The Impact of Payday Lending on Crimes

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Abstract

Police departments located in states allowing payday lending report 14.34% more property crimes than the police departments located in states not allowing payday lending. I also find that the police departments located in counties bordering with states allowing payday lending report more property crimes. Those results are driven by the financial pressure induced by payday loans. Furthermore, the impact of payday lending concentrates in areas with a higher proportion of the minority population.

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1. Introduction

Does borrowing at high-interest rates do more harm than good to the borrowers? The classical economic theory predicts that borrowing would make the borrowers at least weakly better off as consumers reveal their preferences by borrowing. The behavioral model suggests that borrowing does not necessarily improve the borrowers' financial welfare if the borrowers are irrational (Carrell and Zinman, 2014). Policymakers and borrower rights advocate groups often argue that restricting access to expensive credit protects the borrowers' interests (Zinman, 2010). Payday lending is one of the controversial and expensive credits that receive mixed responses from borrowers and policymakers (e.g., Melzer, 2011, Skiba and Tobacman, 2011, Morgan and Strain, 2008, and Morse, 2011.). The payday lending literature has focused primarily on the borrowers' financial welfare but overlooked the social impacts of payday lending. In this paper, I study how payday lending affects crimes. I find that payday lending increases property crimes.

Access to payday lending could affect crime through several different channels. The social disorganization theory (Kubrin et al., 2011) suggests that payday loan stores decrease guardianship against crime by introducing strangers to the neighborhoods. Also, the presence of payday loan stores shows a sign of physical disorder and economic distress in the neighborhood (Kubrin et al., 2011; Lee et al., 2013).

The routine activity theory (Kubrin and Hipp, 2016) suggests that payday loan stores install a large volume of cash in the neighborhood that attracts burglary and robbery of which the cash income from such offenses often facilitates drug consumption. The literature documents the positive relationship between cash and crimes. Wright et al. (2017)

find that the electronic benefit transfer (EBT) program[1] had a negative and significant effect on the overall crime rate and specifically for burglary, assault, and larceny crimes.

Finally, because of the high annual percentage rate (APR) and the single-payment structure, payday loan borrowers often find it is necessary to renew their contracts when their loans mature because of the difficulty to repay the entire balance. Each time a loan is renewed, the borrower incurs relatively high fees, the burden of which over time exacerbates the borrowers' financial difficulties. The financial strain theory (Kubrin et al., 2011) suggests that financially distressed payday loan borrowers may become crime offenders. For instance, personal indebtedness increases crime (McIntyre and Lacombe, 2012), and neighborhoods subject to higher interest rates have more property crimes (Garmaise and Moskowitz, 2006).

Identifying the causal impact of payday lending is challenging because it is often difficult to isolate the exogenous variation in payday lending access (Gathergood et al., 2019). For example, payday lenders often locate their stores in low-income areas (Bhutta, 2014). To mitigate this concern, I follow the literature and exploit the plausibly exogenous variation generated by the state laws prohibiting or allowing payday lending (Melzer, 2011; Carrell and Zinman 2014).

I collect the property crimes data from the Uniform Crime Reporting (UCR) Program. The UCR Program collects statistics on the number of offenses[2] known to law enforcement. I choose the sample period from 1985 to 2014 because that is the complete dataset offered

1 Electronic benefit transfer (EBT) is an electronic system that allows state welfare departments to issue benefits via a magnetically encoded payment card used in the United States. It reached nationwide operations in 2004. The average monthly EBT payout is $125 per participant.

[2] These offenses including murder and non-negligent homicide, rape, robbery, aggravated assault, burglary, motor vehicle theft, larceny-theft, and arson.

by the UCR program. Using the difference-in-differences specification, I find that the agencies[3] located in states allowing payday lending report 14.34% more property crimes than agencies located in states not allowing payday lending, which translates into approximately 270 property crimes per agency per year. Breaking down the type of property crimes, agencies located in states allowing payday lending report 13.88%, 14.91%, and 14.22% more burglary, larceny-theft, and motor theft crimes than agencies located in states not allowing payday lending, those numbers represent approximately 68 burglary crimes, 132 larceny-theft crimes, and 37 motor theft crimes. Also, the results are consistent when replacing the state and year fixed effects with the agency and year fixed effects.

To identify the channel through which payday lending increases property crimes, I conduct a placebo test by replacing the dependent variable with the violent crimes. The rationale behind this test is that if payday lending affects crimes through the non-financial channel(s), then that channel(s) is likely to increase violent crimes as well. Nevertheless, I find that payday lending does not affect violent crimes. This result confirms that payday lending increases property crimes by imposing more financial pressure on its borrowers.

One concern is that the results of the difference in differences analysis could be driven by the trend differences between states allowing and not allowing payday lending. However, such an effect is likely to show up even before the passing of laws allowing payday lending. I, therefore, conduct a dynamic analysis for the effect of payday lending on property crimes. I find that the coefficient estimates are statistically insignificant before the treated states allow payday lending, suggesting that the effect of payday lending on

[3] The UCR program refers police department as agency.

crimes is not driven by the pre-existing differences between states allowing and not allowing payday lending.

A more subtle concern is that the unobservable state characteristics could drive the payday lending laws and local crime simultaneously. For example, state-level budget problems could motivate the states to adopt laws allowing payday lending, and at the same time, the worsening budget problems could also impact crime rates. To ensure that the effects of payday lending on property crimes are not driven by the state-level factors that are correlated with the laws, I follow Melzer (2011) to construct an alternative measure for payday lending, an indicator equal to one not only for the states allowing payday lending but also for the agencies located in counties bordering with a state allowing payday lending. After controlling for the state×year fixed effects to account for the contemporaneous local shocks at the state level, I find that the agencies located near a state allowing payday lending report 18.41% more property crimes than the agencies located further away, suggesting that the effect of payday lending on crimes is not driven by the state-level unobservable factors.

To further identify whether payday lending affects property crimes through the financial pressure channel, I split my sample based on the local economic conditions. I find that the impact of payday lending on property crimes is stronger in areas subject to low economic conditions such as low household income, low income per capita, high unemployment rate, and high property rate. I find that the effects of payday lending on crimes are stronger in 3 out of 4 sub-samples associated with lower economic conditions. Barth et al. (2015) suggest that borrowers who have limited access to banks are likely to use more payday loans. If their argument is valid, I predict that people will use more payday loans in the areas subject to fewer banks. It is reasonable to assume that the effect of payday lending on crimes is stronger in such areas. To test this assumption, I split my sample based on the number of commercial bank branches. I find that the effects of payday lending on crimes are similar in both sub-samples, suggesting that the effect of payday lending on crimes does not change with accessibility to banks. Last, to explore who are the real victims of the property crimes induced by payday lending, I split my sample based on the proportion of minority populations. I find that the effect of payday lending on property crimes is stronger in the areas subject to the higher proportion of the African American population. This result suggests that African American communities suffer more from the negative social impact (Induce more property crimes) of payday lending on property crimes.

Payday lending could not only affects the borrowers' financial welfare[4]but also affects other aspects of borrowers' life. For example, payday lending could cause psychological and health problems, such as chronic stress, which could motivate the borrowers to engage in criminal activities (Pew Charitable Trusts, 2016). Using the payday loan stores data in 2013, Barth et al. (2020) find that the presence of payday lenders may help reduce property crimes as well as personal bankruptcies. Nevertheless, their results may suffer from the endogeneity issue because they do not isolate the exogenous variation in payday lending access. Also, their results may be biased because they only use one year of data. The reason is that they may overlook some unobserved factors that only exist in 2013 that increase property crimes and payday loan stores simultaneously. Cuffe (2013) finds that the access

4 The literature has explored the relationship between payday lending and household financial welfare. Skiba and Tobacman (2011) find that successful first-time payday borrowing often results in additional loans and interest payments in the future. Campbell, Martinez-Jerez, and Tufano (2012) find that payday lending increases involuntary bank account closures. Melzer (2011) finds that payday lending leads to increased difficulty in paying the mortgage, rent, and utility bills. Fitzpatrick and Coleman-Jensen (2014) find that payday loans help protect some households from food insecurity. Karlan and Zinman (2010) find that restricting access to payday lending cause deterioration in the overall financial conditions of households.

to payday lending in some counties of payday lending prohibiting states (Massachusetts, New Jersey, and New York) induces more larceny, fraud, and forgery crimes. Nevertheless, his results are restricted to the three states in the Northeastern region of the United States. Also, he does not find any impact of payday lending on burglary and other types of property crimes. Hynes (2012) investigates the relationship between payday loans' legality and bankruptcy from 1998 to 2009. He reports that payday lending decreases property crimes; However, his results may suffer from the endogeneity issue because he fails to control for the unobservable state characteristics that could drive the payday lending laws and local crimes simultaneously. Also, he does not include the crime data before 1998 which is publicly available. Xu (2016) studies the effect of payday lending on neighborhood crime rates in Chicago, Illinois. She finds that the property crime rate declined by 1.77% in the first year after the adoption of the new law and 1.49% in the second year. Just as Cuffe (2013) does, her study only focuses on a specific region, which does not provide the overall effect of payday lending on the national level.

Because of the data limitation (Geographic and/or time horizon) and problematic identifications, the literature fails to provide a robust estimation of payday lending on property crimes on the national level. My paper is the first one to provide the effect of payday lending on property crimes on a nationwide level with clear identification strategies. Unlike previous literature, my paper suggests that payday lending increases all types of property crimes. This result may act as an alarm to the people who are considering using payday loans to solve their financial difficulties. Also, I explore the channel through which payday lending affects property crimes – the financial pressure induced by payday loans.

My paper also contributes to the literature on property crime by providing another cause for property crimes - payday lending. Previous studies focus on the impact of households' financial welfare on property crimes. Harries (2006) finds that both property and violent crimes were moderately correlated with population density, and these crimes largely affected the same blocks. Using the data from the 2000 British Crime Survey and the 1991 UK census small area statistics, Tseloni (2005) finds that both household and area characteristics, as well as selected interactions, explain a significant portion of the variation in property crimes. Howsen and Jarrell (1987) find that the level of poverty, the degree of tourism, the presence of police, the unemployment rate, and the apprehension rate affect property crimes. Kelly (2006) finds that violent crimes and property crimes are positively influenced by the percentage of female-headed families and by population turnover, and negatively related to the percentage of the population aged 16-24. Sampson (1985) and Patterson (1991) argue that absolute and relative poverty link to property crime only through their association with family and community instability. Drug enforcement also affects property crimes. Benson and Rasmussen (1992) find that the resource reallocations accompanying strong drug law enforcement lead to more property crimes. Besides the households' financial welfare and drug enforcement, law enforcement also plays a role in property crimes. Sjoquist (1973) finds that an increase in the probability of arrest and conviction and an increase in the cost of crime (punishment) both result in a decrease in the number of property crimes. Last, other factors such as temperature also affect property crimes. Cohn and Rotton (2000) find that more crimes were reported during summer than in other months.

The remainder of the paper proceeds as follows. Section 2 describes the data and construction. Section 3 details the empirical strategy and the identifying assumptions. Section 4 provides the main results on the effects of payday lending on property crimes and addresses the identification challenges. Section 5 extends the analysis by comparing the number of crimes reported by police departments located near a state that allows payday lending with the number of crimes reported by police departments located further away. Section 6 presents some cross-sectional tests on the relationship between payday lending and property crimes. Section 7 concludes.

2. Sample construction and variable definitions

I collect the crime data from the Uniform Crime Reporting (UCR) program. The UCR Program collects data on the number of offenses known to law enforcement. The crime data is obtained from the data received from more than 18,000 cities, universities and colleges, counties, states, tribals, and federal law enforcement agencies voluntarily participating in the program. These offenses include murder and non-negligent homicide, rape, robbery, aggravated assault, burglary, motor vehicle theft, larceny-theft, and arson. They are serious crimes that occur with regularity in all areas of the country. My sample period starts from 1985 to 2014. I choose this period because this is the complete property crime dataset on the agency level collected by the UCR program.

2.1. The dependent variable

The UCR program reports eight crimes including murder and non-negligent homicide, rape (legacy & revised)[5], robbery, aggravated assault, burglary, motor vehicle theft,

[5] rape statistics prior to 2013 have been reported according to the historical definitions, identified on the tool as "Legacy Rape". Starting in 2013, rape data may be reported under either the historical definition, known as "legacy rape" or the updated definition, referred to as "revised."

larceny-theft, and arson. In this paper, I mainly focus on the number of property crimes, that is, the sum of burglary, larceny-theft, and motor theft crimes, reported by the agencies. My sample is a panel dataset with agency-year level observations. The dependent variables are the natural logarithm of the number of property crimes (*Ln (No. of property crimes)*), the natural logarithm of the number of burglary crimes (*Ln (No. of burglary crimes)*), the natural logarithm of the number of larceny-theft crimes (*Ln (No. of larceny-theft crimes)*), and the natural logarithm of the number of motor theft crimes (*Ln (No. of motor theft crimes)*).

2.2. State Laws of Payday Lending

Some states have laws that effectively prohibit payday lending by imposing binding interest rate caps on payday loans or consumer loans. Some other states explicitly outlaw the practice of payday lending. For example, Georgia prohibits payday loans under racketeering laws in 2005. New York and New Jersey prohibit payday lending through criminal usury statutes. Arkansas's state constitution caps loan rates at 17 percent annual interest in 2005. Maine caps interest at 30 percent but permits tiered fees that result in up to 261 percent annual rates for a two-week $250 loan. Oregon permits a one-month minimum term payday loan at 36 percent interest less a $10 per $100 borrowed initial loan fees in 1998. Just as many other laws in the United States, the payday lending law also varies in states. These laws are generally well-enforced, if not always perfectly enforced (King and Parrish 2010), and hence provide a good source of variation in the availability of payday loans across states and over time. I list the detailed information of state legislation for payday lending in table A.1. of the appendix. I define the main independent variable, $Allowed_{it}$, to be one if state i's law does not prohibit the standard payday loan contract in year t, and zero otherwise.

2.3. State level and county level control variables

I include several state-level and county-level control variables that correlate with property crimes from several sources. At the state level, I collect GDP per capita, household income, unemployment rate, and poverty rate from the Federal Reserve Bank of St. Louis. To control for the state-level political influences on the legislation, I add dummy variables indicating whether the majority of the statehouse/state senate is controlled by the Democratic party. I also add a dummy variable indicating whether the governor belongs to the Democratic party. I collect those data from Ballotpedia.[6] County-level control variables, such as population, personal income, income per capita, and the number of job opportunities offered, are collected from the current population survey of the United States Census Bureau. I collect the data for minority populations at the county level from the National Bureau of Economic Research (NBER). To match the county-level control variables with the agency-year level observations, I first identify which county the agency is located in, and then match the property crimes reported by the agency with the counties' federal information processing standards (FIPS) code. I use the FIPS code to match the county-level control variables with the agency-level property crime data.

[Insert Table 1 Here]

Table 1 provides descriptive statistics for the agency-year observations sample. All variables are winsorized at the 1% and 99% levels.[7] I have 100,775 agency-year

[6] https://ballotpedia.org/Main_Page.
[7] The results are consistent if I use unwinsorized data.

observations. The average number of *Property crimes* reported by agencies is 2,180. Among all categories of property crimes, the most reported crime is larceny-theft. The average of *Larceny-theft crimes* is 1,420.

[Insert Table 2 Here]

Table 2 reports the univariate comparison of the dependent variables between the agency-year observations allowing payday lending and the agency-year observations not allowing payday lending. The former reports a higher number of property crimes. For example, the difference in the average number of property crimes between the agency-year observations allowing payday lending and the agency-year observations not allowing payday lending is 94. The difference in the median number of property crimes between those two groups is approximately 153.

3. Identification strategy

The controversy over payday lending has led to considerable variation in the state laws governing the industry. Using those differences, I define an indicator $Allowed_{it}$, to be one if state i's law does not prohibit the standard payday loan contract in year t, and zero otherwise. Because my baseline regressions include state and year fixed effects, the variation that identifies the effect of $Allowed_{it}$ comes from states that switch from allowing to prohibiting payday credit or vice versa. $Allowed_{it}$ will deliver unbiased estimates of the effect of payday lending as long as the political economy behind changes in $Allowed_{it}$ does not separately influence or respond to, property crimes. In another word, my identification assumption is that payday law changes are uncorrelated with the changes in unobserved determinants of property crimes. This assumption is valid because states make changes to payday lending laws for reasons other than fighting against the crimes. For example, Minnesota starts allowing payday lending in 1995 because of the Consumer Small Loan Lender Act (https://www.revisor.mn.gov/statutes/cite/47.60) which intends to avoid residents borrowing money from the unlicensed lender. North Carolina bans payday lending in 2001 because of the predatory nature of such loans - inducing financial pressure on the borrowers in North Carolina.

Following Morgan et al. (2012), I study how the number of property crimes changes as the state switches from allowing to prohibiting payday lending, or vice versa. To mitigate the concern that my results are driven by the differences between states allowing and not allowing payday lending, I construct a propensity score-matched sample. Specifically, I proceed as follows. First, I create a panel dataset that contains the state-year level data including the number of crimes, the dummy variable $Allowed_{it}$, and several control variables that are correlated with the number of crimes. Second, I define the treated group as those state-year observations allowing payday lending and the control group as those state-year observations not allowing payday lending. In this sample, 754 (49.346%) state-year observations allow payday lending, and 776 (50.654%) state-year observations do not allow payday lending. Third, I run a Probit regression to estimate the propensity score (P-score) for receiving the treatment for each observation as follows,

$$Prob(Allowed_{it}) = \alpha_t + \beta_1 X_{it} + \varepsilon_{it} \; (1) \, ,$$

where $Allowed_{it}$ is a dummy to be one if state *i* allows payday lending in year *t*, and zero otherwise. The vector X_{it} is a set of state-level control variables that includes the natural logarithm of the state's population (*Ln (state population)*), the natural logarithm of GDP per capita (*Ln (GDP per capita)*), the natural logarithm of household income (*Ln (Household income)*), *Unemployment rate*, *Poverty rate*, the percentage of the minority population, the natural logarithm of the number of crimes (*Ln (No. of crimes)*), a dummy variable equals one if the Democratic party controls the statehouse, a dummy variable equals one if the Democratic party controls the senate, and a dummy variable indicates a Democratic governor. I include year-fixed effects in this model and cluster the standard error at the state level. I report the marginal effects in Table 3. The standard errors are reported in the parentheses. After the Probit regression. I match the treated states with the control states by using the closest P-score in the year that the treated states started to allow payday lending. This matching process is conducted without any replacement. The control state not only includes the observations from states that never allow payday lending but also includes observations from states that allow payday lending outside the ten years window (-5, +5) around the matching treated state's payday lending adoption year.

[Insert Table 3 Here]

This matching process generates 24 pairs of the treated-control states. I then use the corresponding agency-year level observations of the treated-control states to test the effects of payday lending on property crimes. I choose the ten years window, that is, from five years before to five years after the treated states start allowing payday lending. Table 4 provides descriptive statistics for the matched sample used for this estimation. The sample consists of 37,695 agency-year observations. The average number of *Property crimes* is 2,401.12, which is comparable to the average number of *Ln(property crime)* in the full agency-year observations sample. I then estimate the effect of payday lending on property crimes with the following specification,

$$Property\ crime_{it} = \alpha_{s(i)} + \alpha_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \gamma X_{st} + \delta Z_{ct} + \varepsilon_{it} \ (2),$$

where $Property\ crime_{it}$ is *Ln (No. of property crimes)* reported by agency *i* in year *t*. $Treat_i$ equals one if the agency is located in the treated states, and zero otherwise. $Post_t$ equals one for years after the treated state's payday lending adoption year, and zero otherwise. The vector X_{st} includes state-level control variables, such as the *Ln (Household income)*, *Poverty rate*, *Unemployment rate*, and *Ln (GDP per capita)*. The vector Z_{ct} includes county-level control variables, such as *Ln (population)*, *Ln (income per capita)*, *Ln (personal income*), *Ln (No. of jobs)*, the percentage of the minority population. a dummy variable equals one if the Democratic party controls the statehouse, a dummy variable equals one if the Democratic party controls the senate, and a dummy variable indicates the Democratic governor. $\alpha_{s(i)}$ is the state (agency) fixed effects that control for any time-invariant factors across the state (agency) that are correlated with payday lending laws. α_t is the year fixed effects. Following Petersen (2009), I cluster the robust standard errors at the state level because the payday lending laws vary at the state level. Under this specification, $\beta_1$captures the effect of payday lending laws on property crimes.

4. Results

4.1. Baseline difference-in-differences regressions

[Insert Table 4 Here]

Table 5 reports the difference-in-differences results for estimating equation (2). I control for state-fixed effects and year-fixed effects in panel A. The dependent variables are *Ln (No. of property crimes)*, *Ln (No. of burglary crimes)*, *Ln (No. of larceny-theft crimes)*, and *Ln (No. of motor theft crimes)* in columns (1) to (8). The odd and even number columns provide the results without and with the control variables. I include the results without the control variables because if those variables are affected by the treatment themselves then including them produces biased estimates (Angrist and Pischke, 2008). I include the results with control variables to ensure that my results are robust. The standard errors are reported in the parentheses. In panel A, the coefficient estimates of *Treat*×*Post* in columns (1) and (2) are positive and statistically significant at the 10% and 5% levels. Based on the estimates in column (2), agencies located in states allowing payday lending report 14.34% more property crimes than agencies located in states not allowing payday lending do, which amounts to approximately 270 property crimes based on the average number of property crimes. The coefficient estimate on *Treat*×*Post* in column (4) is positive and statistically significant at the 5% level, suggesting that the agencies located in states allowing payday lending report 13.88% more burglary, which translates into approximately 68 burglary crimes based on the average number of burglary crimes. The coefficient estimates on *Treat*×*Post* in columns (5) and (6) are positive and significant at the 5% and 1% levels, suggesting that the agencies located in states allowing payday lending report 14.91% (or 132) more larceny-theft crimes than the agencies located in the states not allowing payday lending do. The coefficient estimate on *Treat*×*Post* in column (8) is positive and significant at the 5% level, suggesting that agencies located in states allowing payday lending report 14.22% (or 37) more motor theft crimes than agencies located in states not allowing payday lending.

In panel B, I replace the state-fixed effects with the agency-fixed effects. I find that the coefficient estimates of *Treat*×*Post* are still statistically significant in columns (1) and (2). In terms of economic significance, based on column (2), agencies located in states allowing payday lending report 6.40% more property crimes than agencies located in states not allowing payday lending do. This is equivalent to 127 property crimes.

[Insert Table 5 Here]

To identify the channels through which the payday lending laws affect property crimes, I perform a placebo test that replaces *Ln (No. of property crimes)* with *Ln (No. of violent crimes)* in equation (2) as the dependent variable. If payday lending affects property crimes through some non-financial channel(s), payday lending should increase violent crimes. If payday lending affects crimes through the social disorganization or routine activities channel, then the borrowers and the payday loan lenders are likely the victims of some violent crimes associated with payday lending. The reason is that offenders of such crimes often use violence to receive cash that facilitates the consumption of drugs and other bad behaviors. Under this scenario, I predict that payday lending increases violent crimes. Nevertheless, if payday lending affects property crimes through the financial channel, then the borrowers are likely the offenders of property crimes. This is because they are trying to fix their financial problems by breaking the law. Under this scenario, the borrowers are more likely to avoid committing violent crimes because they don't want to solve one problem by creating a new and more serious problem.

[Insert Table 6 Here]

Table 6 presents the results of the placebo test. The dependent variables are *Ln (No. of violent crimes)*, *Ln (No. of murder crimes)*, *Ln (No. of rape crimes)*, *Ln (No. of robbery crimes)*, and *Ln (No. of aggravated assault crimes)*. In contrast to the coefficient estimates on *Treat*×*Post* in Table 5, the coefficient estimates on *Treat*×*Post* are much smaller and statistically insignificant in all columns (except for column (5) in panel A), suggesting that the increases in property crimes are driven by the financial pressure induced by payday loans.

4.2. Identification challenges

The consistency of the difference-in-differences estimation depends on the parallel trend assumption, that is, the outcome variables should have parallel trends in the absence of treatment. To ensure that the difference-in-differences estimation is not driven by the pre-existing trend differences between treated and control states, I perform the dynamic analysis for the effect of payday lending laws on the number of property crimes. Specifically, I interact each event year dummy with the treated state dummy, that is, I estimate the following,

$$Property\ crime_{it} = \alpha_s + \alpha_t + \alpha_j + \sum_{k=-3}^{k=3} \beta_k Treat \times Year_k + \gamma X_{st} + \delta Z_{ct} + \varepsilon_{it} \quad (3),$$

where all variables are defined the same as those in equation (2), except for $Year_k$, which is a dummy variable equal to one if the observation is k years after the states allowing payday lending, and zero otherwise. α_i is the state (agency) fixed effects. α_t is the year fixed effects. α_j is the payday lending adoption year fixed effects. In this model, $\beta_k\ 's$ captures the difference between the effect of payday lending on property crimes in year *k* and the effect of payday lending on property crimes in four and five years before the states started to allow payday lending.

If the effect of payday lending on property crimes is not driven by the pre-existing differences between the treated and control states, I expect $\beta_k's$ to be small for *k* less than zero, and $\beta_k's$ to be positive for k greater than zero. However, if the difference-in-differences estimates are driven by the pre-existing differences between the treated and control states, the $\beta_k's$ could be positive for some k less than zero.

[Insert Figure 1 Here]

Figure 1 plots the coefficient estimates and their 95% confidence intervals of $Treat \times Year_k$. The dependent variable is *Ln (No. of property crimes)*. I find that $\beta_k's$ are all small and statistically insignificant for k less than zero, but they become larger and statistically significant for some k greater than zero. These results suggest that the difference-in-differences regression estimation is unlikely to be driven by the pre-existing differences between the treated and control states.

5. Counties close to states legalizing payday lending

The baseline regression delivers unbiased estimates of the effect of payday lending if payday lending law changes are uncorrelated with changes in unobserved determinants of crimes. The natural question for the baseline regression is whether the state legislators target payday lending and crimes at the same time. For example, state-level budget problems could motivate the states to adopt laws allowing payday lending, and at the same time, the worsening budget problems could also impact crime rates. Also, the baseline regression results may be biased by unobserved factors at the state level. To mitigate this concern, I use the same method as Melzer (2021). First, I construct an alternative measure – an indicator called $Access\_X\_Y_{ct}$. The $Access\_X\_Y_{ct}$ equal to one if the center of the county c is located within X and Y miles of a state allowing payday lending in year t, and zero otherwise. I use the state×year fixed effects to reduce the concern that political forces jointly affect payday laws and crimes, as there is little reason to believe that legislators in nearby states directly influence the number of crimes outside of their state. Furthermore, to the extent that political decisions are correlated among adjacent states, the state×year fixed effects in the regressions prevent this source of variation from affecting the $Access\_X\_Y_{ct}$ coefficients. Second, to ensure that the effects of payday lending on crime are not driven by the state-level factors that are correlated with the payday lending laws, I include the state×year fixed effects to account for contemporaneous local shocks at the state level.

I compare the number of property crimes reported by agencies located near a state allowing payday lending with the number of crimes reported by agencies located further away from the state allowing payday lending. In particular, I estimate the following specification,

$$Proeprty\ crime_{it} = \alpha_{s\times t} + \beta_1 Access\_X\_Y_{ct} + \gamma Z_{ct} + \varepsilon_{it}\ (4),$$

For example, Access_0_30_{ct} equals one if the center of a county is located 30 miles or less from a state allowing payday lending, and zero otherwise. Access_30_40_{ct} equals one if the center of a county is located between 30 and 40 miles from a state allowing payday lending, and zero otherwise. The omitted variable is *Access_40_plus*. The $Access\_X\_Y_{ct}$ measure varies within the state year, but only in states prohibiting payday lending. In other words, if the state-year allows payday lending, $Access\_X\_Y_{ct}$ equals one for sure. If the state-year does not allow payday lending, then the $Access\_X\_Y_{ct}$ can be one for some agencies located in the county which is close to a state allowing payday lending. Within the state-year, the effect of $Access\_X\_Y_{ct}$ on crimes is identified by comparing the number of property crimes reported by the agencies near a state allowing payday lending with those reported by the agencies located further away from states allowing payday lending.

I use $Access\_0\_30_{ct}$ and $Access\_30\_40_{ct}$ as the independent variables and *Ln(No. of property crimes)*, *Ln(No. of burglary crimes)*, *Ln(No. of larceny-theft crimes),* and *Ln(No. of motor theft crimes)* as the dependent variables to estimate equation (4). $Access\_0\_30_{ct}$ is an effective measure of payday lending because the borrowers who reside in states prohibiting payday lending but have access to payday lenders use payday loans. Considerable pieces of evidence suggest that people cross into payday allowing states to get loans. Spiller (2006) documents that Massachusetts residents travel to New Hampshire to get loans. Appelbaum (2006) documents the build-up of payday loan stores along the South Carolina-North Carolina border to serve customers from North Carolina, which prohibits payday lending. Those papers also document that payday lenders cluster at such borders, as one would expect if they face demand from across the border. Therefore, I include *Border*, a dummy variable indicating whether the center of the county is located within 25 miles of the state border, in equation (4). *Border* controls for general differences between counties near a state border and other counties.

[Insert Table 7 Here]

Table 7 presents the results of estimating equation (4). The coefficient estimates on $Access\_0\_30_{ct}$ in columns (1) and (2) are positive and statistically significant at the 5% and 10% levels. The coefficient estimates on $Access\_30\_40_{ct}$ are statistically insignificant in all columns, suggesting that the impact of payday lending on property crimes decreases with the distance to states allowing payday lending. The agencies located within 30 miles of a state allowing payday lending report 18.41% more property crimes than agencies located further away. Overall, the results suggest two things. First, the effect on property crimes is not driven by the political forces that jointly affect payday laws and crimes. Second, payday lending not only affects property crimes in states allowing payday lending but also affects property crimes in counties that share a border with the state(s) allowing payday lending.

[Insert Figure 2 Here]

Next, I perform the dynamic analysis for the effect of $Access\_0\_30_{ct}$ on property crimes. To do this, I create event year dummies for $Access\_0\_30_{ct}$ around the year (5 years before to 3 and more years after) states starting allowing payday lending. Figure 2 plots the coefficient estimates and their 95% confidence intervals for *Access_0_30*. The $\beta_k's$ are small and statistically insignificant for all k less than zero, but they become larger and statistically significant for some k greater than zero, suggesting that the results of table 7 are not driven by the pre-existing differences between a pair of border sharing counties (One locates in a state allowing payday lending and the other does not).

6. The cross-sectional tests

6.1. Local economic conditions

To further identify whether payday lending affects property crimes through the financial pressure channel, I split the propensity score matched-sample into two subsamples-the poor economic condition subsample and the wealthy economic condition subsample. I then re-estimate equation (2) and test the difference between the coefficient estimates of *Treat*×*Post* in the poor economic condition subsample and the wealthy economic condition subsample.

[Insert Table 8 Here]

Table 8 presents the results of estimating equation (2) for the poor economic condition and the wealthy economic condition subsamples. Panels A, B, C, and D split the sample based on the state-year median of household income, income per capita, unemployment rate, and poverty rate. I find that the coefficient estimates on *Treat*×*Post* are positive and statistically significant in low household income, low-income per capita, high unemployment rate, and high poverty rate subsamples.

The differences between the coefficient estimates of *Treat×Post* are statistically significant for household income, income per capita, and employment rate subsamples. These results further suggest that the impact of payday lending laws on property crimes is driven by the financial pressure induced by payday loans.

6.2. The availability of other lenders

The lack of access to formal financing could also serve as another channel to induce people to use more payday loans. For example, the lack of commercial banks motivates borrowers to use more payday loans (Barth et al., 2015). Also, Payday loan storeowners are likely to establish their businesses in areas with fewer commercial banks (Pew Charitable Trusts, 2016). If those arguments are valid, I predict that people will use more payday loans in the areas subject to fewer banks. Therefore, if the financial distress induced by payday loans motivates payday loan borrowers to engage in property crimes, this effect should be stronger in areas with fewer commercial banks.

I collect the number of commercial bank branches at the county level from the Federal Deposit Insurance Corporation (FDIC). I split the propensity score matched-sample based on the state-year median number of commercial bank branches. If the lack of commercial banks motivates borrowers to use more payday loans, then the financial pressure induced by payday loans is going to increase for the borrowers. Under this case, I expect to find a stronger effect of payday lending on property crimes in the subsample subject with fewer banks. If the lack of banks does not motivate people to use more payday loans, then the financial pressure would not change. Under this case, the effect of payday lending on crimes should be similar in both areas.

[Insert Table 9 Here]

Table 9 presents the results of estimating equation (2) on the higher number of commercial bank branches and the lower number of commercial bank branches subsamples. The coefficient estimate on *Treat×Post* is positive and significant in column (1) of both subsamples. The coefficient estimate in the lower commercial bank branches subsample is slightly greater than that in the higher commercial bank branches subsample (0.126 vs 0.115). Nevertheless, the difference between those two coefficient estimates is small and statistically insignificant, suggesting that the lack of commercial banks does not motivate people to use more payday loans.

6.3. Who are the real victims of the effect of payday lending

Stegman and Faris (2003) and King, Li, Davis, and Ernst (2005) find that payday lenders are likely to concentrate on the areas subject to the higher minority population. Also, the extensive literature on discrimination in credit markets (Boucher, Barham, and Carter, 2005) suggests that African Americans and other minorities have less access to the lenders such as commercial banks.

To test whether the minorities' population suffers more from the impact of payday lending on property crimes, I split the propensity score matched-sample into two subsamples - a higher minority population subsample and a lower minority population subsample and then re-estimate the equation (2). If as the literature suggests that payday lenders clustered in the minorities' communities, then I would expect that the effect of payday lending on property crimes is stronger in higher minority population subsamples.

[Insert Table 10 Here]

Table 10 presents the results of estimating equation (2) on a higher minority population and lower minority population subsamples. Panels A, B, C, and D split the sample based on the state-year median of the proportion of the African American, Native American, Asian American, and Latino American populations. I find that the differences between the coefficient estimates of *Treat*×*Post* in panels A and D are positive. Also, the difference between the coefficient estimates of *Treat*×*Post* in panels A is statistically significant. The panels B and C suggest that coefficient estimates of *Treat*×*Post* are positive and significant in the lower minority population subsamples. The difference between the coefficient estimates of *Treat*×*Post* is statistically significant in panel C.

These results indicate that the impact of payday lending on property crimes is larger in the African American communities. To explain this result, Stegman (2007) finds that payday lenders cluster in African American communities. The California Department of business oversight (DBO, 2016) shows that payday loan stores in the state are disproportionately located in heavily African American neighborhoods. Also, the financial institutions do not treat their African American clients equally because the commercial banks use credit scores as a primary determinant of loan approval. Since the average African Americans have lower credit scores than the average White Americans have (Ards and Myers, 2001; Ross and Yinger 2002; Federal Reserve Board 2007), the African Americans' likelihood of getting a loan denied is higher. To explain the results in Panel C for Asian Americans. Sun (1998) reports that Asian American families are likely to save a higher proportion of their income. Therefore, payday loan store owners are less likely to establish their businesses in those communities because the demand is lower.

7. Conclusion

This paper studies the impact of state-level payday lending regulations on property crimes in the United States. Consistent with the financial strain theory, evidence from the difference-in-differences regressions show that legalizing payday lending increases property crimes. On average, the agencies located in states allowing payday lending report 13.65% more property crimes than the agencies located in states not allowing payday lending do. Nevertheless, this impact does not hold for violent crimes because the effect is driven by the borrowers' financial pressure. In other words, payday lending increases property crimes mainly by financial distress.

To strengthen my identification strategy, I conduct a dynamic analysis of the effect of payday lending on property crimes. My results suggest that the difference-in-differences regressions are unlikely to be driven by the pre-existing differences between treated and control states. To account for contemporaneous local shocks at the state level, I create an alternative measure following Melzer (2011) and include state×year fixed effects. My results still hold. Last, I perform several cross-sectional tests to identify the heterogeneity of the adverse effect of payday lending on property crimes. My results confirm that (1) payday lending laws have an impact on property crimes through the financial pressure channel. (2) Compared with White Americans, minorities such as African Americans are the real victims of the adverse impact of payday lending.

The payday loans industry makes large amounts of money from people who live close to the financial edge. The policy question is whether those borrowers should be able to take out high-cost loans repeatedly, or whether they should have a better alternative. Critics of payday lenders, including the Center for Responsible Lending, claim that the loans could become a debt trap for people who live paycheck to paycheck. Nevertheless, if the industry's critics devote themselves to stopping payday lenders from capitalizing on the financial troubles of low-income borrowers, they should look for ways to make suitable forms of credit available. Perhaps a solution to payday lending could come from reforms that are more moderate to the payday lending industry, rather than attempts to close them. Some evidence suggests that smart regulation can improve the business for both lenders and consumers. In 2010, Colorado reformed its payday-lending industry by reducing the permissible fees, extending the minimum term of a loan to six months, and requiring that a loan be repayable over time, instead of coming due all at once. Pew reports that half of the payday stores in Colorado closed, but each remaining store almost doubled its customer volume, and now payday borrowers are paying 42 percent less in fees and defaulting less frequently, with no reduction in access to credit.

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Appendix A. Variable description

Variable	Definition (data source)
Crime rates	
Ln(Property crime)	Natural logarithm of the number of property crimes (uniform crime report)
Ln(Burglary)	Natural logarithm of the number of burglary crimes (uniform crime report)
Ln(Larceny theft)	Natural logarithm of the number of larceny-theft crimes (uniform crime report)
Ln(Motor theft)	Natural logarithm of the number of motor theft crimes (uniform crime report)
Ln(Violent crime)	Natural logarithm of the number of violent crimes (uniform crime report)
Ln(Murder)	Natural logarithm of the number of murder crimes (uniform crime report)
Ln(Rape)	Natural logarithm of the number of rape crimes (uniform crime report)
Ln(Robbery)	Natural logarithm of the number of robbery crimes (uniform crime report)
Ln(Assault)	Natural logarithm of the number of assault crimes (uniform crime report)
Payday lending access	
Allowed	Dummy variable equals one if the agency locate in the state allowing payday lending, and zero otherwise
Treat	Dummy variable equals one if the agency is located in the treated states, and zero otherwise
Post	Dummy variable equals one if the year is greater than or equal to the first adoption year of the treated states, and zero otherwise
Access_x_y	Dummy variable equals one if the center of the county is located within X and Y miles of a state that allows payday lending, and zero otherwise
Border	Dummy variable equals one if the center of the county is located 25 miles of a state border, and zero otherwise
Payday border	Dummy variable equals one if the county is located in a range of 15 miles from a state that allows payday lending, and zero otherwise
State characteristics	
Ln(GDP per capita)	Natural logarithm of GDP per capita (Federal Reserve Bank of St Louis)
Ln(Household income)	Natural logarithm of median household income (Federal Reserve Bank of St Louis)
Poverty rate	The ratio of the number of people (in a given age group) whose income falls below the poverty line (Federal Reserve Bank of St Louis)
Unemployment	The share of the labor force that is jobless, expressed as a percentage (Federal Reserve Bank of St Louis)
State characteristics	
Ln(Population)	Natural logarithm of county population (U.S Census Bureau)
Ln(Income per capita)	Natural logarithm of county income per capita (U.S Census Bureau)
Ln(Personal income)	Natural logarithm of county personal income (U.S Census Bureau)
Ln(No. of jobs)	Natural logarithm of the number of jobs offered in each county (U.S Census Bureau)
Native American	The proportion of the Native American population (NBER)
African American	The proportion of the African American population (NBER)
Asian American	The proportion of the Asian American population (NBER)
Latino American	The proportion of the Latino American population (NBER)
Democratic house	Dummy variable equals one if the majority of the statehouse is held by the democratic party (Ballotpedia)
Democratic senate	Dummy variable equals one if the majority of the senate is held by the democratic party (Ballotpedia)
Democratic governor	Dummy variable equals one if the governor is a Democratic party member (Ballotpedia)

Table A.1.

The sample starts in 1985 and ends in 2014. Many states have laws that effectively prohibit payday lending by imposing binding interest rate caps on payday loans or consumer loans. Some other states explicitly outlaw the practice of payday lending. These laws prohibiting or discouraging payday lending are generally well-enforced, if not always perfectly enforced (King and Parrish 2010), and hence provide a good source of variations in the availability of payday loans across states and time. My primary sources of those laws are the laws themselves such as statutes, superseded statutes, and session laws.

Table A.1.

Classifying payday lending laws, 1985–2014

State	Permitted at the start of the sample?	Change 1		Change 2		Change 3	
		Year	Type	Year	Type	Year	Type
AK	No	2004	Yes				
AL	No	1998	Yes				
AZ	No	2000	Yes	2006	No		
AR	No	1999	Yes	2001	No	2005	Yes
CA	No	1997	Yes				
CO	Yes						
CT	No						
DC	No	1998	Yes	2007	No		
DE	No	1987	Yes				
FL	Yes						
GA	No	2001	Yes	2005	No		
HI	No	1999	Yes				
ID	No	2001	Yes				
IL	No	2000	Yes				
IN	No	1990	Yes				
IA	No	1998	Yes				
KS	No	1991	Yes	2005	No		
KY	No	2009	Yes				
LA	No	1990	Yes				
ME	Yes						
MD	No						
MA	No						

MI	No	2005	Yes				
MN	No	1995	Yes				
MS	No	1998	Yes				
MO	No	2002	Yes				
MT	No	1999	Yes				
NE	No	1993	Yes				
NV	Yes						
NH	No	2003	Yes				
NJ	No						
NM	Yes						
NY	No						
NC	No	1997	Yes	2001	No		
ND	Yes	1997	No	2001	Yes		
OH	No	1995	Yes				
OK	No	2003	Yes				
OR	No	1998	Yes				
PA	No						
RI	No	2001	Yes				
SC	No	1998	Yes				
SD	No	1990	Yes				
TN	No	1990	Yes				
TX	No	2001	Yes	2005	No		
UT	No	1999	Yes				
VT	Yes	2001	No				
VA	No	2002	Yes	2005	No	2009	Yes
WA	No	1995	Yes	2005	No		
WV	No						
WI	Yes						
WY	No	1996	Yes				

Table 1 Descriptive statistics

This table reports the summary statistics of the variables used in this paper. The variables are the number of property crimes, the number of burglary crimes, the number of larceny crimes, the number of motor theft crimes, the number of violent crimes, the number of murder crimes, the number of rape crimes, the number of robbery crimes, the number of assault crimes; Allowed, dummy equals one if the state law does not prohibit the standard payday loan contract, and zero otherwise; Allowed_x_y, dummy equals one if the center of the county is located within X and Y miles of a state allowing payday lending, and zero otherwise; Border, dummy variable indicating whether the center of the county is located within 25 miles of the state border; The GDP per capita; The household income; The Poverty rate, percentage of household income below the federal poverty line; Unemployment, The share of the labor force that is jobless, expressed as a percentage; The county population; The county income per capita; The county personal income; The number of jobs offered in each county; Native American, The proportion of Native American population; African American, The proportion of African American population; Asian American, The proportion of Asian American population; Latino American, The proportion of Latino American population

Variable	N	Mean	Std. Dev	P25	Median	P75
Panel A. Crime						
Property crime	100,775	2,179.654	8,602.477	367	720	1,584
Burglary crime	100,775	500.418	1,979.748	70	154	359
Larceny theft crime	100,775	1,419.979	5,059.971	254	502	1,091
Motor theft crime	100,775	260.329	1,749.651	17	42	117
Violent crime	100,775	311.257	2,177.549	22	58	163
Murder	100,775	3.822	28.918	0	0	2
Rape	100,775	18.554	70.022	2	5	14
Robbery	100,775	108.644	1,120.3	3	10	35
Assault	100,775	186.715	1,139.556	13	37	107
Panel B. Payday lending regulation						
Allowed	100,775	0.468	0.499			
Access_0_30	100,775	0.501	0.500			
Access_30+	100,775	0.072	0.258			
Border	100,775	0.381	0.486			
Panel C. State-level characteristics						
GDP per capita	100,775	35,145.36	12,955.61	23,865	34,131	44,239
Household income	100,775	40,491.48	11,071.85	31,496	40,379	48,294
Poverty rate	100,775	0.128	0.032	0.107	0.127	0.155
Unemployment	100,775	6.129	1.920	4.800	5.800	7.200
Democratic	100,775	0.582	0.493			
Democratic Senate	100,775	0.523	0.499			
Democratic Governor	100,775	0.323	0.484			
Panel D. County-level characteristics						
Population	100,775	680,357.4	1406,684	84,789	250,432	694,808
Income per capita	100,775	29,869.9	12,706.79	19,995	27,741	37,098
Personal income	100,775	22,100	42,800	1,967	6,824	22,900
No. of jobs	100,775	401,990.8	819,316.6	42,452	133,250	411,682
White American	100,775	0.841	0.119	0.774	0.870	0.935
Native American	100,775	0.008	0.014	0.002	0.004	0.008
Asian American	100,775	0.033	0.040	0.007	0.018	0.039
African American	100,775	0.108	0.108	0.023	0.066	0.144
Latino American	100,775	0.103	0.143	0.019	0.045	0.138

Table 2 Uni-variate comparison

This table reports the univariate comparison for the sample. Panel A reports the univariate comparison of crimes between agencies located in states allowing payday lending and agencies located in states not allowing payday lending. Panel B reports the univariate comparison of control variables at the county level between agencies located in states allowing payday lending and agencies located in states not allowing payday lending. Panel C reports the univariate comparison of control variables on state-level between agencies located in states allowing payday lending and agencies located in states not allowing payday lending. The sample contains all crime information in the UCR program database originated during the calendar years 1985 through 2014.

	Allowed=1		Allowed=0		Difference	
	N=41,015		N=59,760			
Panel A. Crime	Mean	Median	Mean	Median	Mean	Median
Property crime	2,257.280	813.000	2,163.230	660.000	94.050***	153.000***
Burglary	505.804	142.000	498.107	171.000	7.698**	-29.000**
Larceny theft	1,496.730	576.000	1,392.950	459.000	103.780***	117.000***
Motor theft	268.174	48.000	255.883	39.000	12.290**	9.000**
Violent crime	314.874	71.000	314.286	51.000	0.588***	20.000***
Murder	4.036	1.000	3.679	0.000	0.357*	1.000*
Rape	20.792	7.000	17.368	4.000	3.424	3.000
Robbery	118.553	12.000	97.416	9.000	21.138**	3.000**
Assault	196.263	45.000	182.866	33.000	13.397*	12.000*

Table 3 Probit model regression

I run a Probit model regression to get a propensity score (P-score) for receiving treatment for each observation. The model is displayed as follows, $Allowed_{it} = \alpha_t + \beta_1 \times X_{it} + Year\ fixed\ effect + \varepsilon_{it}$ (1) , where $Allowed$ equals one if state i allow payday lending in year t, and zero otherwise. X_{it} are state-level control variables such as the natural logarithm of the state population (*Ln(state population)*), the natural logarithm of GDP per capita (*Ln(GDP per capita)*), the natural logarithm of household income (*Ln(Household income)*), *unemployment rate*, *poverty rate*, the proportion of minorities' population on the state level, the natural logarithm of crimes, and Democratic, a dummy to be 1 if the majority of the statehouse is controlled by the Democratic party. I include year-fixed effects in this model. I report the marginal effects. The standard errors are reported in the parentheses. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Allowed
	(1)
Ln(population)	-0.542***
	(0.644)
Ln(GDP per capita)	0188
	(0.953)
Ln(Household income)	-1.029**
	(1.467)
Unemployment	-0.016
	(0.059)
Poverty	-2.471
	(6.518)
White American	-82.527
	(229.733)
Native American	-83.474
	(230.763)
Asian American	-82.646
	(229.608)
African American	-83.061
	(229.739)
Ln(No. of crimes)	0.469***
	(0.568)
Democratic	-0.065
	(0.236)
Democratic Senate	0.024
	(0.224)
Democratic Governor	-0.009
	(0.149)
Year fixed effects	Yes
No. of observations	1,400
Pseudo R-squared	0.249

Table 4 Descriptive statistics for the matched sample

This table reports the summary statistics of the variables used in this paper. The variables are the number of property crimes, the number of burglary crimes, the number of larceny crimes, the number of motor theft crimes, the number of violent crimes, the number of murder crimes, the number of rape crimes, the number of robbery crimes, the number of assault crimes; Allowed, dummy equals one if the state law does not prohibit the standard payday loan contract, and zero otherwise; Allowed_x_y, dummy equals one if the center of the county is located within X and Y miles of a state allowing payday lending, and zero otherwise; Border, dummy variable indicating whether the center of the county is located within 25 miles of the state border; The GDP per capita; The household income; The poverty rate, percentage of household income below the federal poverty line; Unemployment, The share of the labor force that is jobless, expressed as a percentage; The county population; The county income per capita; The county personal income; The number of jobs offered in each county; Native American, The proportion of Native American population; African American, The proportion of African American population; Asian American, The proportion of Asian American population; Latino American, The proportion of Latino American population; Democratic, a dummy to be 1 if the majority of the statehouse is controlled by the Democratic party.

Variable	N	Mean	Std. Dev	P25	Median	P75
Panel A. Crime						
Property crime	37,695	2,401.116	9,242.779	384	788	1,749
Burglary crime	37,695	546.835	2,040.211	74	170	403
Larceny theft crime	37,695	1,538.184	5,368.235	264	536	1,182
Motor theft crime	37,695	316.140	2,012.753	20	49	143
Violent crime	37,695	361.086	2,526.395	25	67	184
Murder	37,695	4.540	32.899	0	1	2
Rape	37,695	19.637	74.478	2	6	15
Robbery	37,695	128.246	1,217.265	3	12	42
Assault	37,695	220.316	1,363.641	15	45	125
Panel B. Payday lending regulation						
Treat	37,695	0.511	0.499			
Post	37,695	0.562	0.496			
Panel C. State-level characteristics						
GDP per capita	37,695	32,139.56	9,269.338	24,787	31,490	38,816
Household income	37,695	38,194.51	8,582.36	31,855	37,715	44,005
Poverty rate	37,695	0.133	0.031	0.110	0.131	0.158
Unemployment	37,695	5.791	1.715	4.700	5.500	6.500
Democratic	37,695	0.686	0.464			
Democratic Senate	37,695	0.542	0.499			
Democratic Governor	37,695	0.373	0.482			
Panel D. County-level characteristics						
Population	37,695	817,430.9	1,720,942	85,473	260,812	781,265
Income per capita	37,695	27,110.65	10,391.72	19,495	25,012	32,227
Personal income	37,695	22,100	42,800	1,967	6,824	22,900
No. of jobs	37,695	473,111.3	976,935.5	41,922	141,083	456,522
White American	37,695	0.845	0.115	0.495	0.866	0.991
Native American	37,695	0.008	0.014	0.002	0.004	0.008
Asian American	37,695	0.034	0.040	0.007	0.018	0.042
African American	37,695	0.105	0.108	0.780	0.067	0.933
Latino American	37,695	0.107	0.142	0.021	0.047	0.140

Table 5 Baseline difference-in-differences

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 \times Treat_i \times Post_t + \beta_2 \times Treat_i + \beta_3 \times Post_t + \gamma \times X_{it} + \varepsilon_{it}$. The dependent variable in Columns (1) and (2) is Ln(property crimes), the dependent variable in Columns (3) and (4) is Ln(Burglary crimes), the dependent variable in Columns (5) and (6) is Ln(larceny crimes), and the dependent variable in Columns (7) and (8) is Ln(Motor theft crimes). Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Property crime)	Ln(Property crime)	Ln(Burglary)	Ln(Burglary)	Ln(Larceny)	Ln(Larceny)	Ln(Motor theft)	Ln(Motor theft)
Panel A.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	0.100**	0.134***	0.112**	0.130**	0.104**	0.139***	0.096	0.133**
	(0.045)	(0.043)	(0.050)	(0.053)	(0.042)	(0.039)	(0.066)	(0.056)
Treat	-0.119***	-0.144***	-0.088**	-0.104***	-0.134***	-0.159***	-0.139***	-0.170***
	(0.036)	(0.031)	(0.038)	(0.034)	(0.038)	(0.031)	(0.048)	(0.041)
Post	-0.076**	-0.098***	-0.055	-0.067*	-0.079**	-0.106***	-0.138***	-0.143***
	(0.034)	(0.030)	(0.034)	(0.034)	(0.035)	(0.029)	(0.043)	(0.036)
Ln(Population)		-2.411***		-2.216***		-2.694***		-1.223
		(0.863)		(0.762)		(0.908)		(1.057)
Ln(GDP per capita)		0.816**		0.685*		0.778*		0.901**
		(0.402)		(0.374)		(0.416)		(0.419)
Ln(Household income)		0.411		0.336		0.417		0.389
		(0.251)		(0.246)		(0.255)		(0.320)
Ln(Income per capita)		-2.482***		-2.596***		-2.494***		-2.272*
		(0.819)		(0.752)		(0.833)		(1.159)
Ln(Personal income)		1.650*		1.559*		1.786*		1.182
		(0.869)		(0.782)		(0.898)		(1.145)
Unemployment rate		0.062***		0.071***		0.055***		0.082***
		(0.019)		(0.017)		(0.019)		(0.019)
Poverty rate		0.886**		0.984**		0.793		0.317
		(0.423)		(0.423)		(0.471)		(0.557)
Ln(No. of jobs)		0.973***		0.847***		1.095***		0.493***
		(0.108)		(0.116)		(0.109)		(0.124)
African American		1.008**		1.053**		0.832*		1.809***
		(0.471)		(0.518)		(0.474)		(0.588)
Asian American		-0.630		-1.684**		-0.448		1.690
		(0.893)		(0.751)		(1.033)		(1.208)
Native American		-0.743		-0.149		-1.267		-0.612
		(1.293)		(1.812)		(0.977)		(2.350)
Democratic house		0.001		-0.014		0.006		-0.025
		(0.022)		(0.022)		(0.023)		(0.030)
Democratic Senate		-0.111***		-0.113***		-0.112***		-0.149***
		(0.036)		(0.034)		(0.037)		(0.041)
Democratic Governor		-0.006		-0.002		-0.006		0.015

		(0.027)		(0.026)		(0.027)		(0.032)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,445	37,695	42,445	37,695	42,445	37,695	42,445	37,695
Adjusted R-squared	0.132	0.215	0.202	0.255	0.107	0.191	0.139	0.287

	Ln(Property crime)	Ln(Property crime)	Ln(Burglary)	Ln(Burglary)	Ln(Larceny)	Ln(Larceny)	Ln(Motor theft)	Ln(Motor theft)
Panel B.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	0.088**	0.064*	0.105**	0.074*	0.091**	0.063**	0.084	0.043
	(0.040)	(0.035)	(0.045)	(0.043)	(0.036)	(0.031)	(0.062)	(0.047)
Treat	-0.085***	-0.071***	-0.062**	-0.044	-0.097***	-0.079***	-0.107**	-0.079**
	(0.028)	(0.023)	(0.028)	(0.027)	(0.030)	(0.022)	(0.045)	(0.034)
Post	-0.069**	-0.031	-0.052*	-0.015	-0.073**	-0.033*	-0.126***	-0.057*
	(0.029)	(0.018)	(0.029)	(0.023)	(0.030)	(0.018)	(0.041)	(0.029)
Ln(Population)		0.858***		0.679*		1.021***		1.090**
		(0.217)		(0.341)		(0.227)		(0.537)
Ln(GDP per capita)		0.316		0.248		0.293		0.320
		(0.300)		(0.311)		(0.311)		(0.333)
Ln(Household income)		0.078		0.098		0.080		0.133
		(0.174)		(0.163)		(0.181)		(0.237)
Ln(Income per capita)		-0.113		-0.173		0.016		-0.571
		(0.186)		(0.363)		(0.180)		(0.502)
Ln(Personal income)		0.144		0.108		-0.018		0.579
		(0.173)		(0.312)		(0.172)		(0.485)
Unemployment rate		0.049***		0.060***		0.041**		0.070***
		(0.015)		(0.015)		(0.016)		(0.016)
Poverty rate		0.704*		0.881*		0.625		0.204
		(0.386)		(0.452)		(0.400)		(0.544)
Ln(No. of jobs)		-0.005		-0.036		0.025		-0.154
		(0.129)		(0.131)		(0.134)		(0.180)
African American		8.573***		5.253		9.856***		1.539
		(3.123)		(3.579)		(3.198)		(3.276)
Asian American		-4.777***		-4.334***		-4.655***		-6.499***
		(1.024)		(1.509)		(1.157)		(2.007)
Native American		-3.750		-8.598*		1.292		-11.950
		(4.214)		(4.854)		(3.733)		(7.360)
Democratic		-0.002		-0.014		0.003		-0.027
		(0.021)		(0.021)		(0.021)		(0.024)
Democratic Senate		-0.064**		-0.079**		-0.060**		-0.090***
		(0.026)		(0.029)		(0.027)		(0.026)
Democratic Governor		0.000		-0.009		0.002		0.001
		(0.018)		(0.020)		(0.018)		(0.022)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,445	37,298	42,445	37,298	42,445	37,298	42,445	37,298
Adjusted R-squared	0.92	0.929	0.92	0.926	0.909	0.919	0.921	0.929

Table 6 Placebo tests

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \gamma X_{it} + \varepsilon_{it}$. The dependent variable in Columns (1) and (2) is Ln(Violent crimes), the dependent variable in Columns (3) and (4) is Ln(Murder crimes), the dependent variable in Columns (5) and (6) is Ln(Rape crimes), the dependent variable in Columns (7) and (8) is Ln(Robbery crimes), and the dependent variable in Columns (9) and (10) is Ln(Assault crimes). Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Violent crime)	Ln(Violent crime)	Ln(Murder)	Ln(Murder)	Ln(Rape)	Ln(Rape)	Ln(Robbery)	Ln(Robbery)	Ln(Assault)	Ln(Assault)
Panel A.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	0.057	0.039	-0.035	-0.036	0.065*	0.047	0.000	0.040	0.050	0.019
	(0.044)	(0.042)	(0.028)	(0.024)	(0.036)	(0.033)	(0.059)	(0.047)	(0.044)	(0.042)
Treat	-0.081*	-0.069**	0.011	0.010	-0.071*	-0.056*	-0.039	-0.062**	-0.087*	-0.067
	(0.043)	(0.034)	(0.016)	(0.014)	(0.037)	(0.029)	(0.035)	(0.030)	(0.050)	(0.042)
Post	-0.042	-0.043	0.040**	0.038**	-0.013	-0.033	0.009	-0.021	-0.048	-0.039
	(0.035)	(0.033)	(0.015)	(0.016)	(0.022)	(0.021)	(0.039)	(0.028)	(0.040)	(0.037)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,108	37,358	42,108	37,358	42,108	37,358	42,108	37,358	42,108	37,358
Adjusted R-squared	0.179	0.243	0.119	0.187	0.118	0.162	0.160	0.310	0.200	0.244
	Ln(Violent crime)	Ln(Violent crime)	Ln(Murder)	Ln(Murder)	Ln(Rape)	Ln(Rape)	Ln(Robbery)	Ln(Robbery)	Ln(Assault)	Ln(Assault)
Panel B.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	0.044	-0.009	-0.037	-0.032	0.055	-0.012	-0.014	0.012	0.040	-0.028
	(0.045)	(0.038)	(0.029)	(0.020)	(0.036)	(0.037)	(0.061)	(0.042)	(0.046)	(0.044)
Treat	-0.041	-0.001	0.028***	0.017	-0.046	0.010	0.004	-0.009	-0.051	0.009
	(0.028)	(0.028)	(0.009)	(0.013)	(0.031)	(0.032)	(0.030)	(0.028)	(0.034)	(0.034)
Post	-0.036	0.009	0.039**	0.037**	-0.012	0.019	0.019	0.016	-0.043	0.017
	(0.034)	(0.026)	(0.016)	(0.015)	(0.023)	(0.025)	(0.040)	(0.022)	(0.039)	(0.032)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	42,108	36,961	42,108	36,961	42,108	36,961	42,108	36,961	42,108	36,961
Adjusted R-squared	0.912	0.919	0.802	0.805	0.836	0.839	0.930	0.934	0.877	0.887

Table 7 Account for contemporaneous local shocks at the state level

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 Access + \gamma X_{it} + \varepsilon_{it}$ (4) . The dependent variable in Columns (1) and (2) is Ln(property crimes), the dependent variable in Columns (3) and (4) is Ln(Burglary crimes), the dependent variable in Columns (5) and (6) is Ln(larceny crimes), and the dependent variable in Columns (7) and (8) is Ln(Mother theft crimes). $Access\_X\_Y_{ct}$ is a county-level indicator that equals one if the center of the county is located within X and Y miles of a state allowing payday lending and zero otherwise. All regressions include state×year fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Property crime)	Ln(Property crime)	Ln(Burglary crime)	Ln(Burglary crime)	Ln(Larceny crime)	Ln(Larceny crime)	Ln(Motor theft crime)	Ln(Motor theft crime)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Access_0_30	0.174**	0.169*	0.062	0.060	0.049	0.075**	0.027	0.040
	(0.084)	(0.097)	(0.047)	(0.052)	(0.067)	(0.033)	(0.104)	(0.076)
Access_30_40	0.062	0.076	0.073	0.083	-0.059	0.023	-0.059	0.027
	(0.144)	(0.140)	(0.120)	(0.117)	(0.130)	(0.089)	(0.158)	(0.107)
Border		0.101		0.053		0.012		0.162**
		(0.083)		(0.060)		(0.053)		(0.064)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
State×Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	122,714	100,603	122,750	100,639	122,732	100,621	122,734	100,623
Adjusted R-squared	0.137	0.216	0.193	0.251	0.118	0.194	0.168	0.296

Table 8 Financial pressure

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 \times Treat_i \times Post_t + \beta_2 \times Treat_i + \beta_3 \times Post_t + \gamma \times X_{it} + \varepsilon_{it}$. for subsample split by the state-median of household income, income per capita, employment rate, and poverty rate. The dependent variable in Columns (1) and (2) is Ln(property crimes), the dependent variable in Columns (3) and (4) is Ln(Burglary crimes), the dependent variable in Columns (5) and (6) is Ln(larceny crimes), and the dependent variable in Columns (7) and (8) is Ln(Motor theft crimes). Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Panel A. Household income	Ln(Property crime) Low	Ln(Property crime) High	Diff in (1)	Panel B. Income per capita	Ln(Property crime) Low	Ln(Property crime) High	Diff in (1)
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.347***	-0.067*	0.414***	Treat×Post	0.169***	0.074	0.095*
	(0.056)	(0.036)	P=0.0003		(0.052)	(0.057)	P=0.0943
Treat	-0.213***	0.014		Treat	-0.165***	-0.123***	
	(0.068)	(0.039)			(0.035)	(0.035)	
Post	-0.224***	0.037		Post	-0.098***	-0.085*	
	(0.044)	(0.041)			(0.03)	(0.044)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	17,097	20,598		No. of observations	17,392	20,303	
Adjusted R-squared	0.185	0.248		Adjusted R-squared	0.248	0.216	
Panel C. Unemployment	Ln(Property crime) Low	Ln(Property crime) High	Diff in (1)	Panel D. Poverty	Ln(Property crime) Low	Ln(Property crime) High	Diff in (1)
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.094**	0.201**	-0.107*	Treat×Post	0.039	0.207**	-0.168
	(0.040)	(0.075)	P=0.081		(0.047)	(0.092)	P=0.201
Treat	-0.044	-0.237***		Treat	-0.038	-0.153	
	(0.030)	(0.074)			(0.053)	(0.113)	
Post	-0.071***	-0.181***		Post	-0.041	-0.119	
	(0.019)	(0.057)			(0.037)	(0.081)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	21,231	16,464		No. of observations	20,424	17,271	
Adjusted R-squared	0.213	0.198		Adjusted R-squared	0.197	0.188	

Table 9 Access to commercial banks

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \gamma X_{it} + \varepsilon_{it}$. For subsample split by the state-median of no. of bank branches. The dependent variable in Columns (1) and (2) is Ln(property crimes. Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Ln(Property crime)	Ln(Property crime)	Diff in (1)
Bank branches	Low	High	
	(1)	(2)	(3)
Treat×Post	0.126**	0.115	0.014
	(0.054)	(0.070)	P=0.129
Treat	-0.147***	-0.130***	
	(0.038)	(0.046)	
Post	-0.108***	-0.088*	
	(0.037)	(0.047)	
Control variables	Yes	Yes	
Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes	
No. of observations	14,799	22,896	
Adjusted R-squared	0.234	0.201	

Table 10 Minority population

This table reports the OLS estimation results of $Crime_{it} = \alpha_i + \alpha_t + \beta_1 \times Treat_i \times Post_t + \beta_2 \times Treat_i + \beta_3 \times Post_t + \gamma \times X_{it} + \varepsilon_{it}$. for subsample split by the state-median of the proportion of minority populations. The dependent variable in Columns (1) and (2) is Ln(property crimes. Treat equals one if the agency is a treated state, and zero otherwise. Post equals one if the agency-year observation is after the payday lending adoption. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

Panel A. African American	Ln(Property crime) High	Ln(Property crime) Low	Diff in (1)	Panel B. Native American	Ln(Property crime) High	Ln(Property crime) Low	Diff in (1)
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.146***	0.097*	0.049*	Treat×Post	0.089	0.095*	-0.006
	(0.048)	(0.051)	P=0.086		(0.062)	(0.052)	P=0.471
Treat	-0.171***	-0.124***		Treat	-0.103**	-0.084*	
	(0.030)	(0.034)			(0.044)	(0.049)	
Post	-0.101***	-0.099**		Post	-0.053	-0.065**	
	(0.029)	(0.038)			(0.041)	(0.032)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	19,839	17,856		No. of observations	17,943	20,816	
Adjusted R-squared	0.183	0.245		Adjusted R-squared	0.261	0.184	
Panel C. Asian American	Ln(Property crime) High	Ln(Property crime) Low	Diff in (1)	Panel D. Latino American	Ln(Property crime) High	Ln(Property crime) Low	Diff in (1)
	(1)	(2)	(3)		(1)	(2)	(3)
Treat×Post	0.03	0.158**	-0.128**	Treat×Post	0.118*	0.093*	0.025
	(0.044)	(0.062)	P=0.047		(0.063)	(0.050)	P=0.243
Treat	-0.057	-0.163***		Treat	-0.150***	-0.112***	
	(0.039)	(0.054)			(0.041)	(0.030)	
Post	-0.027	-0.102**		Post	-0.078*	-0.085**	
	(0.031)	(0.038)			(0.043)	(0.033)	
Control variables	Yes	Yes		Control variables	Yes	Yes	
Year fixed effects	Yes	Yes		Year fixed effects	Yes	Yes	
State fixed effects	Yes	Yes		State fixed effects	Yes	Yes	
No. of observations	20,788	17,971		No. of observations	16,956	20,739	
Adjusted R-squared	0.223	0.211		Adjusted R-squared	0.168	0.272	

Figure 1

Figure 1 plots the coefficient estimates and their 95% confidence intervals of $Treat \times Year_k$ for equation (3). $Property\ crime_{it} = \alpha_i + \alpha_t + \alpha_j + \sum_{k=-3}^{k=3} \beta_k \times Treat \times Year_k + \gamma \times X_{it} + \varepsilon_{it}$ (3) , The dependent variable is *Ln (No. of property crimes)*. all variables are defined the same as those in equation (2), except for *Year*, which equals one if the $Year_k$ is k years after the adoption year, and zero otherwise. All regressions include year effects and state (agency) fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

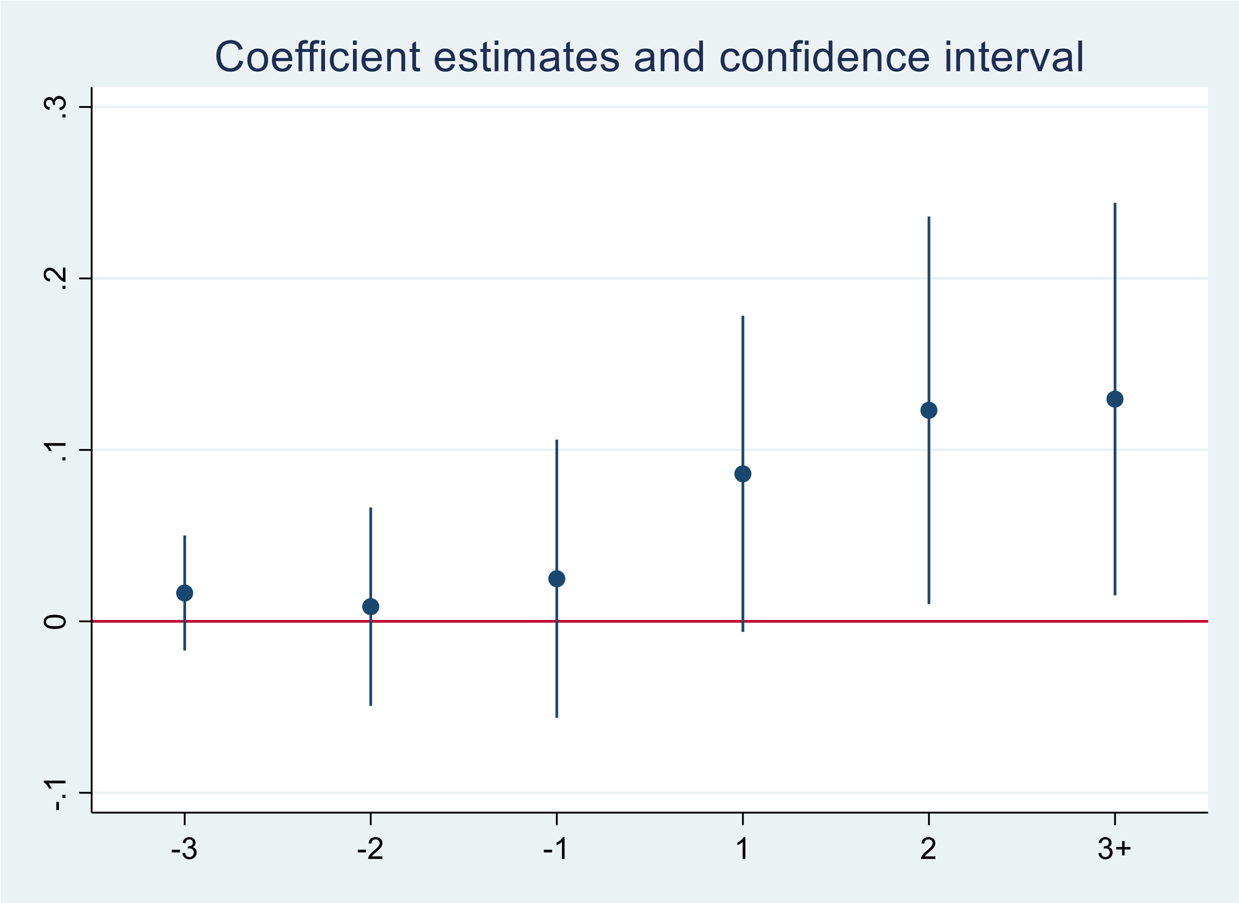

Figure 2

Figure 2 plots the dynamic coefficient estimates and their 95% confidence intervals of *Access_0_30*. The dependent variable is *Ln (No. of property crimes)*. All variables are defined the same as those in equation (2). All regressions include State×year fixed effects. Standard errors are clustered by state. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.