Less Mainstream Credit, More Payday Borrowing?

Evidence from Debt Collection Restrictions^{*}

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Abstract

Governments regulate debt collectors to protect consumers from predatory practices. These restrictions may lower repayment, reducing the supply of mainstream credit and increasing demand for alternative credit. Using individual credit record data and a difference-in-differences design comparing consumers in states that tighten restrictions on debt collection to those in neighboring states that do not, I find that restricting collections reduces access to mainstream credit and increases payday borrowing. These findings provide new evidence of substitution between alternative and mainstream credit and point to a trade-off between shielding consumers from certain collection practices and pushing them into higher-cost payday lending markets.

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1 Introduction

Financial distress is pervasive among American consumers. As of December 2016, over \$600 billion of outstanding household debt was past due and two thirds of those balances were at least 90 days late (FRBNY 2016). When faced with unmet payments, creditors turn to debt collectors to minimize their losses. This practice is widespread and collection agencies recovered nearly \$79 billion from millions of consumers in 2016 (ACA International 2017).

Given their role in extracting payments from borrowers, often through litigation, it is unsurprising that the debt collection industry has attracted intense regulatory scrutiny. Between 2000 and 2015, US states imposed new restrictions on debt collection practices every year. While this legislation has the important goal of protecting consumers from abusive collection practices, it can also have unintended consequences for consumer credit markets, such as a reduction in the supply of credit. Moreover, as consumers lose access to mainstream sources of credit, they may turn to alternative and more expensive financial services, such as payday loans.

This paper studies the impact of debt collection on access to mainstream credit and payday loan usage. It does so using data on the balance sheets of two million borrowers across the US, with information on credit balances, payment history, credit scores, and derogatory credit events such as bankruptcies. Importantly, I link mainstream credit registry data with data on alternative credit, such as high-cost payday loans, to investigate the substitution effect between mainstream and alternative sources of credit.

To evaluate the causal effect of debt collection practices, I exploit time-series variation in the restrictiveness of state-level legislation regarding debt collection (Fedaseyeu 2020). I use 32 regulatory changes that restricted collection practices across 20 states and exploit policy discontinuities at state borders. This empirical strategy uses variation in legislation within pairs of counties that share a border but are located in different states (Holmes 1998; Huang 2008; Dube et al. 2010), and thus compares consumers exposed to the same local economic conditions. Specifically, I compare outcomes of consumers in a state that introduced restrictive legislation with outcomes of consumers in a contiguous county in another state, which are not subject to the same state-level legislation, before and after the legislation change. I start by showing that these legislation changes meaningfully impact the collection industry at the state level, with states that restrict debt collection practices seeing a drop in the number of debt collectors of approximately 256 collectors, or 10 percent of the sample mean.

This paper has three main sets of findings. First, borrowers see a decline in access to mainstream credit, lower credit scores, and higher past-due balances as a consequence of legislation that restricts debt collection practices. I estimate that credit balances of low-income borrowers in states that restrict collection practices decline by \$1,200, or approximately 9 percent of the sample mean, relative to consumers in contiguous counties in other states. I also find that these consumers experience lower credit limits, higher credit usage, and fewer new accounts per inquiries, consistent with a reduction in the supply of credit rather than lower demand. These results are consistent with debt collection having a role in the enforcement of credit contracts and with work that establishes a link between creditor rights and financial development (Porta et al. 1998; Levine 1998; Djankov et al. 2007).

Restricting debt collection practices also leads to lower credit scores and higher balances past due. I estimate that low-income borrowers in states that restrict collection practices see a decline of nearly 3 points (0.4 percent of sample mean) in their credit scores and an increase of \$38 (19 percent of sample mean) in balances past due. The rise in past-due balances is entirely driven by an increase in late-stage delinquencies, which are the accounts that are usually placed with collectors. This suggests that collection restriction lead to a contraction in mainstream credit by making credit contracts less enforceable.

Second, payday borrowing increases when collection activities are restricted and the supply of mainstream credit declines. I estimate an increase of 7 payday loans for every 1,000 consumers—a sizable 70 percent of the sample mean—in states that restrict collection practices, relative to consumers in contiguous counties located in other states. This increase is substantially larger for borrowers who already borrowed from payday lenders prior to legislation changes and for those with limited availability of mainstream credit, consistent with a pecking order theory of consumer financing (Lusardi et al. 2011).

Third, I use changes to state-level debt collection legislation as an instrument for access to mainmainstream credit and estimate the elasticity of payday borrowing with respect to mainstream credit. I find that this elasticity is small overall, but very large for prior payday borrowers and those who are mainstream-credit constrained. I find that prior payday borrowers with less than \$300 in available revolving credit increase payday borrowing by 0.64% when access to mainstream credit declines by 1%. To the best of my knowledge, this work is the first to document that payday borrowing rises as access to mainstream credit declines and provides one of the first measures of the elasticity of substitution between alternative and mainstream sources of credit.

The key identifying assumption behind this empirical strategy is that, in the absence of debt collection restrictions, outcomes for borrowers in the treatment and control groups would have evolved according to parallel trends. I provide evidence for this assumption by analyzing outcomes for the treatment and control groups prior to the legislation change, and find no indication of pre-existing trends. I also show that results are robust to a specification that controls for the distance to the border, which allows for a more precise comparison of the average difference in outcomes at the border. I also verify that results are robust to different empirical specifications, such as having outcome variables in logs instead of levels and using only the first legislation change introduced by a state. I also show that results are robust to excluding states that loosened restrictions on debt collectors during the sample period and states that banned payday loans around the time that debt collection restrictions were introduced.

Moreover, a crucial assumption behind the instrumental variable analysis is that collection

restrictions affect payday loans only through their effect on mainstream credit. I provide support for this assumption through evidence that, unlike mainstream lenders, payday lenders do not usually rely on third-party debt collectors. I conduct a manual review of all public enforcement actions against a payday lender for debt collection violations and show that, in nearly every instance, payday lenders did not rely on third-party collectors. I supplement this evidence with an original survey of members of a trade association of third-party debt collectors and debt buyers, the vast majority of which report that little or none of their portfolio of receivables corresponds to payday loans. Finally, if collection restrictions directly affect payday loans, we would expect to see an increase in payday loan defaults following debt collection restrictions, like we observe with mainstream loans. Consistent with the hypothesis that collection restrictions have no direct effect on payday loans, I find that borrowers are no more likely to default on payday loans after third-party debt collection is restricted.

Literature Review. Despite intense regulatory scrutiny, the literature on debt collection remains small. Dawsey et al. (2013) documents that consumers are less likely to file for bankruptcy in states with legislation that grants them private right of action against abusive in-house collection practices. Fedaseyeu and Hunt (2018) proposes a model in which third-party debt collection agencies will employ harsher practices in equilibrium than what creditors would use themselves due to reputational concerns. Fedaseyeu (2020) analyses nearly all of the changes to debt collection regulation studied in this paper and finds that they lead to lower recovery rates, fewer new revolving lines of credit, and fewer consumer lawsuits at the state level. Romeo and Sandler (2021) analyzes debt collection regulations in four states and finds that they lead to a small decline in new credit card accounts and a small increase in interest rates. Cheng et al. (2020) analyzes civil collection lawsuits and finds that settling out of court does not significantly improve consumers' access to credit or reduce financial distress relative to wage garnishment. Within the debt collection literature, this study is closest to Fedaseyeu (2020). The key contribution of the current study relative to this existing work is using novel data linking mainstream credit records with payday loan records to study the effect of debt collection restrictions on payday borrowing. I also provide one of the first measures of the elasticity of payday borrowing with respect to mainstream credit by using debt collection restrictions as an instrument for access to mainstream credit. I also complement this prior analysis by using a county-border discontinuity design to estimate the effect of debt collection restrictions on mainstream credit, which helps account for the unobserved variation across states in the state-level differences-in-differences research design in Fedaseyeu (2020). Our conclusions regarding mainstream credit are broadly consistent, although I estimate statistically significant declines in revolving balances and credit scores, while Fedaseyeu (2020) finds an increase in revolving balances and no significant change in credit score. My estimates on revolving balances and credit scores are consistent with other findings in Fedaseyeu (2020), such as the fact that new revolving credit lines decrease—which, all else equal, would lead to lower balances—and that past-due balances rise, which should lead to lower credit scores.

This study is also closely related to concurrent work by Miller and Soo (2020). Miller and Soo (2020) estimates the effect of bankruptcy flag removal on payday borrowing and find that flag removal, which increases credit limits, has no significant effect on payday borrowing. There are two key reasons why my results could differ from Miller and Soo (2020). First, they analyze a positive shock to mainstream credit access while I focus on a negative shock to access, and it is possible that the substitution effect between mainstream and alternative credit is asymmetric. Second, their sample consists of consumers who have filed for Chapter 7 bankruptcy and had a bankruptcy flag removed from their record, while mine is a representative sample of borrowers with mainstream credit records linked to payday borrowing records. Even among payday borrowers, bankruptcy filers have higher income and lower debt-to-income ratios, which can potentially explain different responses to changes in access to mainstream credit. This paper is also related to works that find evidence consistent with the "pecking order theory" of consumer sources of financing proposed by Lusardi et al. (2011), which predicts that consumers will draw on savings in the event of an unexpected expense, followed by mainstream credit and/or borrowing from family and friends, followed by more expensive sources of credit, such as payday loans. This is consistent with the findings of Dupas et al. (2017) and Kast and Pomeranz (2021), who find that access to savings accounts reduces borrowing, and Célerier and Matray (2019), who shows that access to a bank account reduces financial strain in the face of negative income shocks. This theory is also consistent with evidence that consumers turn to sources of financing that are even costlier than payday loans following payday bans, such as overdrafts and pawnshop loans. Morgan et al. (2012) finds that payday bans lead to an increase in overdraft fees and returned checks. Melzer and Morgan (2015) also analyzes payday lending restrictions and finds that banks and credit unions reduce overdraft credit limits and prices when payday lending is prohibited. Bhutta et al. (2016) finds that payday loan restrictions cause consumers to shift to other forms of high-interest credit, such as pawnshop loans, rather than mainstream credit. Relatedly, Di Maggio et al. (2020) shows that payday borrowing declines as a consequence of a reduction in costs associated with obtaining overdraft credit.

This paper contributes to this literature by focusing on an understudied rung in the pecking order ladder: the substitution from mainstream credit to payday loans. My findings also help explain why prior works do not find that consumers turn to mainstream credit following payday loan bans (Bhutta et al. 2016). I find that borrowers who turn to payday loans are those with limited access to mainstream credit, which means these borrowers might be unable to go up the ladder toward mainstream credit once access to payday loans are restricted, only down toward even more expensive sources of credit.

Finally, this research also relates to the literature that studies the effect of personal bankruptcy protection on the availability and cost of consumer credit. Gropp et al. (1997) finds that

higher protection in bankruptcy is associated with lower access to credit. Lin and White (2001) shows that mortgage application acceptance is negatively correlated with the level of bankruptcy protection. Severino and Brown (2020) uses changes in the level of bankruptcy protection across US states and finds that bankruptcy protection laws increase borrowers' holdings of unsecured credit. Gross et al. (2021) studies the Bankruptcy Abuse Prevention and Consumer Protection Act and finds that lower bankruptcy filling risk decreases credit card interest rates. This paper adds to this body of work by analyzing the impact of policies regarding debt collection practices, which are another feature of credit markets relating to creditor rights, on access to consumer credit. I also take a broad approach to analyzing access to credit in this setting and focus on both mainstream and alternative sources of credit.

The remainder of this paper is structured as follows. Section 2 describes institutional details of the debt collection industry and the data used in the analysis. Section 3 describes the theories that guide the empirical work. Section 4 details the empirical strategy. Section 5 reports results and evaluates their robustness. Section 6 concludes.

2 Background and Data

2.1 Debt Collection

Debt collection practices are prevalent and, between 2004 and 2016, an average of 13 percent of consumers had at least one account in collections (FRBNY 2016). Mainstream lenders often initially turn to in-house collection departments, but usually rely on debt collection firms, often referred to as third-party debt collectors, or to debt buyers for debt that is more than 90 days late. According to the CFPB, the majority of credit card issuers who were unable to collect on a debt will eventually turn to third-party debt collectors (CFBP 2015). In 2016, third-party debt collectors recovered approximately \$78.5 billion from consumers, earning \$10.9 billion in commissions and fees and returning \$67.6 billion to creditors (ACA International 2017). The main types of debts that this industry collects on are medical debt, student loans, and credit card or other debts related to financial services.

Third-party collectors usually rely on contacting consumers and attempting to negotiate a repayment plan, and name establishing contact with consumers as one of their main challenges (TransUnion 2019). Debt collectors can also resort to litigation and, if successful, are able to garnish the borrower's future wages. In a survey conducted by the Consumer Financial Protection Bureau (CFPB), 32 percent of borrowers reported being contacted by a debt collector and 15 percent of borrowers with a debt collection experience reported being sued by a collector during the preceding year (CFBP 2017). Unsurprisingly, debt collection practices have attracted the attention of legislators and policymakers and, according to the CFPB, no other industry has generated more consumer complaints (CFBP 2014b). This has resulted in intense regulatory activity, both by the CFPB and state legislators, and in attempts by the industry to self-regulate.¹

At the federal level, debt collection practices are governed by the Fair Debt Collection Practices Act (FDCPA), which was instituted in 1977.² The FDCPA prohibits harassment, misrepresentation, and what it defines as "unfair practices," such as the collection of any amount not expressly authorized by the contract that originated the debt or threatening legal action when it is not permitted by law or when there is no present intention of such action. The FDCPA regulates the action of third-party debt collectors and of debt buyers who engage in debt collection, but, in general, not the practices of original creditors collecting their own debt. Furthermore, the FDCPA explicitly allows states to impose further regulation on debt collection practices, as long as the protection afforded to consumers is greater than what is provided by federal law. The CFPB also issued new federal rules in 2020, which will go into effect in November 2021, to "[...] restate and clarify prohibitions on harassment and abuse,

 $^{^{1}}$ See, for instance, the certification program of the Receivables Management Association International (RMAI), a trade association of debt collectors and debt buyers, that seeks to impose standards of best practices among its members (https://rmaintl.org/certification/).

²15 U.S.C. §§ 1692-1692p.

false or misleading representations, and unfair practices by debt collectors when collecting consumer debt."³

State-level regulations on debt collection practices have been adopted by 43 states. These law changes consist of imposing or tightening licensing or bond requirements, raising civil or administrative penalties for violations of debt collection laws, changing how third-party debt collectors can be prosecuted by the state and what private remedies are available to consumers (such as damage provisions or class action lawsuits), or declaring certain debt collection practices unlawful.⁴ Importantly, the applicable law is the collection law of the state in which the consumer resides, regardless of where the original creditor or third-party collector is located.

Unlike mainstream creditors, payday lenders do not rely as much on third-party debt collection and do much of their collection in house. I provide evidence for this claim in two ways. First, I conduct a manual review of all public enforcement actions by the CFPB against a payday lender whose debt collection practices violated the Consumer Financial Protection Act (CFPA), the Dodd Frank Act, or the Fair Debt Collection Practices Act (FDCPA). This information was compiled by searching the CFPB's repository of enforcement actions and manually reviewing entries to determine whether the alleged debt collection violation was committed by an employee of the payday lender or a third-party collector.⁵

Results are summarized in Appendix Table A1. Of the nine public enforcement actions I

 $^{^{3}}$ CFPB, "Consumer Financial Protection Bureau Issues Final Rule to Implement the Fair Debt Collection Practices Act," October 30, 2020. https://www.consumerfinance.gov/about-us/newsroom/consumerfinancial-protection-bureau-issues-final-rule-implement-fair-debt-collection-practices-act/

⁴For more information on the legislation changes regarding debt collection, see Appendix C.

⁵The CFPB's repository of enforcement actions can be found at https://www.consumerfinance.gov/ enforcement/actions/. I filtered actions belonging to product "Debt Collection" and did separate searches for keywords "payday loan," "payday lender," and "payday." I then manually reviewed all entries to ensure that the enforcement action was against a payday lender for debt collection violations and determine whether the alleged violation was committed by in-house or third-party collectors. This information is detailed in the complaints and consent orders associated with each enforcement action.

identify, only one, the one against ACE Cash Express, involved third-party debt collectors.⁶ While the regulatory changes I study do not include the CFPA, the Dodd Frank Act, or the FDCPA, this analysis serves to show that these payday lenders, some of which are among the largest in the country, relied on in-house debt collection at the time these alleged violations happened. Since payday lenders do not usually disclose information on their debt collection practices, these public enforcement actions provide rare insight into the reliance of payday lenders on third-party debt collectors.

I supplement this analysis with an original survey of members of a trade association of thirdparty debt collectors and debt buyers.⁷ As shown in Table 1, 69% of survey respondents reported that 0% of their portfolio of receivables corresponded to payday loans in the past 12 months, and 89% of respondents reported that no more than 10% of their portfolio corresponded to payday loans. These results are consistent with existing survey evidence that revolving debt, medical debt, student debt, tax liens, mortgage debt, other bank debt (like personal and auto loans), and unpaid utility bills account for nearly 98% of all debt collected by third-party debt collectors (ACA International 2017). This means that all other debts, including payday loans, are relatively unimportant for the debt collection industry.

Taken together, these findings suggest that payday lenders do much of their collection in house without relying on collection agencies, and that payday loans are not an important asset class for the third-party debt collection and debt buying industry.

2.2 Data Sources

To study the effect of debt collection practices on consumer outcomes, I use data from the Gies Consumer and small business Credit Panel (GCCP). The GCCP is a novel dataset at the individual level containing linked administrative data on mainstream credit, alternative

⁶ACE Cash Express has since ceased to use third-party debt collectors (Lucas et al. 2016).

⁷Details about the survey can be found in Appendix B.

credit, and small business loans. I use the mainstream and alternative credit dimensions of the GCCP in this work, and describe the sources of these data in this section.

Data on mainstream credit outcomes come from Experian, one of the three main nationwide credit bureaus. These data consist of a random sample of individuals with credit reports and contain a snapshot of individual credit reports in the first quarter of each year starting in 2004. I restrict this sample to the period between 2004 and 2015 since I only have information on debt collection legislation until 2015 and, throughout the sample period, there are over five million unique borrowers in this panel. This dataset has information on credit balances—including mortgages, auto loans, credit cards, and student loans—credit limits, payment history, past due balances, collections, inquiries, and public records such as bankruptcies. Also available are credit scores according to the VantageScore model, a scoring model comparable to the Fair Isaac Corporation (FICO) score with values ranging from 300 to 850.

The GCCP also has information on the tax-reported income of individuals. These data are produced by a model developed and validated by Experian using actual tax-reported income linked to 250 thousand credit records. According to Experian, 77 percent of consumers with an estimated income of \$100,000 or more had actual incomes of at least \$75,000 and over 85 percent of consumers with estimated incomes of less than \$35,000 earned less than \$50,000.

In addition to mainstream credit, this study also focuses on how debt collection practices affect borrowing from alternative credit sources. The most popular of these products are payday loans, which are named after the fact that they are structured as a single payment that corresponds to the amount borrowed plus fees and coincides with the borrower's next payday. Payday loans are unsecured, but generally require some evidence of a regular income. Loan amounts are typically under \$500 and fees average \$10 to \$20 for every \$100 dollars in principal, meaning that costs are very high relative to loan amounts.

Alternative credit products such as payday loans are not reported to major credit bureaus

and, consequently, are not a part of the data set described above. Mainstream credit registry data are thus supplemented with data from Experian's alternative credit bureau, Clarity Services. Clarity functions like a traditional credit bureau, with providers of alternative credit products reporting borrower information to Clarity for verification. However, unlike a mainstream loans, alternative credit products are not regulated by the Fair Credit Reporting Act, which means that Clarity data only includes loans from lenders who use Clarity's underwriting services and covers 70% of nonprime consumers across the United States (Miller and Soo 2020). Clarity data are available from 2012 onward and contain information on payday borrowing by consumers in the sample, both from storefront and online lenders. Importantly, Experian provides a time-invariant, anonymized borrower key that can be used to link borrowers in both databases.

I merge the sample of individuals with mainstream credit reports with Clarity data to determine if and when each individual borrows from payday lenders. Of the more than five million borrowers with a mainstream credit record, approximately 565 thousand have Clarity records between 2012 and 2015. The sample is restricted to consumers residing in contiguous county pairs that are located in different states, as described in Section 4, and includes nearly two million unique borrowers across 12 years of data, of whom nearly 242 thousand have a Clarity record between 2012 and 2015.

In addition to credit data, I also collect state-level data on the debt collection industry from the Census County Business Patterns (CBP). This annual survey provides the number of establishments, number of employees and annual payroll by industry, and tracks third-party debt collection agencies under code 561140 of the North American Industry Classification System (NAICS). These data are used to argue that changes in state-level regulation concerning debt collection practices have a significant impact in the collection industry and to show that the number of debt collectors and collection agencies in a state does not predict legislation changes. Finally, I use data on a number of macroeconomic variables at the state level to evaluate the possibility that omitted factors change contemporaneously with legislation changes regarding debt collection and to control for local economic effects in the estimation procedure. Data on unemployment rates and on income per capita from 2000 to 2015 come from the Bureau of Labor Statistics. I also obtain a house-price index from 2000 to 2015 from the Federal Housing Finance Agency and medical expenditures per capita from 2000 to 2014 (the last available year) from the Kaiser Family Foundation.

Table 2 reports summary statistics for this sample. State-level variables are summarized from 2000 to 2015 and individual-level credit data from 2004 to 2015.⁸ Borrowers in this sample have an average \$50,243 in total debt balances, \$5,470 in revolving balances, and \$23,294 in revolving credit limits (summed over all revolving loans). An average \$212 of these credit balances are past due, not conditional on having balances past due, and there is an average of one payday loan for every 100 consumers in this sample.

Moreover, since data on payday loans is novel, I also compare the characteristics of payday loans in my sample to those of other studies with loan- or individual-level data on these products. Appendix Table A2 builds on the comparison reported in Table II of Miller and Soo (2020) and summarizes payday loan characteristics across a range of different studies, including this one. The samples in this collection of studies differ across many dimensions, including geography, whether the sample is restricted to payday borrowers who have mainstream credit records, whether borrowers have a bankruptcy flag, whether the sample includes payday loans originated by storefront, online payday lenders or both, and whether rollover loans are treated as new loans. However, this analysis serves to provide a benchmark for the data used in this study. Comparing the average number of loans and the average loan size in the current study to the median across all studies, we see that payday borrowers

⁸Exceptions to this rule are alternative credit outcomes, which are available from 2012 to 2015, and state-level medical expenditures per capita, which are available from 2000 to 2014.

in my sample have 2.8 fewer loans (3.2 compared to 6) and 24 fewer dollars per loan (\$349 compared to \$373).

One factor that might explain why payday borrowers in my sample have fewer loans is that rollover loans are not always treated as new loans in Clarity, according to conversations with Clarity data experts. This is in contrast with studies that use administrative data collected through the CFPB's supervisory process such as Wang and Burke (2021), who treat rollover loans as new loans and find that borrowers have an average of 5.8 payday loans. Moreover, unlike in other studies, my sample is a representative sample of all consumers with a mainstream credit record that is then linked to Clarity records for all individuals that are in the Clarity database, and is not a representative sample of payday borrowers or of consumers with records in Clarity. This is in contrast with Miller and Soo (2020), who obtain a representative sample of consumers with any record in Clarity and restrict this sample to individuals who had a Chapter 7 bankruptcy flag removed between 2013 and 2017.

3 Conceptual Framework

According to the CFPB, the majority of credit card issuers who are unsuccessful in collecting on a debt will eventually turn to third-party debt collectors (CFBP 2015). Since mainstream creditors rely on debt collectors and restricting third-party debt collection practices reduces the ability of these firms to collect, we should expect the supply of mainstream credit to contract as a consequence of these regulations. Also, since debt collection serves to enforce consumer credit contracts, restricting debt collection can also affect the payment behavior of consumers, either by making existing borrowers more likely to miss payments (moral hazard) or by attracting riskier borrowers into the market (adverse selection). This could also reduce the supply of credit to these borrowers even absent changes to underwriting standards.

As shown in Section 2.1 payday lenders do not rely as much on third-party debt collection as mainstream lenders, and instead do much of their collection in house. One potential explanation for this fact is that reputational concerns are an important factor in the decision to hire third-party debt collectors (Fedaseyeu and Hunt 2018). As discussed in Section 2.1, original creditors collecting on their own debt are not generally subject to the same regulatory constraints as third-party collectors, which makes in-house collection relatively more attractive. Fedaseyeu and Hunt (2018) show that the ubiquitous use of third-party collection among mainstream creditors can be rationalized by those creditors not wanting to be associated with aggressive collection practices. If payday lenders are less concerned about their reputation, this can explain why they are more likely to rely on in-house collection than mainstream creditors.⁹

Since payday lenders do not rely on third-party debt collectors as much as mainstream lenders, we should not expect restrictions on third-party debt collection to affect the *supply* of payday loans. However, since these regulations affect the supply of mainstream credit, they can affect the *demand* for payday loans if consumers substitute for mainstream credit with payday loans. With typical APRs reaching 400% to 500%, payday loans are an extremely costly source of credit, and consumers should be unlikely to turn to it when less expensive sources of financing are available.

This prediction is consistent with a "pecking order theory" of consumer sources of financing (Lusardi et al. 2011). Lusardi et al. (2011) find that consumers are more likely to draw on savings in the event of an unexpected expense, followed by mainstream credit and/or borrowing from family and friends. This is pecking order is consistent with the findings of Dupas et al. (2017) and Kast and Pomeranz (2021), who find that access to savings accounts reduces borrowing.

On the other hand, payday loans are a more expensive source of financing than mainstream credit, and are thus below mainstream loans in the pecking order. This theory is consistent

⁹The fact that payday lenders reportedly employ very aggressive in-house collection practices (CFBP 2013) is further evidence in favor of this explanation.

with the fact that there is a surge in shopping for and failing to obtain mainstream credit around the time initial payday loan applications occur (Bhutta et al. 2015). This theory also predicts that consumers who turn to payday loans when access to mainstream credit is restricted are the ones who are unable to draw on savings or borrow from family and friends, and are unable to meet their financing needs with their newly reduced supply of mainstream credit. I show evidence in favor of this prediction in Section 5.3.

Finally, this pecking order theory is also consistent with evidence that consumers turn to sources of financing that are even costlier than payday loans following payday bans, such as overdrafts and pawnshop loans. Morgan et al. (2012) find that payday bans lead to an increase in overdrafts and Bhutta et al. (2016) find that payday loan restrictions cause consumers to shift to pawnshop loans, rather than mainstream credit. In addition to shedding light on an important and understudied rung in the pecking order ladder—the substitution from mainstream credit to payday loans—the evidence I uncover helps explain why prior work does not find that payday bans lead to more mainstream borrowing. As I show in Section 5.3, borrowers who turn to payday loans are those with limited access to mainstream credit, which means these borrowers might be unable to go up the ladder toward mainstream credit once access to payday loans are restricted, only down toward even more expensive sources of credit.

4 Empirical Strategy

I estimate the effect of restricting debt collection practices on consumer outcomes using time-series variation in the strictness of state legislation concerning collection practices and exploiting policy discontinuities at state borders. Nearly all of the regulatory changes evaluated were first identified by Fedaseyeu (2020) and, between 2000 and 2015, I analyze 32 changes in state regulations in 20 states.¹⁰ These legislation changes are spread evenly across

¹⁰For more information on the legislation changes regarding debt collection, see Appendix C.

years and Figure 1 shows the cumulative number of legislation changes by year.

The treatment group is composed of consumers residing in counties at the border of states that introduce restrictive debt collection legislation. The control group consists of consumers residing in contiguous counties located in a different state, and I only use variation in debt collection legislation within these pairs of contiguous counties that straddle a common state border.¹¹ This identification strategy compares outcomes for treated and control consumers in all border-county pairs before and after legislation changes. The underlying assumption in employing a county-pair differences-in-differences strategy rather than a standard state-level differences-in-differences strategy is that a consumer is more similar to another consumer residing in a contiguous county than to a consumer residing in a randomly chosen county. Also, by using only variation within pairs of contiguous counties, this strategy controls for time-varying differences across county pairs, such as differences in local economic or creditmarket conditions. Figure 2 shows a map of all border-county pairs with information on how many of the consumers in the sample reside in those counties.

The role of the control group is to provide a counterfactual of what would have happened to consumers if their respective states of residence had not enacted legislation restricting debt collection practices. Accordingly, the key identifying assumption is that, in the absence of legislation changes, outcomes for consumers in treatment and control groups would have evolved according to parallel trends. The main approach used to assess the validity of this assumption is to examine outcomes for consumers in the treatment and control groups prior to the enactment of legislation. As I discuss in Section 5, estimates show that outcomes for the two groups move in close parallel prior to legislation changes.

The baseline specification is

¹¹This identification strategy was first used by Holmes (1998), Huang (2008), and Dube et al. (2010).

$$Y_{ispt} = \beta_1 Index_{st} + \beta_2 X_{ispt} + \kappa_{pt} + \epsilon_{ispt}, \tag{1}$$

where Y_{ipst} is an outcome of consumer *i* residing in state *s* in a county that is part of bordercounty pair *p* in year *t*; *Index_{st}* is a variable that is equals zero before the debt collection legislation change in state *s*, one after the first legislation change, and two in the event the same state enacts another regulation change; X_{ispt} is a set of controls; and κ_{pt} is a vector of border-county-pair×year fixed effects. This specification uses an index that can take values above one instead of a dummy variable identifying only the first legislation change in a state so as to use variation from all instances in which states introduced restrictive debt collection legislation, but I show robustness to using a dummy variable for first legislation changes in Section 5. The coefficient of interest β_1 measures the average change in outcomes for consumers in a county at the border of a state that restricted debt collection legislation relative to consumers in contiguous counties located in another state, following the legislation change.

To provide evidence in favor of the parallel trends assumption discussed above, I also estimate the following specification:

$$Y_{ispt} = \sum_{\tau \in \mathcal{T}} I_s(\tau) + \beta_2 X_{ispt} + \kappa_{pt} + \epsilon_{ispt}, \qquad (2)$$

where $I_s(\tau)$ is equal to one exactly τ years after (or before if τ is negative) state s enacts a new piece of legislation.

Another underlying assumption of this analysis is that changes in state-level legislation regarding debt collection practices meaningfully impact the debt collection industry and, consequently, there are substantial differences in treatment intensity within border-county pairs. Evidence in favor of this assumption comes from estimating the following equation using state-level data:

$$Y_{st} = \beta_1 Index_{st} + \beta_2 X_{st} + \kappa_s + \theta_t + \epsilon_{st} \tag{3}$$

where Y_{st} is either the number of debt collection employees, the number of debt collectors per collection establishment, or the number of collection establishments in state s in year t; $Index_{st}$ is a variable that is equals zero before the debt collection legislation change in state s, one after the first legislation change, and two in the event the same state enacts another regulation change; X_{st} is a set of controls; θ_t is a vector of year fixed effects; and κ_s is a vector of state fixed effects. I also analyze the dynamics around the time of legislation changes by estimating a version of this specification analogous to Equation 2.

Finally, a primary concern in difference-in-differences analyses is the possibility that an omitted factor relevant for the outcome variables of interest changes contemporaneously with the treatment—in this case, with the introduction of state-level legislation regarding debt collection practices. To alleviate this concern, Appendix Table A3 shows results from linear regressions of both the $Index_{st}$ variable and changes to this variable ($\Delta Index_{st}$), corresponding to the enactment of new legislation, on a wide range of variables relating to the state of the economy, credit markets, and the debt collection industry itself. These regressions attempt to predict the introduction of legislation using state-level data on the number of debt collectors, the number of collection establishments, a house-price index, medical expenditures per capita, average credit scores, a measure of payments to revolving past-due balances, average loan balances, the average number of accounts in collection, average balances past due, population, the unemployment rate (in growth rates and in levels), and income per capita (in growth rates and in levels).¹² As shown in Appendix

¹²I construct measure of payments to revolving past-due balances for an individual in a given year is the ratio of all payments to total revolving past due balances in that year, and average that ratio across all consumers in a given state.

Table A3, none of these variables are predictive of legislation changes.

5 Results

5.1 The Effect of Debt Collection Restrictions on the Collection Industry

This section provides evidence that restrictions to debt collection practices meaningfully impact the debt collection industry, which is an underlying assumption of the identification strategy described in Section 4. To do so, I use state-level data on the number of debt collectors and debt collection establishments from County Business Patterns and estimate Equation 3.

The specification in Equation (3) estimates the reduced-form effect of state legislation restricting debt collection practices on the debt collection industry. It includes state fixed effects to account for any level differences between states, as well as time fixed effects to flexibly control for any time trends common to all state. Standard errors are clustered at the state level throughout.

Table 3 shows results of this exercise. In column 1 of Table 3, we see that the number of debt collectors in a state decreases after the state adopts legislation restricting collection practices, relative to states that did not impose new restrictions on debt collectors. Column 2 of Table 3, the preferred specification, controls for state-level unemployment, income per capita, health expenditures per capita, and log population, as well as for a house price index, also at the state level. I find an average decline of 256 debt collectors in states that restrict collection practices, or 10 percent of the sample mean, relative to states that do not impose new restrictions on debt collection.

The remaining columns of Table 3 show that the decline in the number of debt collectors happens through the intensive margin, meaning a reduction in the size of collection establishments, and not through the closing of collection agencies. Column 4 documents a significant reduction of 3.21 debt collectors per establishment, or 14 percent of the sample mean, in states that restricted collection activities relative to other states. In contrast, the decline in the number of collection establishments is small and not statistically distinguishable from zero, suggesting that collection firms respond to these regulatory shocks by adjusting the number of employees and not of establishments.

These findings are evidence that restrictions to debt collection practices have a meaningful impact on the debt collection industry. Moreover, Figure 3 reports estimates and confidence intervals of a dynamic version of Equation 3, and shows that the timing of these results are entirely consistent with legislation changes. In particular, the number of debt collectors and the size of collection agencies in treated and control states move in close parallel prior to legislation changes.

5.2 The Effect on Mainstream Credit

In this section, I show that borrowers at the border of states that restricted debt collection practices see a decline in their access to mainstream credit, relative to borrowers residing in a contiguous county located in a state that did not restrict debt collection. Table 4 reports estimates of the border-county pair specification of Equation 1 for total credit balances, revolving credit balances, revolving credit limits, and credit usage. In these estimates, dependent variables are in levels but, as shown in Appendix Table A4, I obtain consistent results and very similar magnitudes with credit balances and credit limits in logs.

Estimates in Table 4 control for state-level unemployment, income per capita, health expenditures per capita, and log population, as well as for a house price index, also at the state level. Moreover, this specification flexibly controls for unobservable time-varying differences differences across subprime (defined as having a credit score below 620) and prime consumers and across borrowers with and without accounts in collection by including subprime×year and collection×year fixed effects. Since both credit scores and accounts in collection are potentially affected by the treatment, I sort borrowers into these categories in the first year of the sample of credit records (2004) and hold these classifications fixed over time.

In addition to reporting estimates for the full sample of borrowers, Table 4 also shows estimates of Equation 1 separately for borrowers of different income levels. I classify borrowers in 2004 as low income if they are in the first quartile of the 2004 income distribution, as middle income if they are in the middle two quartiles, and as high income if they are in the top quartile of the 2004 income distribution, and hold this classification fixed over time.

Columns 1 to 4 of Panel A of Table 4 report the effect of debt collection restrictions on total credit balances. I find that low-income borrowers experience a decline in total balances of \$1204.83 (9 percent of the sample mean) following collection restrictions, relative to borrowers in contiguous counties located in states that did not restrict collection practices. Columns 5 to 8 of Panel A of Table 4 focus on revolving loan balances, since revolving loans are one of the key types of debts that debt collectors collect on. I find that low-income borrowers see a decline of \$154.49 (15 percent of the sample mean) following collection restrictions. In both sets of outcomes, the only statistically significant declines in credit balances are for low-income borrowers. This pattern is consistent with the fact that these consumers are more likely to have balances past due and to have accounts in collection, although I cannot reject the hypothesis that estimates are the same across the income spectrum.

Figure 4 reports estimates and 95% confidence intervals of Equation 2, which is a dynamic version of Equation 1. It shows that credit balances of low-income borrowers in treatment and control groups move in close parallel prior to treatment, which is evidence in favor of the parallel trends assumption discussed in Section 4.

Next, I show evidence that the reduction in credit balances for low-income consumers is likely due to a restriction in the supply of credit, rather than lower demand. Panel B of Table 4 shows estimates of Equation 1 for outcomes relating to credit limits and credit usage.¹³ Columns 1 to 4 of Panel B of Table 4 report estimates with revolving credit limits as the dependent variable. Specifically, in column 2, I show that low-income consumers experience a decline of \$600.42 (16 percent of the sample mean) in revolving credit limits. Moreover, the remainder of Panel B of Table 4 shows that consumers across the income spectrum experience an increase in the ratio between revolving balances and revolving credit limits, a measure of credit utilization. In particular, low-income consumers experience an increase of 1.40 percentage points (1.6 percent of the sample mean) in revolving balances to limits. Importantly, Figure 5 reports estimates and 95% confidence intervals of Equation 2 and shows that there is no evidence of pre-existing trends in these outcomes.

If the decline in balances that documented in Panel A of Table 4 was driven by lower demand for credit, we would expect to see a decline in credit usage and no change in credit limits. Instead, I find an increase in credit usage and a decline in limits, consistent with lenders reducing the supply of credit in response to states restricting debt collection activities. As an additional piece of evidence to support this hypothesis, Appendix Table A5 shows that the number of new accounts (columns 1–4) and the number of new accounts per inquiry (columns 9–12) also decline as a consequence of collection restrictions. Inquiries correspond to applications for credit and new accounts per inquiry can be viewed as a proxy for a consumer's rate of approval. A decline in new accounts per inquiry is further evidence that debt collection restrictions lead to a negative credit supply shock, especially for low-income consumers. Appendix Table A5 also shows that inquiries are unaffected by debt collection laws (columns 5–8), which is further evidence against a reduction in the demand for credit. The next set of findings sheds light on the effect of restrictions to debt collection practices on the repayment of mainstream loans. I show that borrowers in counties at the border of states that restricted debt collection practices see a decline in credit scores and an increase in past-

¹³This set of results focuses on revolving credit since I do not observe limits for other forms of credit.

due balances, relative to borrowers residing in a contiguous county in another state. Panel A of Table 5 reports estimates of Equation 1 with credit scores and balances past due as dependent variables. In columns 1 and 2, I show that consumers see a decline of 2.55 points in their credit scores and that low-income consumers experience a similar decline of 2.47 points (0.4% of the sample mean). I also find an overall increase in balances past due, which is again concentrated on low-income consumers. Column 6 shows that low-income consumers see past-due balances rise by \$37.65 (19% of the sample mean) as a consequence of collection restrictions. As shown in Appendix Figure A1, the timing of these effects is consistent with the adoption of debt collection legislation and there is no evidence of pre-existing trends in these outcomes.

Panel B of Table 5 reports estimates of Equation 1 separately for balances at each stage of delinquency for low-income borrowers. Consistent with the theory that borrowers are less likely to make payments once collection attempts are restricted, the rise in balances past due is entirely driven by an increase in balances more than 180 days past due, shown in Column 5. Panel A of Appendix Figure A2 plots coefficients and 95% confidence intervals of the event study version of this specification and shows that the timing of the effect of balances more than 180 days past due is also extremely similar to that of total past-due balances (Appendix Figure A1).

Moreover, Columns 6-8 of panel B of Table 5 decompose low-income borrowers into existing borrowers, which are observed in the sample before the current year, and new borrowers, which are observed in the sample for the first time in the current year.¹⁴ I find that the rise in past-due balances is entirely driven by existing borrowers and new borrowers see no significant change in their balances past due following debt collection restrictions. Panel B of Appendix Figure A2 plots coefficients and 95% confidence intervals of Equation 2 and shows

¹⁴ Recall that borrowers are classified as low income in 2004, the first year of the sample, in the baseline analysis. In this analysis, I classify new borrowers as low income if they are in the bottom quartile of the income distribution in the first year that they are observed in the sample.

that past-due balances of existing borrowers follow the same dynamics as total past-due balances (Appendix Figure A1) and that past-due balances of new borrowers are essentially flat around debt collection restrictions. This finding suggests that the rise in balances past due is not driven by a composition effect (i.e., new borrowers being more likely to miss payments) and is instead driven by moral hazard.

Note that two different hypotheses can explain a simultaneous contraction in mainstream credit and rise in past-due balances. The first is that debt collection serves as a mechanism to enforce credit contracts that, when restricted, leads to lower repayment rates on mainstream loans due to moral hazard, causing a contraction in mainstream credit. The second is that lenders respond to debt collection restrictions by supplying less credit, which makes consumers less able to smooth negative shocks with credit and thus more likely to default on existing loans.

I interpret the evidence in panel B of Table 5 as being consistent with the former but not the latter. If lower repayment was driven the fact that consumers are less able to smooth negative shocks, we should expect to see a rise in more recent past-due balances and not just balances more than 180 days past due. We might also expect new and existing borrowers to be similarly harmed and equality more likely to default on loans. Rather, the fact that the rise in past-due balances is driven by existing borrowers and by the late-stage balances that debt collectors usually collect on is consistent with moral hazard. This suggests that collection restrictions lead to lower access to mainstream credit by making credit contracts less enforceable and reducing repayment. As I show in the remainder of this section, lower access to mainstream credit, in turn, pushes some consumers down the pecking order ladder toward more expensive alternative credit.

5.3 The Effect on Payday Borrowing

Next, I analyze the effect of restrictions to debt collection practices on payday borrowing. To do so, I estimate Equation 1 for outcomes relating to payday borrowing, and show that consumers borrow more from payday lenders following debt collection restrictions.

The specification in Equation 1 controls for state-level unemployment, income per capita, and log population, as well as for a house price index, also at the state level.¹⁵ As in Section 5.2, it also controls for unobservable time-varying differences across subprime and prime consumers and across borrowers with and without accounts in collection by including subprime×year and collection×year fixed effects.

Table 6 reports results of this exercise. Across the full sample of borrowers, in columns 1–4 of Table 6, I find an average increase of 7 payday loans for every 1,000 consumers in counties at the border of a state that restricted collection activities, or a sizable 70 percent of the sample mean, relative to consumers in counties that share a border but are located in a different state. I also find an increase in total payday loan amount corresponding to 28 percent of the sample mean, but that is not statistically significant.

In addition to estimating Equation 1 for the full sample of borrowers, I also use the framework described in Section 3 to identify borrowers that are likely to turn to payday loans when access to mainstream credit declines, and estimate Equation 1 separately for those borrowers. As detailed in Section 3, a pecking order theory of consumer borrowing predicts that payday loans will be used as a last resort by borrowers without savings, the ability to borrow from family and friends, or access to mainstream credit. Since I do not observe savings or loans from family and friends, I proxy for lack of access to these resources with whether the consumer has taken a prior payday loan. The intuition behind this proxy is that, if payday

¹⁵Since data on alternative credit is only available from 2012 onward, I choose not to control for health expenditures per capita in the baseline specification since those are not available in 2015 and would restrict the sample period further. However, Appendix Table A6 shows that results are robust to controlling for health expenditures.

loans are at the bottom of the pecking order, having taken out a payday loan signals that the borrower does not have access to better alternatives. Since payday borrowing is affected by the treatment, I define a borrower as a prior payday borrower based on whether or not they have a payday loan in the first year of the Clarity sample.

Moreover, to provide a more direct test of the pecking order theory of consumer borrowing, I further restrict the sample of prior payday borrowers to those who are constrained in their access to mainstream credit. Bhutta et al. (2015) find that 90 percent of payday applicants have have less than \$300 of available revolving credit and 80 percent have no available revolving credit. Accordingly, I define a prior payday borrowers as being mainstream-creditconstrained if they have less than \$300 of available revolving credit, with available credit defined as total revolving credit limit minus total revolving credit balance.¹⁶ This classification is also constructed in the first year of the Clarity sample and held fixed throughout.

Columns 5–8 of Table 6 report results of this exercise. Columns 5 and 6 report results for prior payday borrowers, who take out an additional 1.71 payday loans and an additional \$76.85 in total payday loan amount. Consistent with a pecking order theory of consumer borrowing, these estimates are even larger for borrowers who are mainstream-credit constrained. Columns 7 and 8 of Table 6 show that mainstream-credit-constrained borrowers take out an additional 1.78 payday loans (97 percent of the sample mean) and an additional \$123.83 in total loan amount (41 percent of the sample mean).

The finding that the effect of collection restrictions on payday borrowing is concentrated on prior payday borrowers raises the question of whether the effect is entirely driven by the intensive margin, that is, by current payday borrowers taking on additional payday loans. Note that this is not necessarily the case because most prior payday borrowers in my sample are not *current* payday borrowers in any given year. In fact, I document in Appendix Table A7 that debt collection restrictions have an extensive-margin effect on payday borrowing

 $^{^{16}\}mathrm{Results}$ are robust to using alternative cutoffs for defining mainstream-credit-constrained borrowers.

behavior. Columns 1–4 of Appendix Table A7 show estimates of Equation 1 with a dummy that equals one if a borrower takes out a payday loan in a given year as the dependent variable. I find that prior payday borrowers see a 5.4 p.p. increase in the likelihood of having a payday loan and that this likelihood increases by 6.7 p.p. for mainstream-creditconstrained borrowers, corresponding to a sizable of 19% of the sample mean.

Moreover to show that this pattern is driven by an extensive-margin increase in the demand for payday loans, Columns 5–8 of Appendix Table A7 also report results for a dummy that equals one if a borrower has a payday loan inquiry in a given year. We see that the increase in the likelihood of having a payday loan is driven by an increase in the likelihood of applying for a payday loan, which goes up by 8.6 p.p. for prior payday borrowers and by 13.6 p.p.—or 23% of the sample mean—for mainstream-credit-constrained borrowers. These results suggest that some prior payday borrowers who were previously able to meet their financing needs with mainstream credit are forced to turn to payday loans after debt collection restrictions lead to a contraction in mainstream credit.

Finally, I investigate whether consumers are more likely to default on payday loans after debt collection is restricted, or if instead consumers pay higher fees and interest costs associated with their increased borrowing. Columns 1 to 4 of Table 7 show that consumers are no more likely to default on payday loans as a consequence of debt collection restrictions, even if we focus on prior payday borrowers (column 3) or mainstream-credit-constrained borrowers (column 4). This is in contrast with the increase in past-due mainstream credit balances (Table 5) and is consistent with the evidence presented in Section 2.1 that payday lenders, unlike mainstream lenders, do not usually rely on third-party debt collectors.

Since borrowers are no more likely to default on payday loans, we expect that consumers that take out more payday loans as a consequence of debt collection restrictions will incur in additional fee and interest expenses. Columns 5 to 8 of Table 7 report estimates with total payday interest cost, measured as the difference between total payments and initial payday loan amount, as the dependent variable. I estimate that prior payday borrowers pay an additional \$15.48 in total interest costs (column 7) and mainstream-credit-constrained borrowers pay an additional \$27.80 (column 8) as consequence of debt collection restrictions. Payday loan costs are typically around \$10 to \$20 for every \$100 dollars in principal, and thus these magnitudes are consistent with estimates of an increase in principal of \$76.85 for prior payday borrowers (Table 6, column 7) and \$123.83 for mainstream-credit-constrained borrowers (Table 6, column 8). Assuming the typical payday loan maturity of 14 days, these estimates would imply an APR of 525% to 585%.¹⁷

5.4 The Substitution Effect Between Mainstream and Payday Borrowing

In this section, I build on the finding that restricting debt collection practices leads to a decline in the supply of mainstream credit and increased payday borrowing and provide one of the first measures of the elasticity of substitution between mainstream and alternative credit. I do so by using debt collection restrictions as instrument for access to mainstream credit and estimating the following specification:

$$log(Mainstream\ Credit_{ispt}) = \beta_1 Index_{st} + \beta_2 X_{ispt} + \kappa_{pt} + \epsilon_{ispt}$$

$$\tag{4}$$

$$log(Payday \ Loan \ Amount_{ispt}) = \beta_1 log(Mainstream \ Credit_{st}) + \beta_2 X_{ispt} + \kappa_{pt} + \epsilon_{ispt},$$
(5)

where $Mainstream Credit_{ispt}$ are total credit balances from mainstream lenders of consumer i residing in state s in a county that is part of border-county pair p in year t; $Index_{st}$ is a variable that is equals zero before the debt collection legislation change in state s, one after the first legislation change, and two in the event the same state enacts another regulation

¹⁷For prior payday borrowers, the APR implied by these estimates under the assumption of a loan with a 14-day maturity is $(15.48/76.85) \times (365/14) = 525\%$. For mainstream-credit-constrained borrowers, the implied APR is $(27.80/123.83) \times (365/14) = 585\%$.

change; X_{ispt} is a set of controls; κ_{pt} is a vector of border-county-pair×year fixed effects; and Payday Loan Amount_{ispt} is the total payday loan amount of consumer *i* residing in state *s* in a county that is part of border-county pair *p* in year *t*. Standard errors are clustered at the state level in both the first stage and second stage regressions

Using the index of debt collection restrictions as an instrument for mainstream credit, the two-stage least squares estimates of Equation (5) measure the average elasticity of payday borrowing with respect to mainstream credit for borrowers whose access to mainstream credit is impacted by debt collection restrictions. As with any instrumental variables research design, there are two key identifying assumptions. One, that debt collection restrictions are related to mainstream credit. Second, that debt collection restrictions affect payday borrowing only through their effect on mainstream credit.

In addition to the evidence presented in Section 5.2, Columns 1–3 of Table 8 test the first assumption by reporting estimates of Equation (4), along with F-statistics. For all groups of borrowers, I find that debt collection restrictions lead to lower access to mainstream credit, especially for prior payday borrowers.

The second assumption would be violated if payday lenders systematically relied on debt collectors and debt buyers to recover on payday loans, but the evidence presented in Section 2.1 suggests that this is not the case. Moreover, if third-party debt collection was important for the payday lending industry, we would expect to see an increase in past-due payday loans as debt collection practices are restricted, like we observe for mainstream loans (Table 5). However, as shown in Table 7, borrowers are no more likely to default on payday loans after third-party debt collection is restricted.

Columns 4–6 of Table 8 report two stage least squares estimates of Equation 5. As we might expect from the findings of Sections 5.2 and 5.3, the substitution effect is not economically meaningful for the group consisting of all low-income borrowers. However, consistent with the pecking order framework described in Section 3, the elasticity of substitution of payday loans with respect to mainstream credit is very large for initial payday borrowers (column 5), particularly for those with less than \$300 in available mainstream credit (column 6). I find that mainstream-credit-constrained borrowers increase their payday borrowing by 0.64% when access to mainstream credit declines by 1%.

Since the F-statistics I obtain are not large enough to rule out an issue of weak instruments, I report in Table 8 Chernozhukov and Hansen (2008) 95% confidence intervals, which are valid under weak identification, in addition to reporting asymptotic 95% confidence intervals. The lower bounds of the Chernozhukov and Hansen (2008) confidence intervals are similar or larger than the asymptotic intervals, and show that estimates are statistically significant under weak identification.

These findings suggest that mainstream credit and alternative credit are substitutes among payday borrowers, especially among consumers who have no better option than to turn to payday loans. This exercise also provides one of the first measures of the elasticity of substitution between alternative and mainstream credit. Given the magnitude of estimates, these results suggest that access to mainstream credit is a key determinant of payday borrowing, especially of repeat payday borrowing.

5.5 Heterogeneity by Characteristics of Law Changes

The estimates reported so far reflect the reduced form effect of restricting collection practices on mainstream credit and payday borrowing. The fact that regulation changes differ in what provisions they introduce presents a challenge in interpreting these estimates and understanding what restrictions affect consumer outcomes.

In this section, I estimate results separately for the three categories of law changes described in Section 2.1: (1) laws that impose or tighten licensing and/or bonding requirements, (2) laws that impose civil or administrative penalties for debt collection violations or introduce private remedies (such as damage provisions and class action lawsuits), and (3) laws that prohibit certain debt collection practices. Appendix Figure A3 reports the breakdown across the different categories, including laws that fall under two or all three categories.

Appendix Tables A8 and A9 report estimates of Equation (1) for each category of law changes. I find that estimates are fairly homogeneous across the different types of debt collection restrictions, although standard errors are larger as the number of law changes is smaller. The effect of laws that impose penalties or introduce private remedies on access to mainstream credit (Table A8, columns 3 and 7) and payday borrowing (Table A9, columns 3 and 7) are slightly larger than other categories, although it is also possible that this is driven by the fact that most laws fall under this category, which allows for more precise estimates.

We can conclude from this exercise that the reduced form effect of restricting collections by either tightening licensing or bonding requirements, imposing penalties and introducing private remedies, or prohibiting certain collection practices is lower access to mainstream credit, higher balances past due, and increased payday borrowing.

5.6 Robustness to Controlling for the Distance to the Border

Boundary-based approaches like the one in this study hinge on two assumptions. First, that the areas being compared on either side of the border are similar enough that outcomes for consumers on either side of the border would have evolved in parallel in the absence of debt collection restrictions. Second, that the effect of debt collection restrictions is concentrated on one side of the border. Moreover, there is a potential tension between these two requirements as similar areas are arguably more connected, making policy spillovers more likely (Dieterle et al. 2020).

In the context of this study, policy spillovers would require that debt collectors on one side of the border collect on debt of consumers on the other side of the border. There is evidence that interstate collection is not very prevalent, potentially due to requirements by some states that collectors have a physical presence in the state in order to collect and to the fact that restrictions on hours during which borrowers can be contacted present challenges to collecting from states in different time zones (Fedaseyeu 2020). However, to the extent that debt collection firms on one side of the border collect on loans of consumers on the other side of the border, this should cause consumers in the control group to also be negatively affected by the law changes that affect the treatment group, biasing estimates downward. This suggests that estimates in this study are a lower bound on the effect of debt collection restrictions on mainstream credit and payday borrowing.

To address the first concern—that areas on each side of the border might not be similar enough—Dieterle et al. (2020) propose controlling for the distance of a county to the border by using moments of the distance to the border over the population distribution for each county, allowing for a more precise comparison of the average difference in outcomes at the border. Dieterle et al. (2020) use population counts by census block from the 2010 Census and calculate the distance from the center of each census block to the state boundary, approximating a continuous measure of distance to the border.¹⁸

I implement this robustness check in my setting by estimating the following specification:

$$Y_{iscpt} = \beta_1 Index_{st} + \beta_2 X_{iscpt} + D_s \beta_3^s \bar{x_c} + \kappa_{pt} + \epsilon_{iscpt}, \tag{6}$$

where Y_{ipst} is an outcome of consumer *i* residing in state *s* in county *c* that is part of bordercounty pair *p* in year *t*; $Index_{st}$ is a variable that is equals zero before the debt collection legislation change in state *s*, one after the first legislation change, and two in the event the same state enacts another regulation change; X_{iscpt} is a set of controls; D_s is a vector of indicators for each state; \bar{x}_c is the uncentered mean of the Dieterle et al. (2020) distance to

 $^{^{18}}$ An alternative to controling for moments of the distance to the border distribution is to control for the distance from the geographic centroid of an area to another. Bartalotti et al. (2021) show that using moments of the distance to the border over the population distribution eliminates the asymptotic bias present when using a centroid-based measure of distance.

the border distribution; and κ_{pt} is a vector of border-county-pair×year fixed effects. This specification controls for the average distance to the border over the population distribution in each county and estimates separate linear functions in distance for each state, and thus for each side of the border for a given county-pair.

Appendix Tables A10 and A11 present results of this exercise, and show that estimates are robust to controlling for the distance to the border and magnitudes are similar across all outcome variables.

5.7 Robustness to Excluding States that Banned Payday Lending

A key assumption behind the analysis of the effect of restrictions to debt collection practices on payday borrowing is that no omitted factors that affect payday lending change contemporaneously with the treatment, i.e., with the introduction of state-level restrictions to debt collection practices. Of particular concern is the possibility that states introduce new regulations on payday lending at the same time that they restrict debt collection practices.

Fortunately, there does not appear to be much overlap between new debt collection and new payday lending laws. Of the 32 changes to changes to debt collection regulation that I analyze, only one happened in the same year as one of the 19 changes to payday lending laws identified by Morgan et al. (2012), Melzer and Morgan (2015), and Melzer (2018). In 2004, Georgia both restricted collection practices and banned payday lending. If we expand the definition of overlap to include changes to payday loan regulations in the year preceding or following a restriction to collection practices, Oregon's payday loan ban in 2007, which followed a restriction in collection practices in 2006, also fits the criteria.

While only two of the 32 collection restrictions I study happen in conjunction with or shortly before a payday lending ban, note that even a systematic pattern of banning payday loans around the time that debt collection is restricted could not account for the finding that payday borrowing *expands* with the collection-induced negative shock to mainstream credit access. As further evidence that my findings are not driven by overlap between debt collection and payday lending regulations, Appendix Tables A12 and A13 report estimates of Equation 1 excluding all consumers from the two states with regulatory overlap, Georgia and Oregon, and show that results of this exercise are qualitatively identical to the baseline estimates.

5.8 Robustness to Using Only First Legislation Changes

As discussed in Section 4, I use all restrictions in debt collection practices adopted by states during the sample period to estimate the reduced-form effect of debt collection restrictions. This includes instances in which states adopted new legislation more than once and, accordingly, the main explanatory variable is a state-level index that equals zero before a regulatory change in the state, one after the first legislation change, and two in the event the same state enacts another regulatory change.

This section assessed the robustness of results to using only the first regulatory change in each state by reporting estimates of the following equation:

$$Y_{ispt} = \beta_1 Treated_s \times Post_{st} + \beta_2 X_{ispt} + \kappa_{pt} + \epsilon_{ispt}, \tag{7}$$

where $Treated_s$ is a dummy for state s being among the states that adopt restrictions in debt collection practices and $Post_{st}$ is zero prior to the first legislation change in state s and one after.

Appendix Tables A14 and A15 show results of this exercise. For nearly all outcome variables, restricting to the first regulatory change in each state increases the magnitude of the estimates. In particular, I estimate that low-income borrowers experience a decline of \$1858.49 in credit balances, \$242.32 in revolving balances, \$956.85 in revolving limits, and 6.75 points in credit scores. I also estimate that payday borrowers take on an additional 2.8 payday loans and an additional \$826.14 in total payday loan amount, although the latter is only
significant at 10%. Overall, considering all debt collection restrictions adopted by a state or just the first leads to very similar conclusions.

5.9 Robustness to Excluding States that Loosened Restrictions

This study focuses on legislation that restricted debt collection practices, which account for the vast majority of regulatory changes relating to this industry between 2000 and 2015. However, according to Fedaseyeu (2020), four states adopted legislation that lifted restrictions on debt collectors between 2000 and 2015: Colorado, Florida, Louisiana, Maine, and Tennessee. In this section, I assess the robustness of my findings to excluding from the sample all consumers residing in these states. The goal of this exercise is to alleviate concerns that these states are not an adequate control group for states that restricted collection practices.

Appendix Tables A16 and A17 show estimates of Equation 1 excluding all consumers residing in Colorado, Florida, Louisiana, Maine, and Tennessee. Results are qualitatively identical to baseline estimates and, in particular, I obtain very similar magnitudes across all outcome variables.

6 Conclusion

Debt collection is at the forefront of an important policy debate. Policymakers and consumer advocates alike worry about potentially abusive debt collection practices and, in the 15 years of data I analyze, state legislators introduce new restrictions on debt collection every year. While this study is unable to speak to the effect of these restrictions on the abusive practices they target, it provides causal empirical evidence that informs this debate by focusing on the effects of this legislation on both mainstream and alternative credit markets.

This paper makes use of a unique panel with data on credit balances, credit limits, and payment history of two million US borrowers linked to data on alternative financial services such as payday loans. I study debt collection restrictions across 20 states and find that these restrictions lead to a negative shock to the supply of credit to low-income borrowers, which is likely driven by lower repayment.

I also find that debt collection restrictions cause consumers to borrow more from payday lenders, suggesting that mainstream and alternative credit are substitutes, especially for borrowers with limited access to mainstream credit. In addition to providing one of the first measurements of the elasticity of substitution between alternative and mainstream credit, these findings point to a link between the debate on abusive debt collection practices and concerns regarding the financial burden imposed on consumers by high-cost payday loans.

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Figure 1: Total Number of New Legislation by Year

This figure shows cumulative number of state-level legislation changes that restricted debt collection practices between 2000 and 2015. The legislation changes I study are described in Appendix C.



Figure 2: Contiguous Border Counties

This figure shows the number of borrowers in 2004, the first year of the credit sample, residing in counties at the border of a state that share a border with a county in a different state.



Figure 3: Timing of Effect on Debt Collectors

This figure shows the timing of the effect of debt collection restrictions on debt collection employees (panel A) and on the number of debt collectors per establishment (panel B). This figure plots coefficient estimates and 95 percent confidence intervals from a dynamic version of Equation 3, replacing the *Index* variable with dummies equal to one exactly τ years after (or before if τ is negative) a state enacts a new piece of legislation. Observation is at the state-year level and standard errors are clustered at the state level. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index.



Figure 4: Timing of Effect on Credit Balances

This figure shows the timing of the effect of debt collection restrictions on the total credit balance (panel A) and revolving credit balance (panel B) of low-income borrowers. This figure plots coefficient estimates and 95 percent confidence intervals from Equation 2. Observation is at the consumer-year level and standard errors are clustered at the state level. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Also included are fixed effects to control for unobservable time-varying differences across subprime and prime consumers and across borrowers with and without accounts in collection.



Figure 5: Timing of Effect on Revolving Credit Limits and Usage

This figure shows the timing of the effect of debt collection restrictions on the revolving credit limit (panel A) and revolving balance-to-limit ratio (panel B) of low-income borrowers. This figure plots coefficient estimates and 95 percent confidence intervals from Equation 2. Observation is at the consumer-year level and standard errors are clustered at the state level. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Also included are fixed effects to control for unobservable time-varying differences across subprime and prime consumers and across borrowers with and without accounts in collection.



Figure 6: Timing of Effect on Payday Loans

This figure shows the timing of the effect of debt collection restrictions on the number of payday loans (panel A) and total payday loan amount (panel B). This figure plots coefficient estimates and 95 percent confidence intervals from Equation 2. Observation is at the consumer-year level and standard errors are clustered at the state level. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Also included are fixed effects to control for unobservable time-varying differences across subprime and prime consumers and across borrowers with and without accounts in collection.

Table 1: Results of Survey of Debt Collectors and Debt Buyers

Panel A: Collected or purchased payday loans in past 12 months

	# of Respondents	%	
Yes	11	34.38%	
No	21	65.63%	

Panel B: Percentage of portfolio corresponding to payday loans

	# of Respondents	%	
0%	22	68.75%	
$0\!\!-\!\!5~\%$	2	6.25%	
5 - 10 %	4	12.50%	
10 - 20%	0	0.00%	
20 - 50%	0	0.00%	
50 - 75 %	4	12.50%	
> 75 %	0	0.00%	

Notes: This table reports results of an original survey of members of a trade association of debt buyers and debt collectors. 32 members responded to the survey—a response rate of 8%. Details of the survey are described in Appendix B.

	Mean	Med.	St. Dev.	Ν
Total Balance	53,242.96	$1,\!426.00$	109,616.44	24,337,675
Revolving Balance	$5,\!470.22$	47.00	$15,\!418.24$	24,337,675
Revolving Credit Limit	$23,\!294.33$	3,000.00	42,943.58	24,337,675
Revolving Balance-to-Limit Ratio	62.91	70.00	49.17	24,337,675
Number of New Trades	0.34	0.00	0.70	$24,\!337,\!675$
Inquiries	1.41	1.00	1.97	$24,\!337,\!675$
Balance Past Due	212.07	0.00	$1,\!193.11$	24,337,675
Revolving Balance Past Due	22.12	0.00	138.47	$24,\!337,\!675$
Number of Accounts in Collections	1.27	0.00	3.43	$24,\!337,\!675$
Balance in Collections	696.83	0.00	2,210.59	24,337,675
Credit Score	667.13	662.00	102.62	24,337,675
Wage Income (Modeled)	41,017.67	35,000.00	$23,\!912.96$	$24,\!337,\!675$
Number of Payday Loans	0.01	0.00	0.35	7,801,416
Total Payday Amount	2.23	0.00	106.93	7,801,416
State Debt Collection Employees	2,582.06	$1,\!471.50$	$3,\!165.08$	816
State Debt Collection Establishments	95.80	61.00	104.98	816
State Unemployment Rate	5.88	5.50	2.01	816
State House Price Index	194.57	188.75	44.63	816
State Income Per Capita	$38,\!340.62$	$37,\!117.50$	8,691.44	816
State Population	$5,\!924,\!433.72$	$4,\!148,\!199.00$	$6,\!629,\!947.06$	816
State Medical Expenditures Per Capita	6,374.50	6,292.00	1,591.08	765

Notes: This table shows descriptive statistics for all consumer-level and state-level variables. This table summarize state-level variables from 2000 to 2015 and individual-level credit data from 2004 to 2015. Exceptions to this rule are alternative credit outcomes, which are available from 2012 to 2015, and state-level medical expenditures per capita, which are available from 2000 to 2014.

Dependent Variable:	Debt C	ollectors	Establish	ment Size	Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Index	-224.31** (88.43)	-256.07^{**} (118.79)	-3.29^{***} (1.16)	-3.21^{***} (1.09)	-1.13 (2.92)	-0.36 (2.57)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Mean	2,582.06	2,582.06	23.57	23.57	95.80	95.80
Observations	816	765	816	765	816	765

Table 3: Effect of Debt Collection Restrictions on Debt Collectors

Notes: All columns report estimates of the linear regression model specified in Equation (3). Debt Collectors is the number of employees in the debt collection industry. Establishment Size is the number of employees in debt collection divided by the number of debt collection establishments. Establishments is the number of establishments in the debt collection industry. Observation is at the state-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Differences in the number of observations across regressions with and without controls is due to the fact that health expenditures per capita are not available in 2015. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4: Effect of Debt Collection Restrictions on Mainstream Credit

Dependent Variable:		Credit Balances				Revolving Credit Balances		
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	-3785.33 (2440.48)	-1204.83** (461.21)	-1275.23 (1009.43)	-4857.33 (4531.80)	-132.15 (189.64)	-154.49^{***} (41.90)	-58.97 (92.30)	163.44 (305.54)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	$53,\!242.96$	$13,\!216.63$	39,268.90	$134,\!697.34$	$5,\!470.22$	1,032.18	4,276.15	$14,\!345.03$
Observations	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838	$16,\!533,\!065$	2,734,772	$9,\!049,\!455$	4,748,838

Panel A: Mainstream Credit Balances

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:		Revolving Credit Limits				Revolving Balance-to-Limit Ratio			
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-1063.87 (771.76)	-600.42^{***} (181.27)	-402.59 (379.71)	-708.74 (1222.74)	1.01^{***} (0.32)	1.40^{***} (0.46)	0.79^{***} (0.25)	0.67^{**} (0.30)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	23,294.33	3,653.09	18,000.69	62,564.96	62.91	89.66	61.90	30.18	
Observations	16,533,065	2,734,772	9,049,455	4,748,838	16,533,065	2,734,772	9,049,455	4,748,838	

Notes: All columns report estimates of the linear regression model specified in Equation (1). Credit Balances are the sum of balances across all debt types from mainstream lenders. Revolving Credit Balances are the sum of balances across all revolving balances. Revolving Balance-to-Limit Ratio is the ratio of revolving credit balances to revolving credit limits. I classify borrowers in 2004 as low income if they are in the first quartile of the 2004 income distribution, as middle income if they are in the middle two quartiles, and as high income if they are in the top quartile of the 2004 income distribution, and hold this classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Differences in the number of observations across regressions are due to differences in the number of borrowers in each income category. * p < 0.10, ** p < 0.05, *** p < 0.01.

Dependent Variable:		Credit Score				Balances Past Due			
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-2.55^{**} (1.10)	-2.47^{**} (1.15)	-1.91^{**} (0.74)	-2.12 (1.33)	16.04^{**} (7.66)	37.65^{***} (14.00)	14.87 (9.29)	10.08 (9.74)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$\operatorname{Subprime} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Distance Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	667.13	600.97	670.71	750.11	212.07	200.19	254.95	184.60	
Observations	$16,\!533,\!065$	2,734,772	$9,\!049,\!455$	4,748,838	$16,\!533,\!065$	2,734,772	$9,\!049,\!455$	4,748,838	

Table 5: Effect of Debt Collection Restrictions on Credit Scores and Past-Due Balances

Panel A: Credit Scores and Past-Due Balances

Panel B: Past-Due Stage and Borrower Composition

Dependent Variable:		Balances Past Due				Balances Past Due		
	Low Income	30 Days	60 Days	90-180 Days	>180 Days	Low Income	Existing Borrower	New Borrower
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	37.65^{***} (14.00)	0.61 (0.55)	-1.17 (1.51)	3.08 (4.84)	37.18^{**} (15.43)	37.65^{***} (14.00)	39.18^{***} (13.72)	-0.64 (0.44)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	200.19	6.42	6.03	56.27	134.86	200.19	226.08	3.67
Observations	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,272,674	793,495

Notes: All columns report estimates of the linear regression model specified in Equation (1). Credit Score is the borrower's credit score according to the VantageScore model. Balances Past Due is the sum of all balances that are 30 days or more past due. In Panel B, columns 2 to 5 split Balances Past Due of low-income borrowers by stage of delinquency, from 30 days past due to more than 180 days, and columns 7 and 8 decompose low-income borrowers into existing borrowers, which are observed in the sample before the current year, and new borrowers, which are observed in the sample for the first time in the current year. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Differences in the number of observations across regressions article to differences in the number of borrowers in each income category. * p < 0.10, ** p < 0.05, *** p < 0.01

Dependent Variable:		Payda	ay Loans		Payday Loan Amount			
	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	0.006^{**} (0.003)	0.007^{**} (0.003)	1.71^{***} (0.48)	1.78^{***} (0.42)	0.514^{*} (0.296)	0.638 (0.609)	76.85^{*} (42.02)	$123.83^{***} \\ (37.78)$
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Mean	0.010	0.010	1.83	1.85	2.23	2.23	311.42	306.06
Observations	4,854,079	4,854,079	8,886	6,096	4,854,079	4,854,079	8,886	6,096

Table 6: Effect of Debt Collection Restrictions on Payday Loans

Notes: All columns report estimates of the linear regression model specified in Equation (1). Mainstream Credit is the sum of balances across all debt types from mainstream lenders. Payday Loan Amount is the sum of loan amount across all payday loans. I classify borrowers in the first year of the Clarity sample as payday borrowers if they have at least one payday loan and further as credit constrained if they have no more than \$300 in available mainstream revolving credit, and hold that classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. Controls include unemployment, income per capita, log population, and a house-price index. * p < 0.00, ** p < 0.05, *** p < 0.01

Dependent Variable:	Has Payday Delinquency				Payday Interest Cost			
	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	-0.000 (0.000)	0.000 (0.000)	-0.010 (0.012)	-0.016 (0.015)	0.07 (0.05)	0.07 (0.07)	15.48^{*} (9.04)	27.80^{**} (13.88)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Mean	0.000	0.000	0.123	0.140	0.26	0.26	46.21	46.09
Observations	4,854,079	4,854,079	8,886	6,096	4,851,996	4,851,996	7,821	5,249

Table 7: Payday Default and Total Interest Payments

Notes: All columns report estimates of the linear regression model specified in Equation (1). Has Payday Delinquency is a dummy for whether the borrower has a payday loan in any stage of delinquency. Payday Interest Cost is the difference between total payments and the initial loan amount. I classify borrowers in the first year of the Clarity sample as payday borrowers if they have at least one payday loan and further as credit constrained if they have no more than \$300 in available mainstream revolving credit, and hold that classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01

Dependent Variable:	Μ	Mainstream Credit			Payday Loan Amount			
	All	Payday Borrower	Credit Constrained	All	Payday Borrower	Credit Constrained		
	(1)	(2)	(3)	(4)	(5)	(6)		
Index	-0.20***	-1.64***	-1.48**					
	(0.05)	(0.38)	(0.58)					
Mainstream Credit				0.01	-0.43**	-0.64**		
				(0.01)	(0.16)	(0.27)		
Asymptotic Confidence Interval				[-0.02, 0.04]	[-0.75, -0.11]	[-1.19, -0.10]		
Weak IV Confidence Interval				[-0.03, 0.04]	[-0.80, -0.10]	[-2.66, -0.29]		
Controls	Yes	Yes	Yes	Yes	Yes	Yes		
F-Stat	9.44	19.04	6.38					
Observations	473,083	8,886	6,096	473,083	8,886	6,096		

Table 8: Access to Mainstream Credit and Payday Borrowing

Notes: All columns report two-stage least squares of the regression model specified in Equation (5). Payday Loans are the total amount of loans from payday lenders. Payday Loan Amount is the sum of loan amount across all payday loans. I classify borrowers in the first year of the Clarity sample as payday borrowers if they have at least one payday loan and further as credit constrained if they have no more than \$300 in available mainstream revolving credit, and hold that classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01

A Additional Results



Appendix Figure A1: Timing of Effect on Credit Scores and Past Due Balances

This figure shows the timing of the effect of debt collection restrictions on the credit score (panel A) and balance past due (panel B) of low-income borrowers. This figure plots coefficient estimates and 95 percent confidence intervals from Equation 2. Observation is at the consumer-year level and standard errors are clustered at the state level. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Also included are fixed effects to control for unobservable time-varying differences across subprime and prime consumers and across borrowers with and without accounts in collection.



Appendix Figure A2: Timing of Effect on Past-Due Stage and Borrower Composition

This figure shows the timing of the effect of debt collection restrictions on balance more than 180 days past due (panel A) and balance past due for both new and existing low-income borrowers (panel B). This figure plots coefficient estimates and 95 percent confidence intervals from Equation 2. Observation is at the consumer-year level and standard errors are clustered at the state level. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. Also included are fixed effects to control for unobservable time-varying differences across subprime and prime consumers and across borrowers with and without accounts in collection.



Appendix Figure A3: Summary of Law Changes by Category

This figure breaks down the 32 state-level legislation changes that restricted debt collection practices by type of change. Law changes are categorized as: (1) laws that impose or tighten licensing and/or bonding requirements, (2) laws that impose civil or administrative penalties for debt collection violations or introduce private remedies (such as damage provisions and class action lawsuits), and (3) laws that prohibit certain debt collection practices.

Year	Payday Lender Name	In-House Collection?	Third-Party Collection?
2013	Cash America International	Yes	No
2014	ACE Cash Express	Yes	Yes^{\star}
2015	EZCORP	Yes	No
2016	Moneytree	Yes	No
2018	Cash Express	Yes	No
2019	Cash Tyme	Yes	No
2019	NDG Financial Corp.	Yes	No^\dagger
2020	Approved Cash Advance	Yes	No
2020	Cash Store	Yes	No
Total	9	9	1

Appendix Table A1: CFPB Public Actions against Payday Lenders

Notes: This table summarizes all public enforcement actions by the CFPB against a payday lender whose debt collection practices violated the Consumer Financial Protection Act (CFPA), the Dodd Frank Act, or the Fair Debt Collection Practices Act (FDCPA). Information was obtained from the CFPB's repository of enforcement actions, which can be found at https://www.consumerfinance.gov/enforcement/actions/. I filtered actions belonging to product "Debt Collection" and did separate searches for keywords "payday loan," "payday lender," and "payday." I then manually reviewed all entries to ensure that the enforcement action was against a payday lender for debt collection violations and determine whether the alleged violation was committed by in-house or third-party collectors. This information is detailed in the complaints and consent orders associated with each enforcement action.

 \star ACE Cash Express has since ceased to use third-party debt collectors (Lucas et al. 2016).

[†] NGD Financial Corp. collected through a wholly owned subsidiary that collected only the accounts of NGD Financial Corp. itself. These types of firms are not generally considered third-party collectors under the FD-CPA (CFBP 2016).

Appendix Table A2:	Payday	Loan	Characteristics	Across	Different	Studies
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	Avg. # Payday Loans	Avg. Payday Loan Size	Description of Sample
Fonseca (2022)	3.2	349	Clarity sample with mainstream credit record *
Allcott et al. (2021)	5.35	373	1,205 payday borrowers from an Indiana lender
Wang and Burke (2021)	5.8	528	CFPB supervisory dataset of storefront payday lenders
Miller and Soo (2020)	6	551	Clarity sample with Ch. 7 bankruptcy flag removed
Skiba and Tobacman (2019)	N/A	279	145k payday borrowers from Texas lender
Fritzdixon and Skiba (2016)	N/A	354	2,947 online payday borrowers from Tennessee lender
CFBP (2013, 2014a)	6^{\dagger}	392	CFPB supervisory dataset of storefront payday lenders
Bertrand and Morse (2011)	9.2	373	Texas survey of 1,441 payday borrowers
Lawrence and Elliehausen (2008)	8.3	N/A	National phone survey of 450 payday borrowers
Median	6	373	

Notes: This table builds on the analysis in Table II of Miller and Soo (2020) and summarizes payday loan characteristics across different studies, including this one. Column 2 reports the average number of payday loans per borrower per year. Column 3 reports the average size of a payday loan. Column 3 briefly describes the key characteristics of the sample. The samples differ across many dimensions, including geography, whether payday borrowers have mainstream credit records, whether borrowers have a bankruptcy flag, whether the sample includes payday loans originated by storefront or online payday lenders, and whether rollover loans are treated as new loans or not. However, this analysis serves to provide a benchmark for the data used in this study.

 \star Unlike all other studies listed, the sample in the current study (Fonseca, 2021) includes both payday borrowers and non payday borrowers. To make my sample comparable to other studies, I compute the average number of payday loans per year and the average loan size conditional on payday borrowing.

[†] The CFPB reports the median and not the average number of payday loans per year.

Dependent Variable:	Δ Ir	ndex	Index		
	(1)	(2)	(3)	(4)	
Debt Collection Employees	0.000	0.000	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Debt Collection Establishments	-0.000	-0.000	0.000	0.000	
	(0.000)	(0.000)	(0.003)	(0.002)	
House Price Index	-0.000	0.000	-0.002	-0.002	
	(0.001)	(0.001)	(0.002)	(0.002)	
Medical Expenditures Per Capita	0.000	0.000	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Average Credit Score	0.002	0.005	-0.007	-0.004	
	(0.005)	(0.005)	(0.014)	(0.015)	
Payments/Past-Due Revolving Balances	-0.010	-0.011	0.046	0.037	
	(0.020)	(0.020)	(0.032)	(0.035)	
Average Loan Balances	0.000	-0.000	-0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Average Number of Collections	0.029	0.009	-0.361	-0.355	
	(0.089)	(0.089)	(0.332)	(0.350)	
Average Balances Past Due	0.000	0.000	-0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
State Population	0.000	0.000	-0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Unemployment Rate	-0.015		0.053		
	(0.019)		(0.037)		
Income Per Capita	0.000		0.000		
	(0.000)		(0.000)		
Unemployment Growth		-0.007		0.005	
		(0.023)		(0.028)	
Income Growth		-0.569		-0.368	
		(0.620)		(0.916)	
State FE	Yes	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes	
Observations	561	561	561	561	

Appendix Table A3: Predicting Legislation Changes

This table reports results of linear regressions of both changes in and the level of the $Index_{st}$ variable and changes to this variable on the number of debt collectors, the number of collection establishments, a house-price index, medical expenditures per capita, average credit scores, a measure of payments relative to past-due revolving balances, average loan balances, the average number of accounts in collection, average balances past due, population, the unemployment rate (in levels and in growth rates), and income per capita (in levels and in growth rates). The measure of payments relative to past-due revolving balances is the ratio of all payments to total past due revolving balances, across all consumers in a given state-year.

Dependent Variable:		Credit B	Balances		R	Revolving Credit Balances				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Index	-0.19^{***} (0.06)	-0.20^{***} (0.05)	-0.14^{**} (0.05)	-0.10^{*} (0.05)	-0.10^{*} (0.06)	-0.17^{***} (0.05)	-0.05 (0.04)	-0.03 (0.05)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
$\operatorname{Subprime} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Observations	$16,\!533,\!065$	2,734,772	$9,\!049,\!455$	4,748,838	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838		

Panel A: Mainstream Credit Balances

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:]	Revolving C	redit Limits		Revolving Balance-to-Limit Ratio				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-0.11^{**} (0.06)	-0.22^{***} (0.07)	-0.06 (0.05)	-0.04 (0.03)	0.03^{**} (0.01)	0.04^{*} (0.02)	0.03^{**} (0.01)	0.02 (0.02)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$\operatorname{Subprime} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Collection imes Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Observations	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838	$16,\!533,\!065$	2,734,772	$9,\!049,\!455$	4,748,838	

Notes: All columns report estimates of the linear regression model specified in Equation (1), with dependent variables in logs. I classify borrowers in 2004 as low income if they are in the first quartile of the 2004 income distribution, as middle income if they are in the middle two quartiles, and as high income if they are in the top quartile of the 2004 income distribution, and hold this classification fixed over time. To deal with the large number of zeros in the sample, I use a log(x + 0.001) transformation. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A5: Impact on Number of New Accounts and Inquiries

Dependent Variable:	riable: New Trades					Inqu	iries			New Trades Per Inquiry			
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
Index	-0.01^{**} (0.00)	-0.02^{***} (0.00)	-0.01* (0.00)	-0.00 (0.00)	-0.00 (0.02)	-0.00 (0.02)	0.01 (0.02)	0.00 (0.02)	-0.00 (0.00)	-0.01^{***} (0.00)	-0.01** (0.00)	-0.00 (0.00)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Subprime \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	0.34	0.21	0.32	0.50	1.41	1.10	1.45	1.78	0.26	0.20	0.25	0.31	
Observations	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838	8,986,356	1,132,428	4,668,963	$3,\!184,\!965$	

Notes: All columns report estimates of the linear regression model specified in Equation (1). New Trades is the total number of new mainstream credit trades for a given consumer. New Trades Per Inquiry is the total number of new mainstream credit trades divided by the total number of mainstream credit inquiries. I classify borrowers in 2004 as low income if they are in the first quartile of the 2004 income distribution, and hold this classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log oppulation, and a house-price index. Differences in the number of observations across regressions are due to differences in the number of borrowers in each income category. * p < 0.10, ** p < 0.05, *** p < 0.01

Dependent Variable:		Payda	ay Loans			Payday Loan Amount				
	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Index	0.006^{**} (0.003)	0.003^{*} (0.002)	1.516^{**} (0.579)	1.622^{***} (0.548)	0.514^{*} (0.296)	0.514 (0.554)	133.582* (78.516)	180.146^{***} (59.962)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes		
Mean	0.005	0.005	1.457	1.483	1.766	1.766	349.723	342.490		
Observations	4,854,079	3,677,633	6,741	4,622	4,854,079	3,677,633	6,741	4,622		

Appendix Table A6: Robustness to Controlling for Medical Expenditures: Payday Borrowing

Notes: All columns report estimates of the linear regression model specified in Equation (1). Payday Loans are the total amount of loans from payday lenders. Payday Loan Amount is the sum of loan amount across all payday loans. I classify borrowers in the first year of the Clarity sample as payday borrowers if they have at least one payday loan and further as credit constrained if they have no more than \$300 in available mainstream revolving credit, and hold that classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01

Dependent Variable:		Has Pa	yday Loan			Has Payday Inquiry				
	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Index	0.000 (0.000)	-0.000 (0.000)	0.054^{**} (0.021)	0.067^{***} (0.017)	-0.002 (0.001)	-0.003 (0.001)	0.086^{***} (0.032)	0.136^{***} (0.043)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes		
Mean	0.002	0.002	0.354	0.354	0.015	0.015	0.570	0.582		
Observations	4,854,079	4,854,079	8,886	6,096	4,854,079	4,854,079	8,886	6,096		

Appendix Table A7: Extensive-Margin Effect on Payday Loans

Notes: All columns report estimates of the linear regression model specified in Equation (1). Has Payday Loan is a dummy that equals one if the borrower takes out a payday loan in a given year. Has Payday Inquiry is a dummy that equals one if the borrower has a payday loan inquiry in a given year. I classify borrowers in the first year of the Clarity sample as payday borrowers if they have at least one payday loan and further as credit constrained if they have no more than \$300 in available mainstream revolving credit, and hold that classification fixed over time. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01

Appendix Table A8: Heterogeneity by Characteristics of Laws: Balances, Limits, and Usage

Panel A: Mainstream Credit Balances

Dependent Variable:		Credit	Balances		Revolving Credit Balances				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-1204.83**				-154.49***				
	(461.21)				(41.90)				
Licensing and Bonding		-948.82^{*}				-153.46^{**}			
		(506.22)				(62.75)			
Penalties and Remedies			-1468.46^{***}				-167.77^{***}		
			(423.28)				(43.67)		
Prohibited Practices				-572.79				-86.03*	
				(945.45)				(48.19)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	13,216.63	13,216.63	13,216.63	13,216.63	1,032.18	1,032.18	1,032.18	1,032.18	
Observations	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:		Revolving C	Credit Limits		Revolving Balance-to-Limit Ratio				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-600.42***				1.40***				
	(181.27)				(0.46)				
Licensing and Bonding		-574.39^{*}				1.40^{*}			
		(302.78)				(0.76)			
Penalties and Remedies			-706.63***				1.69^{***}		
			(186.37)				(0.49)		
Prohibited Practices				-120.60				-1.04*	
				(150.13)				(0.49)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	3,653.09	3,653.09	3,653.09	3,653.09	89.66	89.66	89.66	89.66	
Observations	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	

All columns report estimates of the linear regression model specified in Equation (1) for low income borrowers, with law changes broken down into three categories: (1) laws that impose or tighten licensing and/or bonding requirements, (2) laws that impose civil or administrative penalties for debt collection violations or introduce private remedies (such as damage provisions and class action lawsuits), and (3) laws that prohibit certain debt collection practices. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A9: Heterogeneity by Characteristics of Laws: Payday Borrowing and Past-Due Balances

Panel A: Payday Borrowing

Dependent Variable:		Payda	y Loans			Payday Loan Amount				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Index	1.711***				76.853*					
	(0.475)				(42.020)					
Licensing and Bonding		0.650^{*}				106.159^{*}				
		(0.348)				(58.542)				
Penalties and Remedies			1.809^{***}				115.245^{**}			
			(0.543)				(45.620)			
Prohibited Practices				1.813***				111.728**		
				(0.526)				(44.616)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Collection×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Mean	1.827	1.827	1.827	1.827	311.417	311.417	311.417	311.417		
Observations	8,886	8,886	8,886	8,886	8,886	8,886	8,886	8,886		

Panel B: Credit Scores and Past-Due Balance

Dependent Variable:		Credit	t Score		Balances Past Due				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-2.47**				37.65***				
	(1.15)				(14.00)				
Licensing and Bonding		-2.83*				19.91^{*}			
		(1.56)				(9.97)			
Penalties and Remedies			-3.01**				46.89^{***}		
			(1.24)				(15.68)		
Prohibited Practices				-1.05				51.75^{**}	
				(1.13)				(19.96)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	600.97	600.97	600.97	600.97	200.19	200.19	200.19	200.19	
Observations	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	2,734,772	

All columns report estimates of the linear regression model specified in Equation (1) for prior payday borrowers in Panel A and low income borrowers in Panel B, with law changes broken down into three categories: (1) laws that impose or tighten licensing and/or bonding requirements, (2) laws that impose civil or administrative penalties for debt collection violations or introduce private remedies (such as damage provisions and class action lawsuits), and (3) laws that prohibit certain debt collection practices. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A10: Robustness to Controlling for Distance to the Border: Balances, Limits, and Usage

Dependent Variable:		Credit Ba	Revolving Credit Balances					
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	-3460.21*** (1138.41)	-1341.56^{***} (462.97)	-1531.42** (753.42)	-3571.03^{**} (1706.52)	-158.45 (105.68)	-121.38^{***} (40.03)	-67.55 (66.11)	12.11 (170.65)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	53,242.96	13,216.63	39,268.90	134,697.34	$5,\!470.22$	1,032.18	4,276.15	$14,\!345.03$
Observations	$16,\!533,\!065$	2,734,772	$9,\!049,\!455$	4,748,838	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838

Panel A: Mainstream Credit Balances

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:		Revolving Cr	Revolving Balance-to-Limit Ratio					
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	-953.42** (390.48)	-468.89^{***} (148.65)	-297.57 (224.26)	-867.76 (568.58)	0.53^{***} (0.20)	0.71^{**} (0.31)	0.26 (0.20)	0.42^{**} (0.20)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	23,294.33	$3,\!653.09$	18,000.69	62,564.96	62.91	89.66	61.90	30.18
Observations	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838

All columns report estimates of the linear regression model specified in Equation (??), which controls the uncentered mean of the Dieterle et al. (2020) distance to the border distribution. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A11: Robustness to Controlling for Distance to the Border: Payday Borrowing and Past-Due Balances

Panel A: Payday Borrowing

Dependent Variable:	Payday Loans				Payday Loan Amount			
-	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	0.005^{*} (0.003)	0.007^{**} (0.004)	1.86^{***} (0.56)	1.84^{***} (0.46)	0.25 (0.36)	1.00 (0.73)	65.35* (38.52)	$117.80^{***} \\ (33.12)$
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.010	0.010	1.83	1.85	2.23	2.23	311.42	306.06
Observations	$4,\!854,\!079$	$4,\!854,\!079$	8,886	6,096	$4,\!854,\!079$	$4,\!854,\!079$	8,886	6,096

Panel B: Credit Scores and Past-Due Balance

Dependent Variable:	Credit Score					Balances Past Due				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Index	-1.79^{***} (0.64)	-1.75^{**} (0.78)	-1.22^{**} (0.57)	-1.30^{**} (0.60)	12.60^{*} (6.96)	28.45^{**} (10.83)	12.85 (8.32)	6.36 (7.70)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
$Subprime \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Distance Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Mean	667.13	600.97	670.71	750.11	212.07	200.19	254.95	184.60		
Observations	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838	$16,\!533,\!065$	2,734,772	9,049,455	4,748,838		

All columns report estimates of the linear regression model specified in Equation (??), which controls the uncentered mean of the Dieterle et al. (2020) distance to the border distribution. Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A12: Robustness to Excluding States that Banned Payday Lending: Balances, Limits, and Usage

Panel A: Mainstream Credit Balances

Dependent Variable:		Credit B	alances		Revolving Credit Balances				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-3430.94 (2681.64)	-1413.42^{***} (496.66)	-918.48 (1080.99)	-4869.97 (4886.72)	-68.43 (211.10)	-167.69^{***} (45.81)	-8.17 (100.77)	210.50 (342.01)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	$53,\!548.78$	$13,\!296.46$	39,360.56	135, 120.56	$5,\!498.42$	1,038.56	4,286.58	$14,\!372.15$	
Observations	$16,\!097,\!528$	2,648,840	8,801,850	4,646,838	16,097,528	2,648,840	8,801,850	4,646,838	

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:		Revolving Cr	edit Limits		Revolving Balance-to-Limit Ratio				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-771.63 (854.17)	-655.11^{***} (198.88)	-205.45 (409.11)	-361.80 (1354.08)	0.78^{**} (0.33)	1.49^{***} (0.51)	0.54^{**} (0.23)	0.54 (0.33)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Subprime \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	23,449.89	$3,\!682.33$	$18,\!074.42$	62,751.29	62.73	89.52	61.80	30.12	
Observations	16,097,528	2,648,840	8,801,850	4,646,838	$16,\!097,\!528$	2,648,840	8,801,850	4,646,838	

All columns report estimates of the linear regression model specified in Equation (1) excluding all consumers residing in states that banned payday lending within a 2-year window of restricting collection practices (Georgia and Oregon). Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A13: Robustness to Excluding States that Banned Payday Lending: Payday Borrowing and Past-Due Balances

Panel A: Payday Borrowing

Dependent Variable:	Payday Loans				Payday Loan Amount			
-	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	0.006^{*} (0.003)	0.007^{*} (0.003)	1.693^{***} (0.492)	1.760^{***} (0.420)	0.465 (0.325)	0.585 (0.605)	75.027^{*} (42.970)	$\frac{115.646^{***}}{(36.766)}$
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Mean	0.010	0.010	1.841	1.862	2.276	2.276	313.981	308.910
Observations	4,729,327	4,729,327	8,766	6,030	4,729,327	4,729,327	8,766	6,030

Panel B: Credit Scores and Past-Due Balance

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Dependent Variable:	Credit Score					Balances Past Due				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Index	-2.18^{*} (1.21)	-2.84^{**} (1.27)	-1.55^{*} (0.79)	-1.81 (1.45)	20.10^{***} (7.43)	45.19^{***} (14.52)	19.70** (9.24)	10.37 (10.28)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
$\operatorname{Subprime} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Mean	667.50	601.23	670.91	750.22	212.54	200.49	255.70	184.97		
Observations	16,097,528	2,648,840	8,801,850	4,646,838	16,097,528	2,648,840	8,801,850	4,646,838		

All columns report estimates of the linear regression model specified in Equation (1) excluding all consumers residing in states that banned payday lending within a 2-year window of restricting collection practices (Georgia and Oregon). Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.
Appendix Table A14: Robustness to Using First Law Changes: Balances, Limits, and Usage

Panel A: Mainstream Credit Balances

Dependent Variable:	Credit Balances				Revolving Credit Balances				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Treated imes Post	-5738.52** (2393.18)	-1858.49** (694.04)	-2079.69^{*} (1114.28)	-6668.67 (5182.80)	-383.27* (191.96)	-242.32^{***} (63.36)	-170.51 (113.44)	2.75 (347.81)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	$53,\!242.96$	$13,\!216.63$	39,268.90	$134,\!697.34$	$5,\!470.22$	1,032.18	$4,\!276.15$	$14,\!345.03$	
Observations	$22,\!383,\!643$	2,734,772	9,049,455	4,748,838	$22,\!383,\!643$	2,734,772	9,049,455	4,748,838	

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:		Revolving C	Credit Limits	Revolving Balance-to-Limit Ratio				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated imes Post	-2135.19^{**} (837.60)	-956.85^{***} (292.88)	-1052.14^{**} (499.62)	-1632.27 (1461.66)	2.91^{***} (0.87)	2.94^{***} (0.95)	1.88^{***} (0.55)	1.27^{**} (0.48)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	$23,\!294.33$	3,653.09	18,000.69	62,564.96	62.91	89.66	61.90	30.18
Observations	22,383,643	2,734,772	9,049,455	4,748,838	22,383,643	2,734,772	9,049,455	4,748,838

All columns report estimates of the linear regression model specified in Equation (7). Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A15: Robustness to Using First Law Changes: Payday Borrowing and Past-Due Balances

Panel A: Payday Borrowing

Dependent Variable:		Payda	y Loans		Payday Loan Amount			
	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated×Post	0.005^{*} (0.003)	0.005 (0.003)	1.677^{**} (0.632)	1.792^{***} (0.589)	0.402 (0.299)	0.461 (0.632)	93.284 (67.740)	142.633^{**} (58.463)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Mean	0.010	0.010	1.827	1.848	2.228	2.228	311.417	306.061
Observations	4,854,079	4,854,079	8,886	6,096	4,854,079	4,854,079	8,886	6,096

Panel B: Credit Scores and Past-Due Balance

Dependent Variable:		Credi	it Score			Balances Past Due				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Treated×Post	-7.39^{***} (2.54)	-6.75^{**} (2.62)	-5.04^{***} (1.68)	-4.11^{**} (1.94)	21.36** (8.77)	44.91^{***} (14.47)	27.92^{**} (11.81)	20.09 (13.67)		
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Mean	667.13	600.97	670.71	750.11	212.07	200.19	254.95	184.60		
Observations	22,383,643	2,734,772	9,049,455	4,748,838	22,383,643	2,734,772	9,049,455	4,748,838		

All columns report estimates of the linear regression model specified in Equation (7). Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A16: Robustness to Excluding States that Loosened Restrictions: Balances, Limits, and Usage

Dependent Variable:		Credit B	alances		Revolving Credit Balances				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-3830.62 (2491.90)	-1177.56^{**} (477.90)	-1280.22 (1029.29)	-4998.49 (4632.71)	-142.47 (192.17)	-155.91^{***} (43.76)	-72.91 (91.88)	150.51 (311.19)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime imes Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$\operatorname{Collection} \times \operatorname{Year} \operatorname{FE}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	$54,\!151.98$	$13,\!390.19$	39,786.57	$136,\!408.33$	5,552.52	1,048.24	4,337.93	$14,\!459.09$	
Observations	15,615,679	2,570,132	8,509,449	4,536,098	$15,\!615,\!679$	2,570,132	8,509,449	4,536,098	

Panel B: Mainstream Credit Limits and Usage

Dependent Variable:		Revolving C	redit Limits		Revo	lving Balance-to-Limit Ratio		
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	-1086.02 (786.41)	-630.29*** (185.97)	-418.38 (388.91)	-730.69 (1248.16)	1.00^{***} (0.32)	1.44^{***} (0.48)	0.78^{***} (0.25)	0.67^{**} (0.31)
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	23,662.49	3,730.75	18,274.33	63,072.77	62.49	89.37	61.49	30.03
Observations	15,615,679	2,570,132	8,509,449	4,536,098	15,615,679	2,570,132	8,509,449	4,536,098

Notes: All columns report estimates of the linear regression model specified in Equation (1) excluding all consumers residing in states that lifted restrictions on debt collectors between 2000 and 2015 (Colorado, Florida, Louisiana, Maine, and Tennessee). Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table A17: Robustness to Excluding States that Loosened Restrictions: Payday Borrowing and Past-Due Balances

Panel A: Payday Borrowing

Dependent Variable:	Payday Loans				Payday Loan Amount			
	All	All	Payday Borrower	Credit Constrained	All	All	Payday Borrower	Credit Constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index	0.006^{**} (0.003)	0.007^{**} (0.003)	1.606^{***} (0.461)	1.629^{***} (0.377)	0.503^{*} (0.296)	0.648 (0.616)	65.429 (42.951)	$100.319^{***} \\ (31.687)$
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Mean	0.009	0.009	1.744	1.806	2.066	2.066	297.894	301.761
Observations	4,586,197	4,586,197	7,725	5,216	4,586,197	4,586,197	7,725	5,216

Panel B: Credit Scores and Past-Due Balance

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Dependent Variable:	Credit Score				Balances Past Due				
	All	Low Income	Medium Income	High Income	All	Low Income	Medium Income	High Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Index	-2.60^{**} (1.12)	-2.67^{**} (1.17)	-2.01^{***} (0.75)	-2.13 (1.36)	18.47^{**} (7.54)	40.86^{***} (13.94)	18.23^{**} (8.98)	10.85 (10.00)	
County-Pair×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Subprime×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
$Collection \times Year FE$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Mean	668.16	601.59	671.78	750.51	208.83	196.37	250.56	184.95	
Observations	15,615,679	2,570,132	8,509,449	4,536,098	15,615,679	2,570,132	8,509,449	4,536,098	

Notes: All columns report estimates of the linear regression model specified in Equation (1) excluding all consumers residing in states that lifted restrictions on debt collectors between 2000 and 2015 (Colorado, Florida, Louisiana, Maine, and Tennessee). Observation is at the consumer-year level and standard errors, clustered at the state level, are reported in parentheses. The bottom rows specify the fixed effects and controls included in each column, as well as the mean of the dependent variable. Controls include unemployment, income per capita, health expenditures per capita, log population, and a house-price index. * p < 0.10, ** p < 0.05, *** p < 0.01.

B Survey

The pool of potential respondents consisted of the members of the Receivables Management Association International (RMAI), a trade association of third-party debt collectors and debt buyers. All members received an email with an invitation to participate in the survey and answer the following questions:

Over the last 12 months, did your company collect on, purchase, or sell payday loans? Please select one of the options below:

- a. Yes
- b. No

Over the last 12 months, approximately what percentage of your portfolio of receivables corresponded to payday loans? Please select one of the options below:

a. 0%
b. 0-5%
c. 5-10%
d. 10-20%
e. 20-50%
f. 50-75%
g. More than 75%

Members were not compensated for taking part in the survey. The survey was conducted during the month of September 2021 and a total of 32 members responded to the survey—a response rate of 8%. Survey responses are summarized in Table 1.

C Debt Collection Legislation Changes

Nearly all changes to legislation regarding debt collection practices I study were first identified by Fedaseyeu (2020), and are described in Appendix B of that work. His sources include the National Consumer Law Center's publication Fair Debt Collection, the National List of Attorneys white papers, and Google search. I independently validate all legislation changes identified in this existing work and add four legislation changes in three states, which are described below.

- California, 2013: In 2013, California enacted legislation to restrict the debt collection activities of debt buyers. The bill asserted that debt buyers could only collect on consumer debt, either in house or through a third-party debt collector, if in possession of certain information concerning the debt, including proof of the debtor's agreement to the debt. It also created a private right of action for individuals, allowing them to bring suit against debt buyers who engage in illegal collection practices.
- 2. California, 2015: In 2015, California amended the 2013 bill to give more time for debtors to challenge a judgment or a default judgment. Under the 2013 act, a judgment debtor could file a motion to set aside a judgment and for leave to defend an action relating to the debt up to 2 years after the judgment was entered. This amendment extended the deadline for this recourse for up to 6 years after the judgment was entered, if the collector is a debt buyer or a third-party collector collecting on behalf of a debt buyer.
- 3. Minnesota, 2013: In 2013, Minnesota adopted an act to restrict to six years the statute of limitation during which action can be taken against a debtor. It also required additional documentation from collectors entitled to a default judgment, including evidence that the consumer owes the debt and documentation establishing that the amount claimed to be owed is accurate.

4. Washington, 2013: In 2013, Washington extended its debt collection statutes to debt buyers.