ever passed a PECCB, while never treated states never passed a PECCB.

Figure 2: Percent with Any Collections



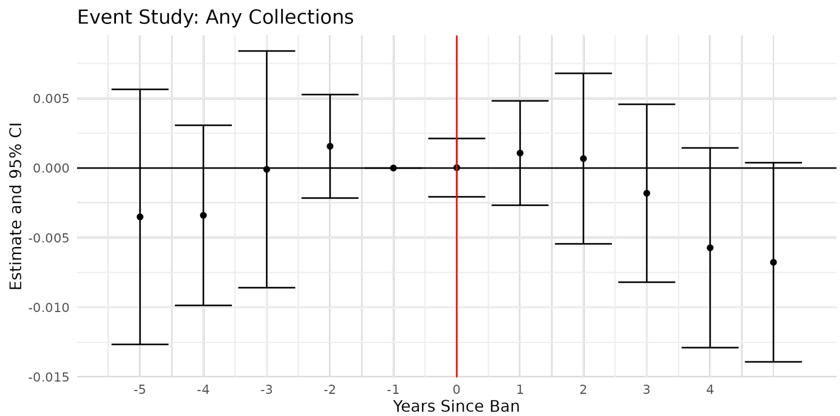
Averages calculated using credit bureau data. Ever treated states are states that ever passed a PECCB, while never treated states never passed a PECCB.

Figure 3: Percent with Any Bankruptcies



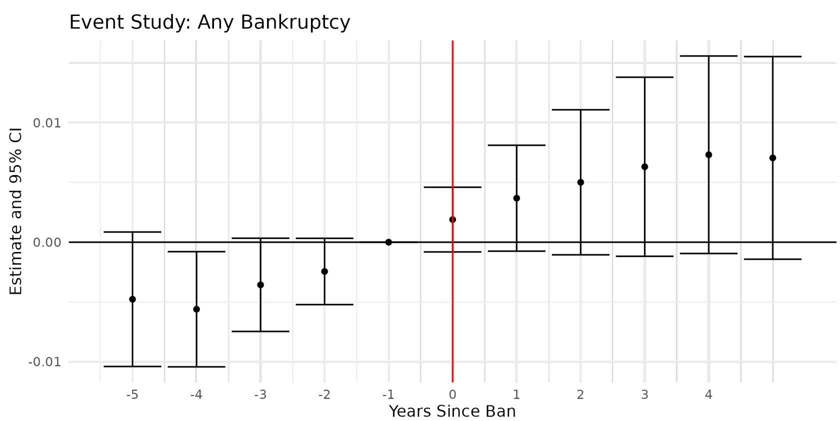
The outcome is the probability of having any accounts 30 days past due. The regression includes controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 4: Event Study: Any Accounts 30 Days Past Due



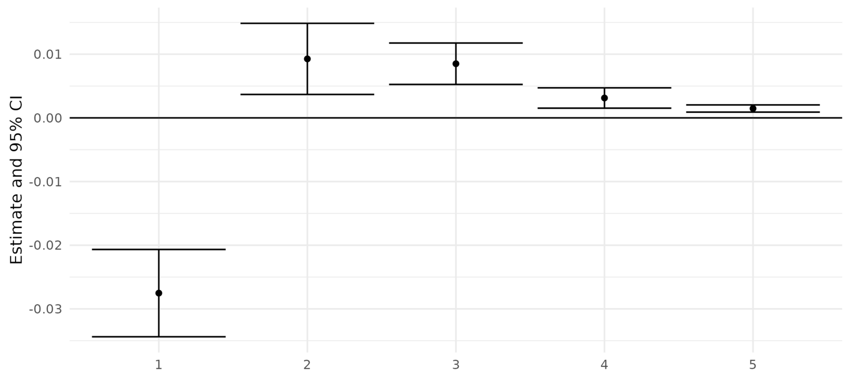
The outcome is the probability of having any accounts in collections. The regression includes controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 5: Event Study: Any Collections



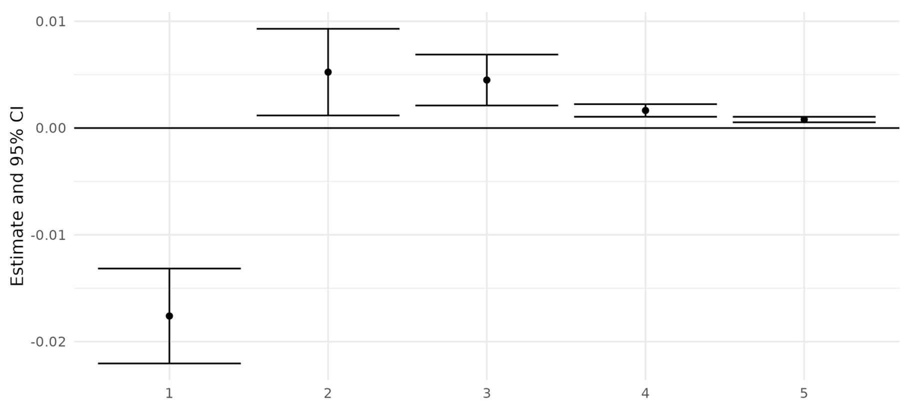
The outcome is the probability of any bankruptcy. The regression includes controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 6: Event Study: Any Bankruptcy



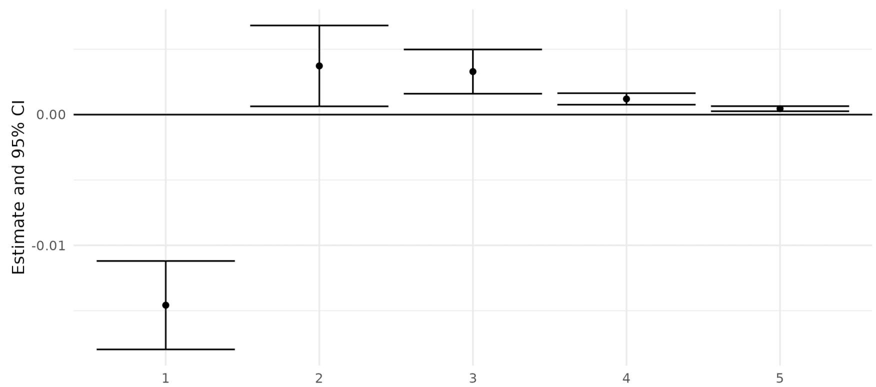
The outcome is the probability of having any accounts 30 days past due. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 7: Heterogeneity by Credit Score Quintile: Any Accounts 30 Days Past Due



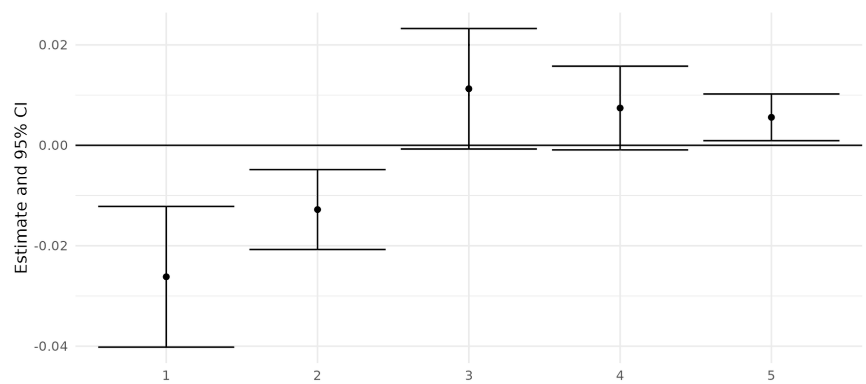
The outcome is the probability of having any accounts 60 days past due. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 8: Heterogeneity by Credit Score Quintile: Any Accounts 60 Days Past Due



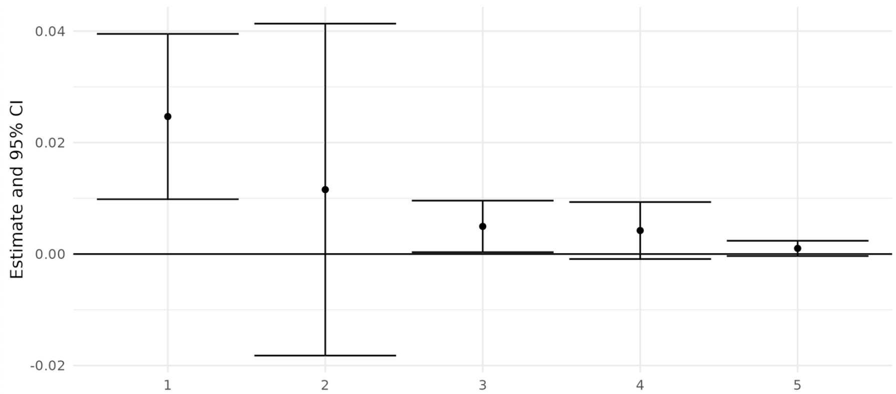
The outcome is the probability of having any accounts 90 days past due. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 9: Heterogeneity by Credit Score Quintile: Any Accounts 90 Days Past Due



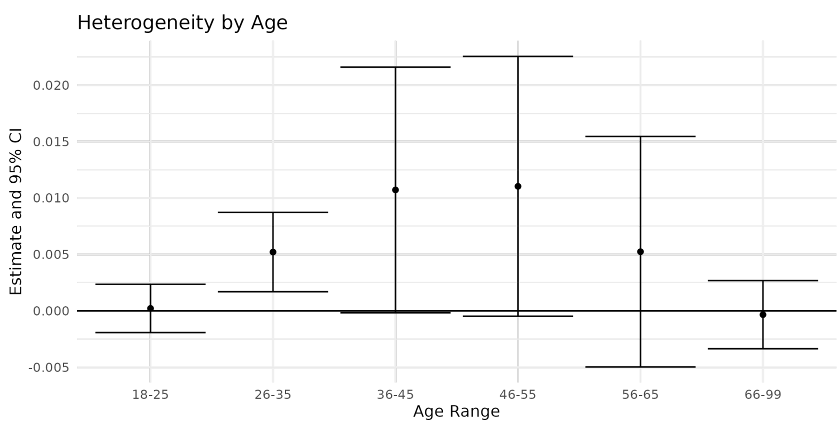
The outcome is the probability of having any collections. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 10: Heterogeneity by Credit Score Quintile: Any Collections



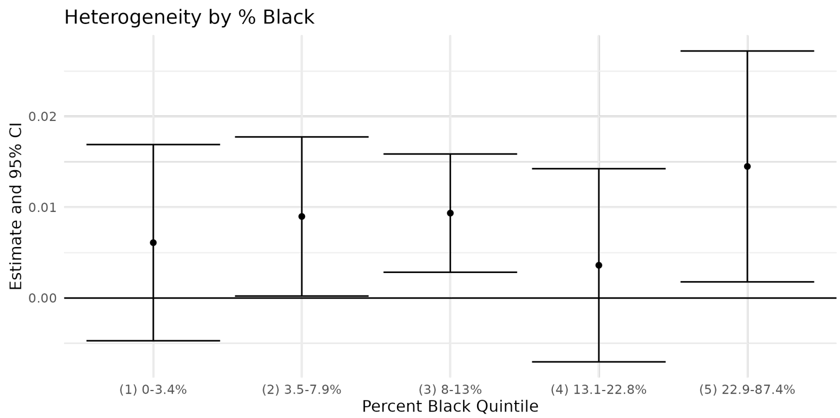
The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 11: Heterogeneity by Credit Score Quintile: Bankruptcy



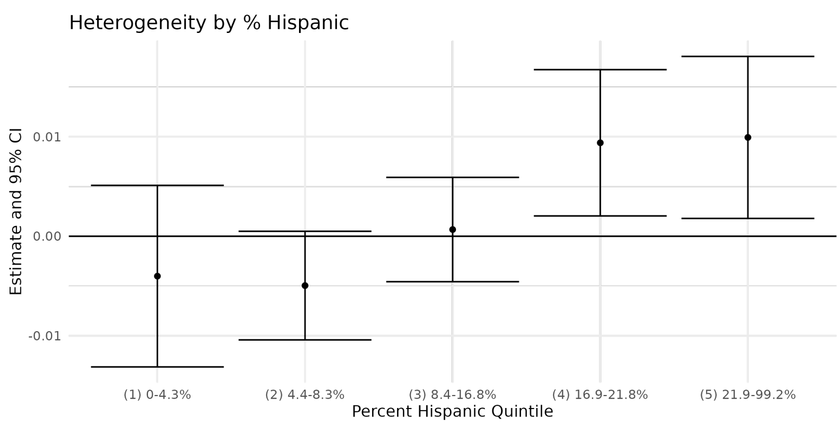
The outcome is the probability of any bankruptcy. All regressions include controls for county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 12: Heterogeneity by Age



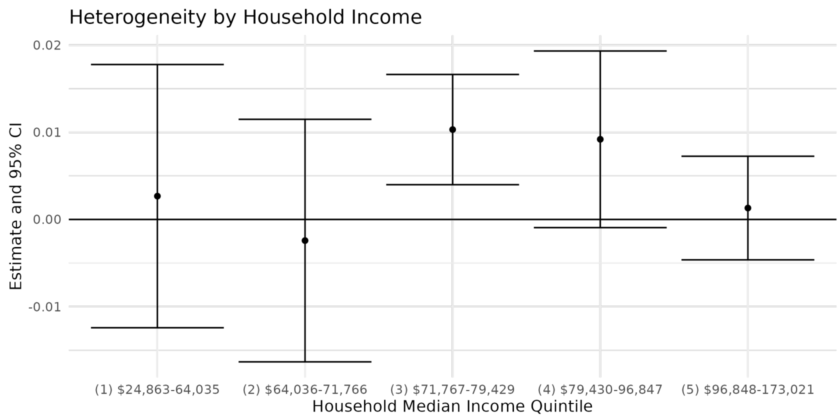
The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. To provide context for the reader, quintiles are labeled with the range used to define that quintile in 2019.

Figure 13: Heterogeneity by Percent Black



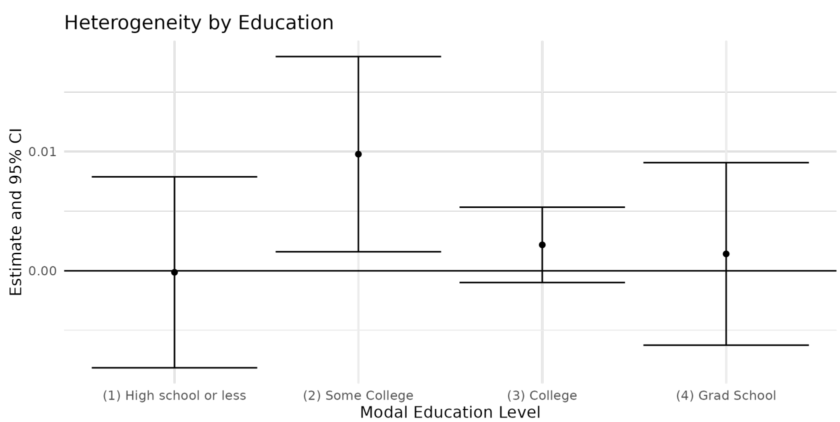
The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. To provide context for the reader, quintiles are labeled with the range used to define that quintile in 2019.

Figure 14: Heterogeneity by Percent Hispanic



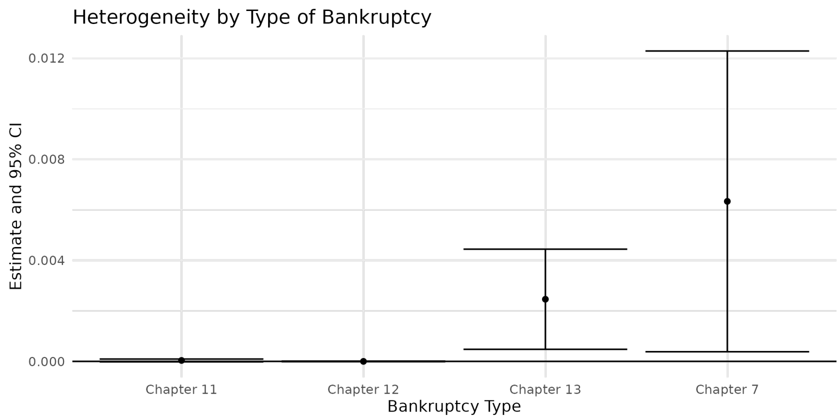
The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. To provide context for the reader, quintiles are labeled with the range used to define that quintile in 2019.

Figure 15: Heterogeneity by Median Income



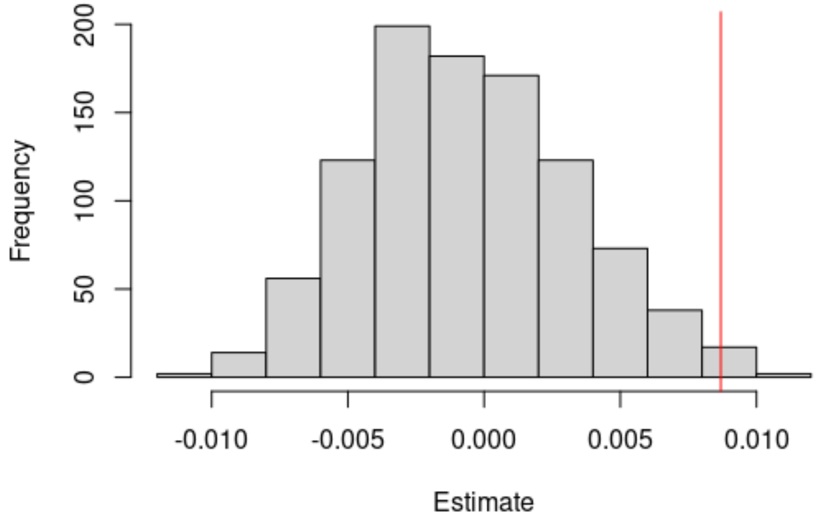
The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 16: Heterogeneity by Education



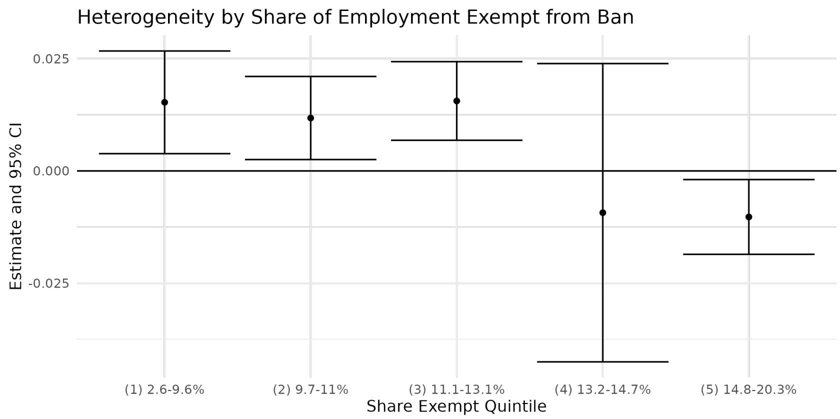
The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 17: Heterogeneity by Type of Bankruptcy



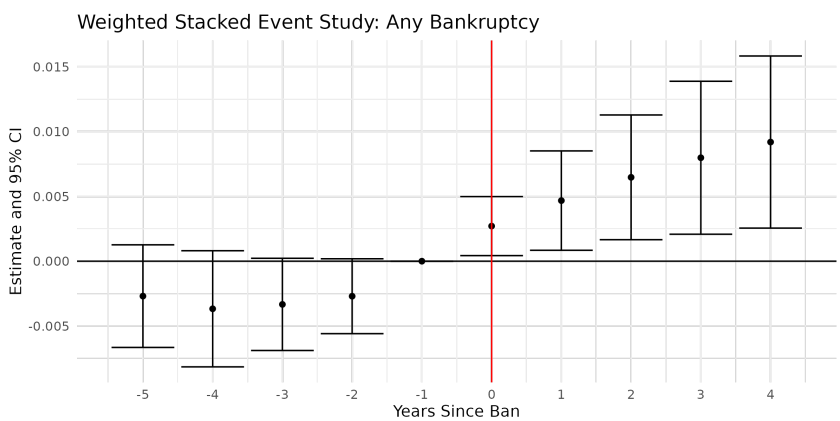
This histogram shows the distribution of estimates from the permutation test. Each estimate is the two-way fixed effects estimate from a regression using 12 randomly selected placebo treatment states, with each assigned a randomly selected placebo treatment year in the range of the actual treatment years. The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. The vertical line shows the TWFE estimate using actual treated states and years.

Figure 18: Histogram of Permutation Test Estimates



The outcome is the probability of any bankruptcy. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level.

Figure 19: Heterogeneity by Share Exempt



The outcome is the probability of any bankruptcy. The regression includes controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. The event study is estimated using the weighted stacked DID method from Wing et al. (2024).

Figure 20: Weighted Stacked Event Study: Any Bankruptcy

11 Appendix

11.1 Logistic Regression Results

Results using a logistic regression instead of a linear probability model are very similar to the main results. Table 5 shows estimated effects of PECCBs using a logistic regression. PECCBs are estimated to increase the odds of bankruptcy by 46.5%. This estimate is economically large and is significant at the 5% level. Estimated effects on the odds of having any any accounts 30 days past due, 60 days past due, 90 days past due, or in collections are smaller in magnitude than the estimated effect on the odds of bankruptcy: odds do not change by more than 8.3% for any of these non-bankruptcy outcomes. Point estimates for the non-bankruptcy outcomes are also negative, and most are statistically insignificant. The logistic regression results share many similarities with the linear probability model: a large, statistically significant increase in bankruptcy and smaller, statistically insignificant, negative point estimates for the non-bankruptcy outcomes. One difference is that the estimated effect on the odds of having any accounts 90 days past due is statistically significant with the logistic regression, while it is insignificant with the linear probability model.

Logit Results

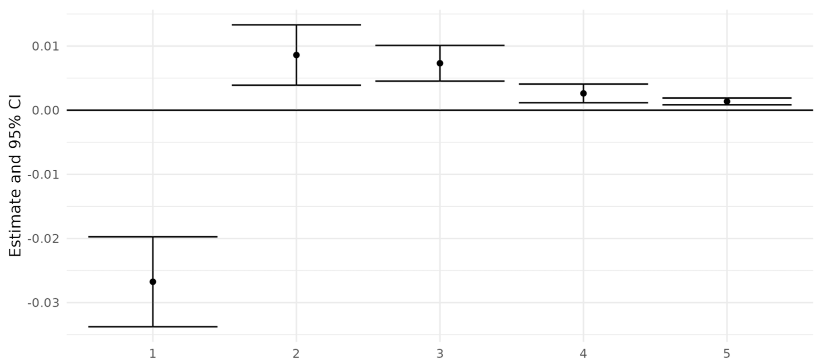
Outcome	**Coefficient**	**Std. Error**	**Percent Change in Odds**
Any Accounts 30 Days Past Due	-0.041	0.044	-3.98%
Any Accounts 60 Days Past Due	-0.064	0.042	-6.16%
Any Accounts 90 Days Past Due	-0.087**	0.043	-8.30%
Any Collections	-0.023	0.042	-2.27%
Any Bankruptcy	0.382**	0.15	46.54%

*: p<0.1, **: p<0.05, ***: p<0.01. Coefficients estimated using logistic regression. Clustered standard errors in parentheses. All regressions include year, state and individual fixed effects, an individual-level age control, county-level demographic controls and state-level homestead exemption controls.

Table 5: Logit Results

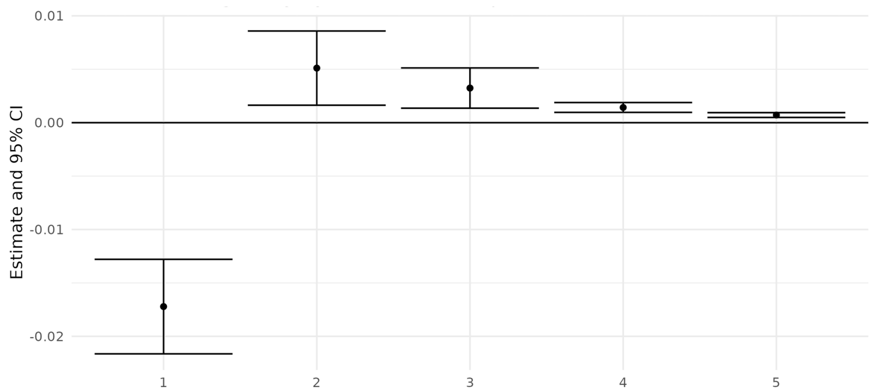
11.2 Heterogeneity by Credit Score Excluding Consumers with Bankruptcies

The figures below show the results of heterogeneity analysis by credit score for the four non-bankruptcy outcomes with consumers who have ever filed bankruptcy excluded from the sample. The purpose of this exercise is to determine whether the effects on these outcomes, in particular the decrease in non-bankruptcy financial distress measures among low-scoring individuals, are the result of filing for bankruptcy or a different effect on a separate set of individuals. The results when individuals who have ever filed bankruptcy are excluded from the sample are nearly identical to the results with those individuals included in the sample, indicating that the effects on non-bankruptcy financial distress measures are not driven by the effects of bankruptcy on those measures.



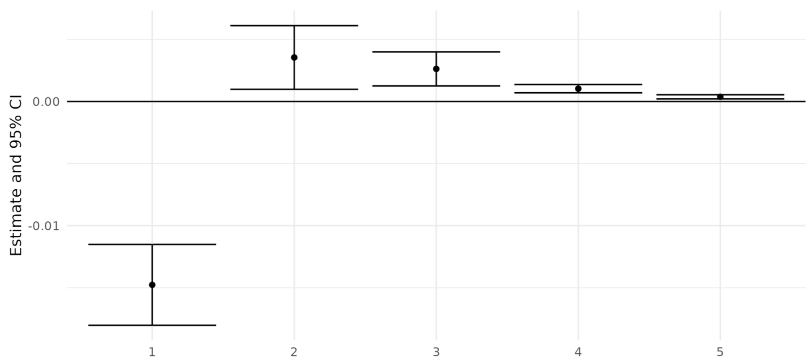
The outcome is the probability of having any accounts 30 days past due. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. All consumers who have ever filed bankruptcy have been removed from the sample.

Figure 21: Heterogeneity by Credit Score Quintile: Any Accounts 30 Days Past Due



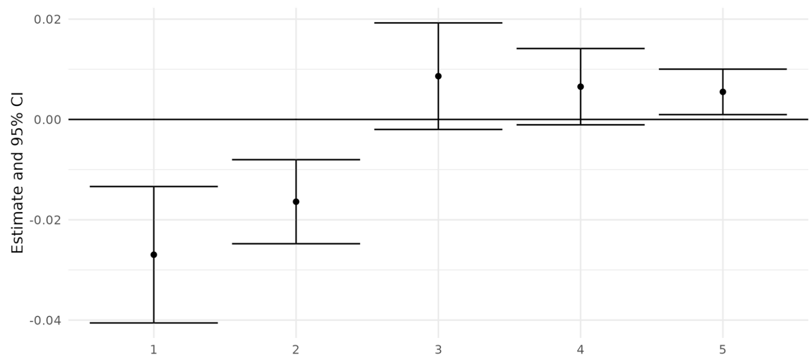
The outcome is the probability of having any accounts 60 days past due. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. All consumers who have ever filed bankruptcy have been removed from the sample.

Figure 22: Heterogeneity by Credit Score Quintile: Any Accounts 60 Days Past Due



The outcome is the probability of having any accounts 90 days past due. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. All consumers who have ever filed bankruptcy have been removed from the sample.

Figure 23: Heterogeneity by Credit Score Quintile: Any Accounts 90 Days Past Due



The outcome is the probability of having any collections. All regressions include controls for age and county demographics, as well as state, year and individual fixed effects. Standard errors are clustered at the state level. All consumers who have ever filed bankruptcy have been removed from the sample.

Figure 24: Heterogeneity by Credit Score Quintile: Any Collections

11.3 Results Excluding Washington, D.C.

Washington, D.C. differs from other treated states in that it is technically not a state, and consists of only one city. I therefore repeat the analysis using a sample that excludes Washington, D.C. Table 6 shows that removing Washington, D.C. slightly reduces the sample size from 26,287,366 to 26,233,166. When rounded to three decimal places, the point estimate and standard error estimated using this sample are identical to those obtained with the full sample, showing that any unique characteristics of Washington, D.C. which differ from the other treated states do not drive the main results.

	Any Bankruptcy
TWFE Estimate	0.009**
	(0.004)
N	26,233,166

Note:
*: p<0.1, **:p<0.05, ***: p<0.01. Standard errors clustered at the state level in parentheses. The regression includes state, year and individual fixed effects, as well as state-level demographic controls and an individual-level control for age. Washington, D.C. has been dropped from the sample for this analysis.

Table 6: Excluding Washington, D.C.

11.4 Additional Robustness to Heterogeneous Treatment Effects

In addition to the weighted stacked DID estimator from Wing et al. 2024, I use two other estimators that are robust to heterogeneous treatment effects as additional robustness checks. Results using both of these alternative estimators should be interpreted with caution, as computational constraints arising from the large size of the dataset did not allow the use of individual fixed effects with these two alternative estimators. All other fixed effects and controls are the same as in Equation (1).

First, I use the two-stage difference-in-differences (two-stage DID) estimator from Gardner (2022). As the name suggests, this estimator uses a two-step procedure. First, the outcome is regressed on group and period fixed effects using a sample of only untreated observations. The estimated group and period fixed effects are then subtracted from the outcome, producing an adjusted outcome that is regressed on an indicator for treatment in the second stage. A generalized method of moments asymptotic framework is used for inference. This procedure identifies the average treatment effect on the treated even in the presence of treatment effect heterogeneity.

Second, I use the multiperiod difference-in-differences (DIDM) estimator from de Chaisemartin and D'Haultfœuille (2020). For each pair of consecutive time periods, the DIDM estimator estimates the average treatment effect across all groups whose treatment status changed between those time periods. This estimator can be used in any setting in which there are groups whose treatment status remains constant in each pair of consecutive time periods, a condition which is satisfied in this setting.

An important note in interpreting results from these alternative estimators is that each of the three estimators (two-way fixed effects, DIDM, and two-stage DID) estimates a different estimand. Therefore, even in the absence of bias from heterogeneous treatment effects, we would not expect these three estimators to produce exactly the same point estimates. However, the two alternative methods can provide insight into whether bias from heterogeneous treatment effects leads to a substantive difference in interpretation of the results.

Table 7 shows results using these two alternative estimators. Both estimates are positive and include the main two-way fixed effects estimate in their 95% confidence interval. The point estimate using two-stage DID is a 0.45 percentage point increase in the probability of filing for bankruptcy, while the point estimate using the DIDM estimator is a 0.36 percentage point increase in the probability of filing for bankruptcy. The estimates using these two methods that are robust to heterogeneous treatment effects indicate that in this setting, bias from heterogeneous treatment effects does not have a substantial effect on either the sign or magnitude of the results. Due to the similarity between the two-way fixed effects estimate, the results from these two alternative estimators, and the estimate from the weighted stacked DID estimator, the two-way fixed effects estimate is used as the main result of the paper.

Table 7: Robustness to Heterogeneous Treatment Effects

Method	**Estimate**	**Std. Error**	**95% Confidence Interval**
Two-Stage DID	0.0045	0.0038	[-0.0029, 0.0119]
DIDM	0.0036	0.0035	[-0.0033, 0.0105]

Two-Stage DID is the two-stage difference-in-differences method from Gardner (2022). DIDM is the multiperiod difference-in-differences estimator from de Chaisemartin and d'Haultfoeuille (2020). Both methods are robust to heterogeneous treatment effects. Regressions include controls for age and county demographics, as well as state and year fixed effects. Individual fixed effects are included for Two-Stage DID but not for DIDM.