

# Credit Access and Household Well-being: Evidence from Payday Lending\*

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## Abstract

How does gaining access to expensive credit affect the well-being of credit-constrained households? I use plausibly exogenous zip code-level variation in the temporal accessibility of payday loans to examine the causal effects of access to payday loans on household well-being. Using suicide attempts and deaths as a measure for household distress, I find detrimental effects from payday loans; that is, having access to payday loans substantially increases suicide risk. The dynamic analyses show that there is no existing trend during the pre-payday periods; however, a sharp increase in attempted suicides emerges only after gaining access to payday loans. Further analyses show that the effects are significant only among people who are effectively eligible for payday loans—the employed and those with private insurance—especially in zip codes with a high share of finance-constrained households. Finally, increased suicide risk in zip codes with access to payday loans appears to be related to mental health deterioration from financial distress.

*JEL Codes:* D14, D18, G23, G28, I31

*Keywords:* Credit Access, Payday Loan, Household Finance, Well-being, Suicide Risk

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# 1 Introduction

Does expanding access to expensive credit improve the well-being of borrowers? Traditional economic theories suggest that credit allows households to smooth consumption over their life cycles and to invest in both physical capital and human capital. In this view, high-cost credit, such as payday loans, should not cause social welfare losses as long as the market is highly competitive and borrowers and lenders understand the risks and costs involved in the transactions. Thus, a high interest rate is just a trade-off between present consumption over future consumption. In recent decades, traditional models with symmetric information and rational, time-consistent borrowers have been challenged by a radically different alternative called behavioral economics. This view offers extensive examples of consumers' divergence from rationality owing to behavioral bias, for example, self-control problems, time-inconsistent preferences, over-optimism, over-confidence, narrow framing, and present bias.<sup>1</sup>

Similarly to the ambiguous predictions from economic theories, empirical evidence is mixed on the effects on consumer welfare of having access to high-cost credit. With respect to payday loans, a strand of research finds evidence for positive effects of credit supply on household well-being (Zaki, 2016; Parsons and Van Wesep, 2013; Morse, 2011; Morgan et al., 2012; Morgan and Strain, 2008). On the other hand, other studies report negative effects (Zinman, 2010; Agarwal et al., 2009; Carrell and Zinman, 2014; Skiba and Tobacman, 2009; Morgan et al., 2012; Melzer, 2011), while yet others find null effects (Bhutta, 2014; Bhutta et al., 2015).

Overall, there is a lack of consensus in both theoretical and empirical research on whether expanded credit to financially constrained households does more good than harm, especially when expensive credit, such as payday loans, is at the center of the discussion. The main goal of this study is to empirically test whether access to expensive loans benefits or harms

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<sup>1</sup>See Zinman (2014) for a review of theoretical work on household debt.

borrowers, with a particular interest in the measure for the longer-term well-being of borrowers, which has not been examined in the literature. Such an analysis that aims to establish a causal link from credit supply, or access to credit, to borrowers' well-being faces several challenges. This is because omitted factors could potentially affect both access to credit and well-being of borrowers. One such challenge is location choices by payday lenders. Payday lenders choose to enter a geographical market where high demand for such short-term loans is present or expected, that is, locations where a large proportion of households are credit-constrained and demand for credit is not satisfied by mainstream financial institutions. Examples of such locations include military bases (Carrell and Zinman, 2014; Carter and Skimmyhorn, 2016; Flannery and Samolyk, 2005) and areas with a large share of low- to moderate-income households (Melzer, 2011) and minority groups (King et al., 2005).

The second challenge is regulations on financial institutions. A state's regulatory forces might endogenously react in multiple ways to the state's economic conditions and the availability and scope of the local social welfare system. In addition, lobbying efforts by alternative financial services providers, such as payday lenders, are an endogenous factor. Lobbying incentives by payday lenders are based on the profitability of and demand for payday loans, and lobbyists have the ability to influence payday-related bills in state legislatures.<sup>2</sup> These factors tend to have disproportional impacts on financially constrained populations who are likely to have high demand for emergency loans. If any of the omitted variables are at play—that is, the availability of payday loans is correlated with unobserved regional factors that potentially affect the well-being of borrowers—then the estimates of our models will be biased.

To address the endogeneity concerns of credit access, I use an identification strategy that isolates the variation in access to credit from potentially endogenous state and local economic conditions. This strategy, similar to that in Melzer (2011), uses the proximity of zip codes in

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<sup>2</sup>Not surprisingly, Fox and Mierzwinski (2000) find intense lobbying activities by lobbyists hired by payday lenders in the late 1990s and report that lobbying strategy seems to be working to enact financial services providers' preferred version of legislation.

states that ban payday loans (“payday-banning states” hereafter) to the borders of states that allow payday loans (“payday-allowing states” hereafter). In addition, this strategy compares the changes in household well-being between zip codes with and without close proximity to payday-allowing states, before and after the emergence of payday lending in the neighboring states.<sup>3</sup> The idea behind this identification strategy is quite simple. Residents in zip codes living close to a neighboring payday-allowing state can easily cross the border to take out payday loans because travel costs are low. However, those who live far from the border have little incentive to cross the border to borrow \$500 from payday lenders, because the total travel costs increase proportional to the distance and time traveled. Therefore, this strategy mitigates endogeneity issues by focusing on changes in the variation in credit availability of zip codes in states that never allowed payday lending but have a contiguous state with payday stores that have rapidly emerged.

For the measure of borrowers’ well-being, I use suicide risk. Both suicide attempts and deaths are extreme cases of household distress. According to a theoretical model in Daly et al. (2013), individuals have different thresholds for suicide, and any negative shocks to happiness (e.g., unemployment and financial difficulties) can cause an individual to locate below his or her life satisfaction threshold for suicide. In addition, Hamermesh and Soss (1974) develop an economic theory of suicide that explains why people commit suicide. According to the model, an individual might end his or her life if the total discounted lifetime utility falls below zero. Suicide risk in this sense is a measure of longer-term changes in well-being (or distress) compared to measures of short- or mid-term changes in well-being, which are used in the existing empirical literature (e.g., bank overdrafts, late bill payments,

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<sup>3</sup>While the core idea is similar to that of other studies that use the identification strategy in Melzer (2011), a major improvement of this research’s identification strategy is that it uses zip code instead of county as the geographic unit. As explained in a later section, accurately measuring the shortest distance of a zip code to the border of neighboring states that allow payday loans is at the heart of this empirical identification strategy. By using zip codes rather than county codes, which might assign somewhat erroneous distance-to-border measures, I minimize measurement errors in the construction of the essential variable in my empirical models. Moreover, complemented by zip code-level demographic information from the census data (e.g., household incomes, poverty rates, and education levels), my strategy allows for much finer identification of credit-constrained households.

and personal bankruptcies), which evaluate only current or near future utilities. A suicide attempt might be the optimal choice for some people with persistent distress whose stream of future expected utilities is dismal.

The findings of this study favor the view that access to dangerous credit adversely affects consumers' long-term well-being. Gaining access to payday loans substantially increases suicide attempts by 10%. The baseline finding is supported by additional analyses that take advantage of the main eligibility criterion for payday loans—employment of applicants.<sup>4</sup> The effects of access to payday loans on suicide risks are significant only among those aged under 65 years, while the elderly person who is less likely or unlikely to be eligible for payday loans does not experience increased suicide attempts. Moreover, only people with private insurance or the employed increase suicide attempts and I find no significant effects for the uninsured or beneficiaries with Medicare and Medicaid. Further analyses show that zip codes with higher demand for payday loans experience significantly larger increases in attempted suicides. Finally, the results from a diverse set of robustness tests and a placebo test suggest that the relationship between access to payday loans and increased suicide attempts is not coincidental.

Overall, the most conservative estimate, 10%, implies there were an additional 5.5 suicide attempts per 100,000 people, which, if converted to the national level, would amount to an additional 15,000 suicide attempts in 1998. As such, the results suggest there is a large social cost from increased suicide attempts (and suicide deaths) caused by gaining access to payday loans. At a minimum, suicide attempts incur annual medical costs of \$142 million in the late 1990s and early 2000s.<sup>5</sup>

How is gaining access to this expensive credit linked to increased suicide risks? First, payday loans cause financial distress for many consumers. It is apparent that a significant

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<sup>4</sup>Although unemployed retirees who are mainly on Social Security might still qualify for payday loans, not all payday loan lenders accept applications from such unemployed retirees, possibly because there are no wages to be garnished from them they were to miss payments. With these increased costs and possible stricter screening for retired applicants, there would be much less incentive for the elderly to travel to a payday lender.

<sup>5</sup>In the sample used in this study, the average medical charge per attempted suicide is \$9410.

share of payday borrowers becomes trapped in financial distress: 80% of loans are not paid off by the first due date and, as Figure 1 shows, 50% of all loans become part of long sequences of more than 10 loans (CFPB, 2014).<sup>6</sup> Second, excessive debts, in turn, could lead to deterioration in the mental health of borrowers through stress. A growing body of literature suggests that stress is associated with deterioration in both physical and mental health (McEwen, 1998; Cooper, 2004; Schneiderman et al., 2005). If financial distress increases overall stress level, we expect that changes in household balance sheets will affect health and healthcare usage as well. Indeed, studies find support for this link (e.g., Gross and Tobacman (2014); Evans and Moore (2011); Dobkin and Puller (2007); Parker et al. (2013); Currie and Tekin (2015); Deaton (2011)). Thus, if excessive financial debts adversely affect mental or emotional health, payday borrowers can develop suicidal ideation.

This research provides evidence that mental health plays a role in explaining the link between financial distress and increased suicide risk. Payday borrowers who suffer from mental well-being deterioration might consult a doctor in outpatient settings to obtain prescription antidepressants. Emotional and mental instability caused by financial problems can exacerbate borrowers' suicide risks and, as a result, people in such circumstances might choose prescription drugs as a method to commit suicide, as antidepressants are within quick and easy reach before their suicidal impulses diminish. The results from a test for the mental health explanation confirm this hypothesis; suicide attempts by poisoning with antidepressants—drugs used for the treatment of mental instability—significantly increase after gaining access to payday loans. This effect is prominent only for those aged under 65 years, who are the main consumers of payday loans. Although my data do not allow for an examination of the exact responses of the number of antidepressant prescriptions on the intensive margin (existing patients who had been taking antidepressants before obtaining access to payday loans) and the extensive margin (new patients who started to take antide-

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<sup>6</sup>The right tail in the distribution of payday loan sequences in Figure 1 is important, especially because borrowers with those loans would have paid more than twice as much fees as the original loan amount. For example, since it costs on average \$100 per renewal (rollover) on a \$500 loan, 10 or more renewals would amount to \$1,000 or more.

pressants owing to payday loans), the stark increase in suicide attempts by poisoning with psychotropic drugs is unlikely to be fully driven by the patients who already had mental distress.<sup>7</sup>

The findings of this study relate to the extensive literature examining the effects of access to payday loans on diverse economic and welfare outcomes. To the best of my knowledge, the use of the measure of long-term well-being—suicide risks—as well as mental health explanation is new to the literature. In addition, using zip-level data in the construction of the distance-to-borders measure, this research reduces measurement errors compared to county- or state-level distance measures used in prior studies.

Furthermore, this study is related to a growing body of work that evaluates how financial and economic conditions are linked to health, especially suicide. Studies identify unemployment and permanent income shocks as the two main economic factors that contribute to the risk of suicide (Henry and Short, 1954; Hamermesh and Soss, 1974; Kposowa, 2001; Mortensen et al., 2000). In addition, the existing literature finds that, unlike mortalities from other causes, suicide is the only exception that is highly counter-cyclical; in other words, only suicide mortalities increase during recessions (Ruhm, 2000; Luo et al., 2011; Gerdtham and Johannesson, 2003; Granados, 2005; Evans and Moore, 2011). The findings of my study complement the previous research, in that I provide evidence for a mental health channel through which financial distress can affect the risks of suicide.

The rest of the paper proceeds as follows. Section 2 provides background on the payday lending industry and briefly reviews theories and the previous empirical literature. Section 3 introduces the data and describes the empirical strategy. Section 4 presents the empirical results. Finally, Section 5 concludes.

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<sup>7</sup>To the best of my knowledge, for this research’s period of interest, no data are available that contain drug prescription or usage information at the individual level in outpatient and physician’s office settings.

## 2 Institutional Background and Literature Review

### 2.1 Background on Payday Lending Industry

A payday loan is a short-term, high-interest consumer loan with maturity period of 2 weeks. The common loan amount is less than \$500 with an associated fee of \$20 per \$100 advanced. During the underwriting process, the lender typically verifies the borrower’s identification, bank account, record of previous pay stubs, and personal information. If approved by the lender, the borrower writes a post-dated check for the principal amount plus fees. While a payday loan has a fixed 2-week maturity, rollovers are common.<sup>8</sup>

The payday lending industry has been hotly debated since its inception, due to the usurious interest rates charged by lenders and frequent rollovers by borrowers and this controversial industry remains under scrutiny today.<sup>9</sup> Proponents of this high-cost, short-term credit argue that payday loans help desperate borrowers solve temporary cash flow issues by providing easy-to-obtain liquidity, which otherwise would not be accommodated by mainstream lenders. Opponents, however, are concerned that payday loans can be harmful, becoming an endless “debt-trap” owing to high fees and rollover feature.<sup>10</sup>

The payday lending industry emerged in the mid-1990s. A series of banking deregulations in the 1980s and 1990s increased the competitive environment in mainstream banking. Competition incentivized many financial institutions to exit the market for less-profitable short-term loans and instead to focus more on profitable longer-term loans. This gap between demand for and supply of short-term credit has since been filled by alternative financial services providers, such as payday lenders.<sup>11</sup> Before the mid-1990s, there were only a small

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<sup>8</sup>Rolling-over a loan is in practice identical to taking out a new loan with an incremented amount. For example, if a borrower takes out a first loan for \$100, she would write a check for \$120, including the fees of \$20. If this loan were to be rolled over, she would then write a new check, with the first check voided, for \$140, which is the original check amount plus additional fees of \$20.

<sup>9</sup>An average payday borrower who renews a loan of \$400 eight times would end up with more finance fees, \$640, than the original loan amount, \$400.

<sup>10</sup>CFPB (2014) finds that the majority of payday loans—four out of five—are rolled over within the 2-week period.

<sup>11</sup>See Caskey (2005) and Barr (2004) for more detailed discussion of the background on the emergence of



number of check cashers, pawn shops, and other alternative financial services providers participating in cashing paychecks, but thereafter, a skyrocketing number of outlets began to engage in payday lending, resulting in explosive growth rates. Exponential growth was observed across the nation (Fox, 1998, 1999; Barr, 2004; Caskey, 2005; Stegman, 2007; Melzer, 2011). For instance, in Indiana, one of the few states that collected state-wide statistics on payday lenders, the number of payday lenders skyrocketed five-fold and the loan volume eight-fold (from \$12 million to \$98 million) in 2 years (Fox, 1998). Another example is a Tennessee payday loan company, Check Into Cash, which reported a three-fold increase in loan volume in 1996 from the previous year.<sup>12</sup> As a result of the exploding number of payday lenders, payday outlets became more prevalent than even McDonald's and Burger King combined: there were more than 22,000 payday stores in 2004 versus 21,000 combined McDonald's and Burger King branches (Karger, 2005; Barr, 2004).

New York and New Jersey banned payday lending during the entire sample period of this study from 1994 to 2000. As Figure 2 shows, among the neighboring states of New York and New Jersey, only Delaware and Pennsylvania allowed payday lending in this period. In Delaware, licensed non-depository lenders were allowed to lend without any cap on interest rate (5 Del. C. § 2201-2244). According to Delaware's Office of the State Banking Commissioner, the first appearance of payday lending was E Z Cash of Delaware, Inc. in July 1998. Throughout the sample period, Pennsylvania imposed bans on payday lending using a rate ceiling that lenders can charge on a loan (P.A. 7 P.S. § 6201-6219). Lenders in Pennsylvania, however, successfully evaded state small loan and usury laws based on a legislative loophole that permits brokering loans (P.A. 73 P.S. § 2181-2192). In other words, they formed partnerships with chartered financial institutions, which were exempt from usury laws and small loan laws, to offer small loans and claimed themselves to be "brokers" so as to avoid regulatory restrictions. Several reports and previous studies indicate that payday lenders did not

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the payday lending industry

<sup>12</sup>See Fox et al. (1997) and Fox (1998) for more examples on how fast the payday lending industry grew after the mid-1990s.

begin their operations in Pennsylvania before 1997 (Fox, 1998; Melzer, 2011; Brickley, 1999). In all other neighboring states to New York or New Jersey (i.e., Connecticut, Massachusetts, and Vermont), payday loans were effectively prohibited for the entire sample period.

The rise of payday lending in Delaware and Pennsylvania provides a good source of temporal and geographic variations in access to payday loans to borrowers who reside in the payday-banning states of New York and New Jersey. Because only those who live close enough to the payday-allowing states gain access to payday loans by crossing the borders, those who live “economically” far from the borders of those states would not gain access. Therefore, this study’s identification strategy takes advantage of the natural experiment in the supply of payday loans in order to identify the causal effects of gaining access to short-term, high-interest loans on borrowers’ distress.

## **2.2 Suicide Attempts and Mortalities as Measures of Household Distress**

Economists recognize that a subjective well-being measure can be useful in the analysis of consumer preferences and social welfare. However, unlike researchers outside economics, such as psychologists, who have long been undertaking subjective well-being research, economists have begun to undertake empirical research in this area only in recent decades. The main reason for the late development is economists’ skepticism about the quality of subjective well-being data. Critics argue that self-reported measures commonly used in the subjective well-being research are not reliable, because respondents of a survey can answer quite differently depending on the wording and order of the survey questions; respondents can have an ambiguous understanding of what happiness and life satisfaction really mean in the questionnaires; and the attitudes of survey participants can be non-coherent (Deaton and Stone, 2013; Kahneman and Krueger, 2006; Daly et al., 2013; Bertrand and Mullainathan, 2001; Wilkinson, 2007; Daly et al., 2011; Krueger and Schkade, 2008).<sup>13</sup>

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<sup>13</sup>Among studies that report the divergence between self-reported measures of subjective well-being and revealed preferences, an interesting and relatively recent one is Benjamin et al. (2014). They survey U.S.

To overcome these unreliability concerns of self-reported well-being measures, I use suicide attempts and mortalities as a measure of distress or negative subjective well-being. As Oswald (1997) states, suicide data offer “intrinsically more compelling” information about individuals’ responses to unhappiness than survey participants’ answers to questionnaires, and cannot be performed in an experiment setting. Thus, suicide risk has been studied in both theoretical and empirical research. According to the theoretical model in Daly et al. (2013), individuals have different thresholds for suicide, and any gradual or abrupt changes in happiness can cause them to locate substantially below their suicide thresholds.<sup>14</sup> In addition, Hamermesh and Soss (1974) develop an economic theory of suicide that explains why people commit suicide. According to the model, an individual takes his or her life if the discounted total lifetime utility falls below zero. With increased risk of suicide, people might commit suicide. A suicide attempt or death by an individual, therefore, is a revealed choice after a thorough examination of the utility of continuing one’s life.

Consistent with the theory, empirical research has found considerable association between suicide and subjective well-being, and researchers have used suicide as a reliable measure of well-being. Using cross-country panel data, Helliwell (2007) shows a negative association between subjective well-being and suicide.<sup>15</sup> Stevenson and Wolfers (2006) examine the effects of unilateral divorce law on suicide, as well as other measures of family distress, such as domestic violence and spousal homicide, and conclude that female suicide declined by 8% to 16%. Daly et al. (2011, 2013) use suicide to test whether the relative status of being poorer or less happy relatively to others in a neighborhood affects suicide risk. Instead of relying on unreliable subjective measures, it would be more reliable to use the revealed outcomes to understand the well-being of populations.

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medical students who are enrolled in the National Residence Matching Program and find stark differences between actual residents’ choices and students’ subjective well-being rankings.

<sup>14</sup>Although negative shocks to happiness change only the suicidal outcomes of marginal people who become located below their suicide thresholds, research shows that the general population whose members are well above their suicide thresholds also shifted down in the happiness distribution (Daly and Wilson, 2009).

<sup>15</sup>Case and Deaton (2015) find no association between suicide and life satisfaction. Using the same data, however, I find a negative correlation between them. The difference between their and my analyses come from the inclusion of self-evaluation about one’s future life (Lee, 2017).

Moreover, suicide risk is a better measure of the changes in the long-term well-being of a population. The outcome variables employed in the existing literature are mostly measures of temporary or transitory states of well-being; examples include foreclosures (Morse, 2011), bounced checks (Morgan et al., 2012; Morgan and Strain, 2008), bankruptcies (Morgan and Strain, 2008; Skiba and Tobacman, 2009; Morgan et al., 2012), bank overdrafts and late bills (Zinman, 2010; Melzer, 2011), credit card delinquency (Agarwal et al., 2009), and job performance (Carrell and Zinman, 2014). Among them, the most long-term welfare consequence is personal bankruptcy. Even bankruptcy does not have significantly lasting effects on the filers, mainly because its purpose is to provide debt relief, or a fresh start, for those who are in severe financial trouble.<sup>16</sup> Suicide risk, however, is a measure of long-term well-being, because a person has to examine his or her stream of current and future expected utilities before making a suicide decision. Put differently, even though an individual might encounter temporary financial distress, if he or she sees a brighter near future, this person would not develop suicidal ideation. On the other hand, a person could be suicidal if he or she expects a consistent and permanent dismal future.

In addition to the reliability and long-term measure advantage, suicide risk is an important measure of well-being in the field of household finance. First, a suicide decision is closely related to changes in a household's financial circumstances, which are often exacerbated by negative shocks, such as personal emergencies and unemployment. In particular, the suicide literature has identified that unemployment and negative permanent income shocks are the two main economic factors that contribute to the risk of suicide (Henry and Short, 1954; Hamermesh and Soss, 1974; Kposowa, 2001; Mortensen et al., 2000; Evans and Moore, 2012; Ruhm, 2000; Granados, 2005; Gerdtham and Johannesson, 2003; Pierce and Schott, 2015). Moreover, recent empirical studies confirm that, unlike mortalities from other causes that are pro-cyclical, suicide is an exception that is highly counter-cyclical, meaning that there

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<sup>16</sup>The bankruptcy record is deleted after 7 or 10 years from the filing date of the bankruptcy, depending on the chapter of bankruptcy. However, the recovery of credit takes a relatively shorter amount of time (Jagtiani and Li, 2015); bankruptcy filers continue to receive credit card solicitations, though with inferior terms and conditions (Han et al., 2015); and the filers continue to work similar hours (Han and Li, 2007).

are more suicide mortalities during recessions (Ruhm, 2000; Luo et al., 2011; Gerdtham and Johannesson, 2003; Granados, 2005). Ruhm (2000) finds that a 1 percentage point increase in the unemployment rate is associated with a 1.3% increase in suicides. The strong association of suicides with business cycles, according to the author’s conjecture, is due to deterioration in mental health caused by unemployment. My research provides evidence on the connection between household finance and suicide risk, with a particular focus on the mental health channel.

By using suicide attempts, I examine a causal link between the changes in household balance sheets that are attributable to newly gained access to payday loans and the subjective evaluation of borrowers’ well-being measured by suicide outcomes. Of course, there are also non-economic factors that affect the risk of suicide among people; for instance, personal events, such as the passing of family or loved ones and development of mental illness. As long as these non-economic factors are uncorrelated with access to payday loans or with the proximity of these people’s locations to borders of payday-allowing states, then my estimates will not be confounded by the non-economic factors.

## 3 Empirical Analysis

### 3.1 Suicide Attempts and Deaths data

For the analysis, I use the State Inpatient Databases (SID) for New York and New Jersey for the years 1994–2000, constructed by the Agency for Healthcare Research and Quality’s Healthcare Cost and Utilization Project (HCUP).<sup>17</sup> The HCUP collects the universe of discharge-level inpatient data from states with partnership. Each discharge record includes

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<sup>17</sup>As explained earlier, this study focuses on states that never allowed payday lending but have a contiguous state that eventually allow payday lending. New York and New Jersey are the only two states before 2000 that share a border with a payday-allowing state and provide zip code information of inpatients to HCUP. I choose 2000 as the final year of this study’s analysis period because of the rapid expansion of the Internet in the early 2000s, which has increased demand for and supply of online payday loans. In general, few state laws prevent individuals from obtaining payday loans that are offered by out-of-state or online lenders. Moreover, the patent of Prozac, the most popular antidepressant, expired in August 2001. To minimize any possible spurious correlations that could bias the results of this research, I choose 1994 to 2000 as the sample period of this project.

demographic information of the discharged patient (e.g., age, sex, and race), diagnostic and procedure codes, admission type, length of stay, total medical charges, payer information, and, most importantly, zip code of the patient. Since one of the main contributions of this study to the literature is the utilization of more detailed geographic units, zip code information is crucial.

Each diagnosis category is constructed according to Clinical Classifications Software (CCS) for the ninth version of International Classification of Disease (ICD-9) (HCUP, 2010). For the list of CCS codes used to construct the outcome variables employed in this study, see the appendix. The dependent variables are the numbers of attempted suicide discharges by a variety of demographic groups, which are aggregated to the zip code level in each year. The data include more than 18 million discharge-level observations over 7 years, 1994–2000. The large sample size is critical because it enables the analysis of research to capture changes in hospitalizations of relatively rare events, attempted suicides. In addition, I supplement the zip code-level hospital data with demographic information from the 1990 decennial census.

One concern about the use of suicide attempt data is that the rates of (failed) suicide attempts are greater than those of (successful) attempts, or suicide deaths.<sup>18</sup> To address this concern, I also use suicide mortality data from the National Center for Health Statistics (NCHS). The NCHS data are a census of death records that list the cause(s) of death for deceased. Due to the confidentiality restrictions of the NCHS, the smallest geographic unit is a county.<sup>19</sup>

### 3.2 Payday Access Measure

Similarly to Melzer (2011), I consider zip-to-border distance to construct the measure for proximity of a zip code to a border of its neighboring states. In this study, therefore,

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<sup>18</sup>Suicide mortality was 6.9 per 100,000 people and attempts were 52.3 per 100,000 people in New York and New Jersey during 1994–2000, which equated to 7.6 attempts per completed suicide. The correlation between suicide attempt and suicide death is 88.2 for the period. Such a high correlation is highly anticipated.

<sup>19</sup>In addition, the confidentiality restrictions require that any mortality counts less than 10 be suppressed. I use the unsuppressed, county-level version of the data, provided by the NCHS. For the publicly available mortality data, see <https://wonder.cdc.gov/mortSQL.html>. Researchers who are interested in using the unsuppressed data can submit an application to the NCHS.

any zip codes in New Jersey and New York that are less than 25 miles from a border of either Pennsylvania or Delaware (the shaded area in Figure 3) are considered to have access to payday loans after payday lending emerged in Pennsylvania and Delaware.<sup>20</sup> All other zip codes in New Jersey and New York are considered to have no access throughout the sample period, 1994–2000.<sup>21</sup> The construction of the payday access variable is based on the assumption that people travel across borders to take out payday loans. As expected, Melzer (2011) and Bhutta (2014) find evidence that there are significantly more payday stores along the borders of payday-banning states.

### 3.3 Summary Statistics

Table 1 presents summary statistics on demographics, business patterns, and hospitalization rates of zip codes in payday-banning states (New York and New Jersey). The first two columns are summary statistics for zip codes within 25 miles of a border of Pennsylvania or Delaware (labeled *Payday Border*), and the last two columns are for those farther than 25 miles away from the two borders (labeled *non-Payday Border*).

There are slight differences between the two groups of zip codes. The payday border zip codes, on average, have smaller population, fewer number of business establishments, and slightly lower annual payroll. Moreover, the main dependent variable, suicide attempt, is a bit more common in zip codes bordered with a payday-allowing state. Other than these differences, the two zip code groups, stratified by payday access, have similar characteristics.

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<sup>20</sup>New Jersey is geographically separated from Delaware by Delaware River that runs through the entire border between New Jersey and Delaware. However, most residents in New Jersey can easily travel to Delaware via 28 bridges. To minimize incorrect assignments of payday access measure, instead of relying on birds-eye-view distances that ignore separations by the river, I manually recoded the travel distances between New Jersey’s affected zip codes and Delaware. In fact, some zip codes in Cape May County and Cumberland County at the bottom part of New Jersey required a recoding of the payday access measure. Thus, these zip codes are coded to have no access to payday stores in Delaware (based on the 25 mile cutoff) throughout the sample period.

<sup>21</sup>As in Melzer (2011), the results are not sensitive to the chosen distance, 25 miles. Regression results that use other distances will be provided upon request.

### 3.4 Estimation Framework

Due to the count nature of inpatient data, I use a fixed-effects Poisson model. Specifically, I estimate the conditional expectation of the outcome measures as

$$\mu_{zct} = \exp(\alpha_z + \beta \text{Payday}_{zt} + X'_{zt}\delta + \text{Border}_z \cdot t + \gamma_t + \eta_c \cdot t + \epsilon_{zt}), \quad (1)$$

where  $\mu_{zct}$  is the expected number of an outcome measure (e.g., the number of attempted suicide discharges and other discharge categories) by residents of zip code  $z$  and county  $c$  in New York or New Jersey in year  $t$ .  $\alpha_z$  represents a fixed effect for the zip code that controls for time-invariant differences in observable (e.g., the relative size and characteristics of population of the zip code) and unobservable characteristics. The inclusion of zip code fixed effects leads to the identification of the effects of payday access,  $\beta$ , solely from within the zip code variation across time.  $\text{Payday}_{zt}$  is a dummy variable that equals one if the zip code in New York or New Jersey is within 25 miles of any Pennsylvania or Delaware border after the rise of payday lending in Pennsylvania or Delaware, and zero otherwise.  $X_{zt}$  represents a set of time-varying controls, which includes the logarithms of zip code-level average annual payroll, total number of employees and establishments, and total number of all inpatient discharges.  $\text{Border}_z \cdot t$  is the linear time trend for bordered areas, where  $\text{Border}_z$  equals one if the zip code is located within 25 miles of any border and, zero otherwise. This linear time trend for the border captures any secular differences across areas depending on the proximity to a state border.  $\gamma_t$  captures the synthetic year fixed effect. Furthermore,  $\eta_c \cdot t$  represents a county-specific linear time trend, and  $\epsilon_{zt}$  represents an error term.

The identifying variation in the *Payday* variable arises from geographic variation in border proximity as well as cross-time variation in the availability of payday loans owing to the emergence of payday stores in neighboring states. Furthermore, by including only zip codes in payday-banned states in the analyses, this identification strategy exploits plausibly exogenous variation in access to payday loans.



The model is estimated by the Poisson quasi-maximum likelihood estimator (QMLE). Under the mild condition that the conditional mean is correctly specified, this estimator provides consistent estimates, is fully robust to distributional misspecification, and does not require that the distribution be Poisson (Wooldridge, 1999).<sup>22</sup> In all regressions, unless described otherwise, standard errors are corrected to account for an arbitrary correlation at the county level.

Although this study mainly uses Poisson models, owing to the count nature of the inpatient data, as a robustness check, I also compare estimates from the OLS specifications of the following form:

$$\log(Y_{zct}) = \alpha_z + \beta \text{Payday}_{zt} + X'_{zt} \delta + \text{Border}_z \cdot t + \gamma_t + \eta_c \cdot t + \epsilon_{zt}, \quad (2)$$

where  $Y_{zct}$  is an outcome measure and all other variables are defined as before.

## 4 Empirical Results

The results presented in this section suggest that access to payday loans increases suicide attempts. The analysis begins by investigating the effects of access to short-term high-interest loans on suicide attempts by all age groups. To examine the possible mechanisms for these effects further, I study the relationship of payday loans and suicide attempts in different demographic groups, such as age, race, sex, and insurance type. Finally, I provide evidence for mental health explanation as the channel through which credit access affects suicide risk.

### 4.1 Dynamic Effects

First, I show there was no differential trend across locations prior to gaining access to payday loans and that the effects are pronounced only for those aged under 65 years—the treated

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<sup>22</sup>In addition to a consistent estimation of the count data models, the Poisson quasi-maximum likelihood method can be used to obtain consistent estimates for positive and continuous variables as long as the conditional mean is correctly specified (Wooldridge, 1999).

group—after obtaining access to payday loans. I estimate the following specification:

$$\mu_{zct} = \exp \left( \alpha_z + \sum_{s=-3(0)}^3 \beta_s \text{Payday}_{z,t+s} + X'_{zt} \delta + \text{Border}_z \cdot t + \gamma_t + \eta_c \cdot t + \epsilon_{zt} \right), \quad (3)$$

where  $\mu_{zct}$  is the expected number of discharges of suicide attempts, and  $\text{Payday}_{z,t+s}$  are six separate dummy variables set to one in the  $s$ th year before or after having access to payday loans, and zero otherwise. I do not include an indicator for the year in which payday lending was first allowed (or started to emerge), so  $\beta_t$  measure the dynamics of suicide attempts relative to the base years, denoted as year 0.

The top panel of Figure 4 shows the dynamic impact of access to payday loans on suicide attempts among those aged under 65 years. Although there is a slightly increasing trend of suicide attempts in the pre-payday periods, the point estimates are not statistically different from zero. A sharp increase can be observed after gaining access to payday loans, and there are relatively persistent effects over 3 years. The bottom panel displays the dynamic effects among the control group—the elderly. As can be seen, there is no pattern that is statistically significantly different from zero after gaining access to payday loans. As with previous results, the striking difference between the two age groups provides evidence for adverse effects of having access to payday loans on suicide attempts.

## 4.2 Baseline Empirical Results

Table 2 reports the baseline estimates of the effects of access to payday loans on attempted suicides. In all specifications, dependent variables are regressed on the binary payday access variable, county-specific time trends, border time trends, zip code fixed effects, and year fixed effects; and for all even-numbered columns, a set of zip code level economic controls are included in the estimations. Standard errors are adjusted to account for an arbitrary variance-covariance matrix at county level. The first two columns report estimates from a Poisson regression and all others, estimates from an OLS. All Poisson regressions used in

this paper produce estimates, obtained by the Poisson QMLE with robust standard errors clustered by county, which are completely robust to arbitrary distributional misspecification and serial correlation.

Columns 1 and 2, estimated by Poisson regressions with suicide attempts as the dependent variable, report considerably similar estimates. Exponentiating the coefficients of 0.104 and 0.100, these results indicate that having access to payday loans increases attempted suicides by 11.0% and 10.5%, respectively.<sup>23</sup> Although adding controls slightly decreases the point estimate, the two estimates are of similar magnitude and the effects of payday loan access on attempted suicides are both statistically and economically significant.

As a quick robustness check, I compare these Poisson estimates with estimates from OLS specifications in columns 3 through 6. Columns 3 and 4 use the natural logarithm of the rate of attempted suicides as the dependent variable, where the rate of attempted suicides is calculated as the total number of attempted suicides per 100,000 people. Estimates from these specifications are very significant at the 1% significance level with slightly increased point estimates compared to Poisson specifications. Columns 5 and 6 replicate the previous two models with a slight adjustment for zero count of suicide attempt; that is, attempt counts are increased by 1, in order to keep zip codes with no suicide attempt. The estimates also are significant and quite similar in magnitude with the Poisson estimates.

Overall, the results imply that access to payday loans increases annual suicide attempts by 9.2–12.2%. Stable and consistent results across both Poisson and OLS specifications confirm that adjusting for observations with zero count and changing functional forms of the dependent variable have little effect on the interpretation of the estimated coefficient on *Payday*. If calculated at the mean rate (52.3 suicide attempts per 100,000 people), the baseline estimate (column 2) implies that gaining access to high-interest short-term loans causes an additional 5.5 suicide attempts per 100,000 people, which equates to 1,400

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<sup>23</sup>Since each of these estimates is the log of the ratio of expected attempted suicide counts, a simple conversion is required to interpret them precisely; for example, the estimate from the second column, 0.100, is converted to  $0.105 (= \exp(0.100) - 1)$ .

additional suicide attempts for New York and New Jersey combined each year.<sup>24</sup>

A somewhat important item to check is how suicide *attempt* compares with suicide *mortality*. Table 3 reports the baseline estimates of the effects of access to payday loans on suicides *mortality*. Since mortality data are not available at zip level, the table reports county-level analyses. In general, the estimates are consistent with the zip-level counterparts, except that the size of the effects of payday access on suicide mortality is a bit larger than that on suicide attempts.<sup>25</sup>

### 4.3 How Does Access to Payday Loans Affect Borrowers Differently?

Prior studies have identified the characteristics of primary customers of paydays, for example, households with low to moderate income (Melzer (2011); Pew Charitable Trusts (2012)); those who are divorced or separated, disabled, or without a college degree (Pew Charitable Trusts, 2012); and military personnel (Carrell and Zinman, 2014). This subsection explores heterogeneous effects across diverse demographic groups and tests the hypotheses that people with higher demand for payday loans are more responsive to newly obtained access to payday loans; consequently, there are more suicide attempts in high demand zip codes. Base

#### 4.3.1 Age

Since borrowers need to provide employment records and previous payrolls to payday lenders in order to be eligible for payday loans, I expect to see a pronounced impact of payday access among those aged under 65 years but not among the elderly who are retired. Although unemployed retirees who are mainly on Social Security might still qualify for payday loans,

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<sup>24</sup>The calculation of 1400 additional attempts is based on the population from the decennial census 1990, in which New York and New Jersey have populations of 18 million and 7.7 million, respectively.

<sup>25</sup>The effects of payday access on suicide risks are 18.6% for suicide mortality and 10.5% for suicide attempts. This result might come from different characteristics between mortality and hospitalization due to suicide. Lethal methods of suicide, such as use of firearms and jumping from heights, do not require hospitalization if the attempters die on spot; thus, cases of this sort are not included in my suicide attempt data. In addition, suicide attempters who are initially hospitalized but die during or after treatments are included in the mortality data of this study.

not all payday loan lenders accept applications from them, possibly because there would be no wages to be garnished from them if a payment were missed. With these increased costs and possible stricter screening for retired applicants, there would be much less incentive for the elderly to travel to a payday lender. Indeed, a survey by Pew Charitable Trusts (2012) finds that the primary consumer group of payday loans is prime-age workers and only 2% of the elderly have used payday loans. As such, my approach naturally constructs a control group consisting of people aged 65 years and over.

Each column of Table 4 shows the effect of payday access on attempted suicide by different age groups. As expected, only estimates for the groups that are presumably affected by this credit type are significant and economically meaningful (columns 3–8). Even more convincing, perhaps, is that the effect increases with demand for this loan. In other words, the age group with the largest share of people who have used a payday loan (age 25–44 years) reports the biggest increase in suicide attempts, 16.3%, after gaining access to payday loans, followed by 12.6% and 12.0% for the age groups 25–54 years and 25–64 years, respectively.<sup>26</sup>

For the elderly (column 9) and younger groups (columns 1 and 2), however, the estimates are small and not statistically significant at any standard level.<sup>27</sup> Despite the fact that the estimate for the youngest group (column 1) is insignificant, this group is not a good control group. Because people aged under 19 years (or 20–24 years) are likely to live with parents who are in the range of age groups with high demand for payday loans, there is a possibility for a second order effect of payday loans. In other words, a suicide attempt or death by parents who suffered from debts from payday loans potentially could be followed by one by their children.<sup>28</sup> Even without an attempt by parents, if detrimental effects from payday

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<sup>26</sup>See Pew Charitable Trusts (2012) for detailed reports on demographics of payday loan borrowers.

<sup>27</sup>During the period of the sample used, 1994–2000, the average mean rate of suicide attempts for the elderly was 24.6 per 100,000. One reason for such a low rate, compared with their highest suicide *mortality* rate, is that the elderly who commit suicide tend to use more lethal means due to ease of access and lower physical resilience than younger groups (Conwell et al., 2011). Since all of the specifications in this study include zip code fixed effects, the disparity between the rates of suicide *attempt* and *mortality* does not bias the estimates or harm the eligibility of the elderly as a control group, unless there is a systemic change of methods used for suicide attempts in response to payday loan access.

<sup>28</sup>Family history of suicide is among the top high-risk factors (Carrigan and Lynch, 2003).

loans worsen intra-family relationships or economic hardships, the risk of suicide for children could increase. Therefore, I prefer including the aged under 24 years in the treatment group for a more conservative analysis. Indeed, the regression results in this study hold regardless of inclusion of those aged under 24 years.

### **4.3.2 Sex**

Table 5 explores heterogeneity of the effects of payday loans on suicide attempts across sex. The dependent variable of each column is the number of suicide attempts by a denoted group; for instance, in column 1, the dependent variable is the number of suicides by males aged under 65 years and, in column 2, by females aged under 65 years.

The results imply a 15.8% increase in attempted suicides for males aged under 65 years and an 8.7% increase for females aged under 65 years. Even though the effect of payday access for the younger male group is twice as large as that for the younger female group, because of the higher suicide attempt rate among females, the total expected additional discharges due to payday access are similar for both sexes: 5.8 additional patients for males and 4.5 for females. Columns 3 and 4 present estimates for the elderly. Although the point estimate for elderly males is positive, it is not statistically different from zero. Moreover, the estimate is even negative for elderly females.

### **4.3.3 Race**

Table 6 reports estimates from specifications by race and age. Column 1 shows that having access to payday loans would increase attempted suicides among the white population aged under 65 years by 12.6% and an additional 5.4 discharges, while the white control group (aged 65 years and over) in column 2 has no significant effects. Columns 3 and 4 show estimates for the black population, and columns 5 and 6 for all minorities, including the black population. Although the point estimates for minorities under 65 years (columns 3 and 5) are similar in sizes with that for the white counterpart (column 1), they all have, as expected, positive signs and the estimates are negative and statistically insignificant for the

elderly of both groups.<sup>29</sup>

#### 4.3.4 Insurance

Since the primary target of payday lenders is the employed, who can provide a history of previous paychecks, a logical test we can run is to observe if the group of patients with private health insurance is disproportionately affected by access to payday loans.<sup>30</sup> Taking advantage of the detailed discharge-level inpatient data, I can use payer information. For each discharge record, I categorize the patient as having private health insurance if either the patient’s primary or secondary payer is private insurance, and similarly for Medicaid and Medicare.<sup>31</sup> Discharges that are expected to be paid solely by the patients are categorized as self-pay; that is, either the primary or secondary payer is categorized as self-pay with none of them indicating other types of insurance.<sup>32</sup>

Table 7 reports the results. Only the estimate for patients with private insurance is statistically significant, showing a 13.2% increase in attempted suicides. Patients with Medicaid even have a negative estimate, although it is statistically not different from zero. In addition, the elderly is statistically insignificant. The negative and small effects of payday access for non-private insurance holders makes sense, because very low-income people and the elderly have no or very limited eligibility for payday loans.

#### 4.3.5 Income

Table 8 tests whether zip codes with a high share of low- to moderate-income households that have high demand for payday loans are disproportionately affected by accessing payday loans. Elliehausen and Lawrence (2001) finds that individuals in the income range \$25,000 to \$50,000 are the main consumers of payday loans, accounting for more than half of the total

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<sup>29</sup>Melzer (2011) also finds that payday access has significant effects on economic hardship for only the white population, with insignificant effects for other minority groups.

<sup>30</sup>In the U.S. over %80 of private health insurance is sponsored by employers.

<sup>31</sup>Private health insurance category includes Blue Cross, commercial carriers, and private HMOs and PPOs.

<sup>32</sup>A patient might be grouped into multiple insurance types because a non-trivial share of patients holds both private insurance and Medicare. However, the “self-pay” category is constructed as exclusive from other insurance types.

payday borrowers. Morse (2011) finds that those with income range of \$15,000 to \$45,000 have the largest probability of being financially constrained. With these prior findings, I use three income ranges for the test: household income in the range of \$20,000 to \$40,000, household income in the range of \$12,000 to \$50,000, and household income in the range of \$20,000 to \$50,000.

I split zip codes into those above and below the median share of household income in each range of income. For the income range \$20,000–\$40,000 in columns 1 and 2, a zip code is categorized as “Top 50 pct” if it has above the median share of households with income in the range \$20,000–\$40,000. Similarly, a “Bottom 50 pct” zip code has below the median share of households with income in the range \$20,000–\$40,000. The same construction method is used for columns 3–4 and columns 5–6 with household income ranges of \$12,000–50,000 and \$20,000–50,000, respectively. The estimates in all odd-numbered columns (top half percentile zips) report significant and large effects of payday access on attempted suicides, ranging from 15.0% to 20.8%, or 8.4 to 11.2 additional attempted suicides per 100,000. On the other hand, none of the even-numbered columns (bottom half percentile zips) report statistically significant estimates. Moreover, the point estimates are considerably smaller than the high demand zip codes.

#### **4.3.6 Other Demand Factors**

Table 9 reports estimates from a further test on the effects of payday access using other demand factors: single parents (columns 1–2), divorced people (columns 3–4) and high-school graduates (columns 5–6). Construction of the “Top 50 pct” and “Bottom 50 pct” zip codes are the same as for Table 8; that is, based on the median share of each factor, I divide the sample into two subsamples and perform analyses within each subsample. As described earlier, all those characteristics have positive correlations with demand for payday loans.

The estimates from high-demand zip codes (odd-numbered columns) show that the effects are large; there is a 9.1–12.2% increase in suicide attempts after gaining access to payday loans in zip codes with more single parents, divorced people, and high-school graduates. In



addition, the estimates mean an additional 5.4 to 7.3 discharges per 100,000 people. By contrast, there are no significant estimates for low-demand zip codes and the estimates are considerably small.

#### **4.4 Mental Distress: The Channel through which Financial Distress Affects Suicide Risk**

The existing literature suggests that stress is associated with deterioration in both physical and mental health (McEwen, 1998; Cooper, 2004; Schneiderman et al., 2005). If financial distress increases the overall stress level, we would expect that changes in household balance sheets would affect health and healthcare usage as well.<sup>33</sup> Several studies find support for this channel (e.g., Gross and Tobacman (2014); Evans and Moore (2011); Dobkin and Puller (2007); Parker et al. (2013); Currie and Tekin (2015)). As such, if accumulated household debts attributable to expensive credit worsen emotional and mental instability, then a member of the household who is at risk might develop suicidal ideation and could be located below his or her own suicide threshold. As a result, those who suffer from mental well-being deterioration might consult a doctor and obtain psychotropic drugs (antidepressants), such as Prozac.<sup>34</sup> With easier access to these psychotropic medications, suicidal borrowers are more likely to use prescribed psychotropic drugs as a method to commit suicide. This section provides a test for this mental health channel.

First, I briefly recalculate the main analyses (in Table 4 and Table 7) with a focus on suicide attempts by poisoning, instead of analyses on suicide attempts by all methods. Table 10 reports estimates of the effects of access to payday loans on attempted suicides by poisoning. As Panel A shows, gaining access to payday loans increases the number of suicide

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<sup>33</sup>Indeed, Deaton (2011) finds that the collapse of Lehman Brothers had negative effects on self-reported worry, stress, and life evaluations of Americans.

<sup>34</sup>Fluoxetine, an antidepressant known mostly as Prozac, was first introduced in 1986 by Eli Lilly and Company. The drug was a commercial success. Prozac was the U.S.'s most prescribed antidepressant within 5 years of its introduction (Fitzpatrick, 2010). Because the patent of Prozac expired in August 2001, to minimize this impact, if any, which could bias the results of this research, I choose 1994–2000 as the sample period of this project.

attempts by poisoning only among the treatment group (those aged under 65 years) by 14.1% and does not affect the control group (those aged 65 years and over). Even more reassuring is that the point estimate for the control group is almost zero and even negative. The dynamic effects of payday access on suicide attempts by poisoning are displayed in Figure 5. The top panel shows the dynamic effects for the treated group, which is consistent with our previous findings. There are no pre-existing trends, but after gaining access to payday loans, there is a sudden increase in attempted suicides by poisoning. Unlike the younger neighbors, in the bottom panel, the older population's poisoning attempt indicates no discernible responses to payday loans.

Panel B of Table 10 describes the effects of payday access on suicide attempts by poisoning by different insurance holders. Not surprisingly, the only group significantly affected by payday loans is patients with private insurance, in other words, the employed who are the primary consumers of payday loans (column 1). The point estimate, which is statistically significant at the 1% level, indicates a 19.8% increase in hospitalization of employed people due to suicide attempts by poisoning. Patients with other insurance types do not have significant estimates. Because of the small share of self-paying patients who have no private insurance, Medicaid or Medicare, the majority of zip codes are dropped in the regressions owing to no variation in the dependent variable. In other words, there are no self-paying patients who have attempted to commit suicide by poisoning for many zip codes throughout the sample period. Therefore, with this small number of observations, the point estimate is highly imprecise. The dynamics of suicide attempts by poisoning by patients with a different insurance type (in Figure 6) provide additional support for the treatment-control interpretation; for all insurance-type patients, there are no pre-existing trends but a significant increase only for employed patients (with private insurance).

Finally, Table 11 provides the result for a test of the mental health channel. Patients with mental health deterioration due to financial distress will seek medical care through out-patient settings or physician's office visits and will use prescription drugs to treat depression,

anxiety, and emotional distress. It is plausible that easier access to a means of suicide, or psychotropic drugs, raises the risk of suicide by overdosing on those drugs. Column 2 is the core test for the mental health channel. It indicates that there is a 21.9% increase in suicide-related hospitalization of those aged under 65 years by overdosing on psychotropic agents.<sup>35</sup> Unlike those aged under 65 years, there is no statistically significant effect for the elderly (in column 3) who have little access to payday loans. This confirms the hypothesis that mental health could be one of major driving forces behind suicide risk and financial distress caused by expensive credit. Column 5 reports that suicide attempts by other medicinal drugs increased by 14.3%. This result is expected, because suicidal people use both prescription and non-prescription drugs to commit suicide. The focal point of the test is to observe whether the treatment group uses more psychotropic drugs for poisoning (in column 2) and whether the control group has no statistically significant change (in column 3). For suicide attempts by poisoning with non-medicinal substances (e.g., petroleum products, acids, and lead compounds), columns 7–9 show no effects of access to payday loans.

#### 4.5 Robustness Checks and a Placebo Test

Table 12 presents estimates from a diverse set of specifications as robustness tests. In Panel A, columns 1 and 2 are the baseline estimates (as in columns 8 and 9 of Table 4). Columns 3 and 4 show the coefficients of *payday* in specifications using a subset of the main sample, which includes zip codes with population greater than 1,000 in the 1990 census. Similarly, the specifications of columns 5 and 6 use a subsample that includes zip codes with population greater than 3,000. The estimates are all significant and similar in size with only the younger group's estimates being statistically and economically meaningful.

In Panel B, I perform a further robustness test with another set of subsamples. The subsample used in columns 1 and 2 includes zip codes that have a non-zero count of suicide attempts in each year for the entire sample period. Columns 3 and 4 use zip codes with

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<sup>35</sup>Psychotropic drugs temporarily alter mood, perception, consciousness, and behavior and are prescribed as anti-depressants and anti-psychotics to treat mental illnesses.

at least 5 years of non-zero suicide attempts (out of 7 years), and finally, columns 5 and 6 use zip codes with at least 2 years of non-zero suicide attempts. Having all non-zero suicide attempts means the zip code is likely to have a large population size. Restricting the sample does not alter the interpretation of the previous findings. All estimates for the elderly group are statistically insignificant and small, whereas the treated younger group has significant, large, and stable estimates.

Panel C presents estimates from a falsification test using the following specification with placebo payday access assignment:

$$\mu_{zct} = \exp(\alpha_z + \beta FakePayday_{zt} + X'_{zt}\delta + Border_z \cdot t + \gamma_t + \eta_c \cdot t + \epsilon_{zt}), \quad (4)$$

where  $FakePayday_{zt}$  equals one if a zip code in New York and New Jersey is within 25 miles of a border of payday-banning states (e.g., Vermont, Massachusetts, and Connecticut) after the emergence of payday lending in Pennsylvania and Delaware, and zero otherwise (See Figure 7). These zip codes with  $FakePayday_{zt} = 1$  do *not* have real access to payday loans because none of their neighboring states allowed payday lending. As Panel C shows, the estimates are small and insignificant. The absence of significant effects of placebo payday access measure suggests that the relationship between access to payday loans and increased suicide attempts is not coincidental. The overall findings of this study, as well as the robustness tests, are difficult to be reconciled with alternative explanations for increases in suicide attempts by people aged under 65 years and the employed with private insurance.

## 5 Conclusion

This study analyzes the effects of access to payday loans on the long-term well-being of borrowers, measured by suicide risk. The robust evidence is consistent with payday loans increasing the risk of suicide for low- and moderate-income borrowers and employed workers. I find that having access to short-term, expensive payday loans increases hospitalization due to suicide attempts by 10%. This estimate implies at least an additional 5.5 hospitalized

suicide attempts per 100,000 people and, if converted to the national level, an additional 15,000 attempts in 1998. As such, the results suggest that the social costs to increased suicide attempts and suicide deaths caused by gaining access to payday loans are quite large. At a minimum, suicide attempts incurred annual medical costs of \$142 million per year during the late 1990s and early 2000s.

In addition, the findings provide a possible mechanism through which financial distress caused by expensive credit affects the suicide risk of a population. Due to access to payday loans, borrowers who accumulate excessive amount of debts undergo financial distress. This in turn causes their mental and emotional stability to deteriorate through stress. Mentally distressed people who have easier access to prescription medications, such as antidepressants, are more likely to attempt suicide by poisoning. Although this explanation cannot be tested directly on the extensive margin (new antidepressant prescriptions due to payday access), the finding cannot be explained by alternative explanations.

While the results presented in this study provide strong evidence for detrimental welfare consequences of having access to payday loans, this causal link should not be misinterpreted as a call for a complete ban on payday lending. The fact that 12 million Americans use payday loans per year indicates that there are no better alternatives for low- and moderate-income credit-constrained households. Thus, without fulfilling otherwise unmet demand, depriving already credit-constrained households of payday loans that serve as their last resort of lending will be a welfare-reducing policy. Hence, careful and thorough government intervention is required to regulate payday loans.

## Appendix: CCS Single-Diagnosis Codes

- Suicide Attempt: CCS 662
- Poisoning by Psychotropic Agents: CCS 241
- Poisoning by Other Drugs: CCS 242
- Poisoning by Non-medicinal Substances: CCS 243
- Poisoning: CCS 241, 242, 243

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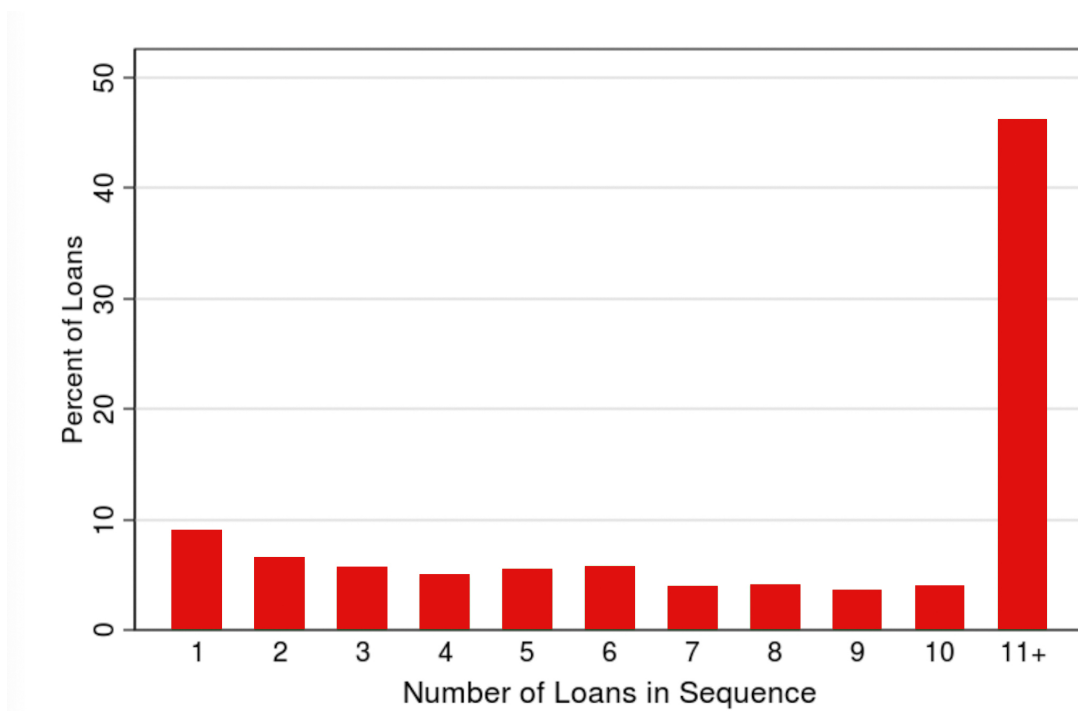
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Figure 1: Distribution of Number of Loans by Sequence



*Notes:* This figure is from a report by the Consumer Financial Protection Bureau (CFPB, 2014). It shows the distribution of the number of loans by sequence. For instance, a first loan in sequence indicates a new loan, and a fifth loan in sequence indicates that four renewals (rollovers) have been made on the initial loan. Approximately half the total loans are in the 10th or more sequence.

Figure 2: A Map of Northeastern States



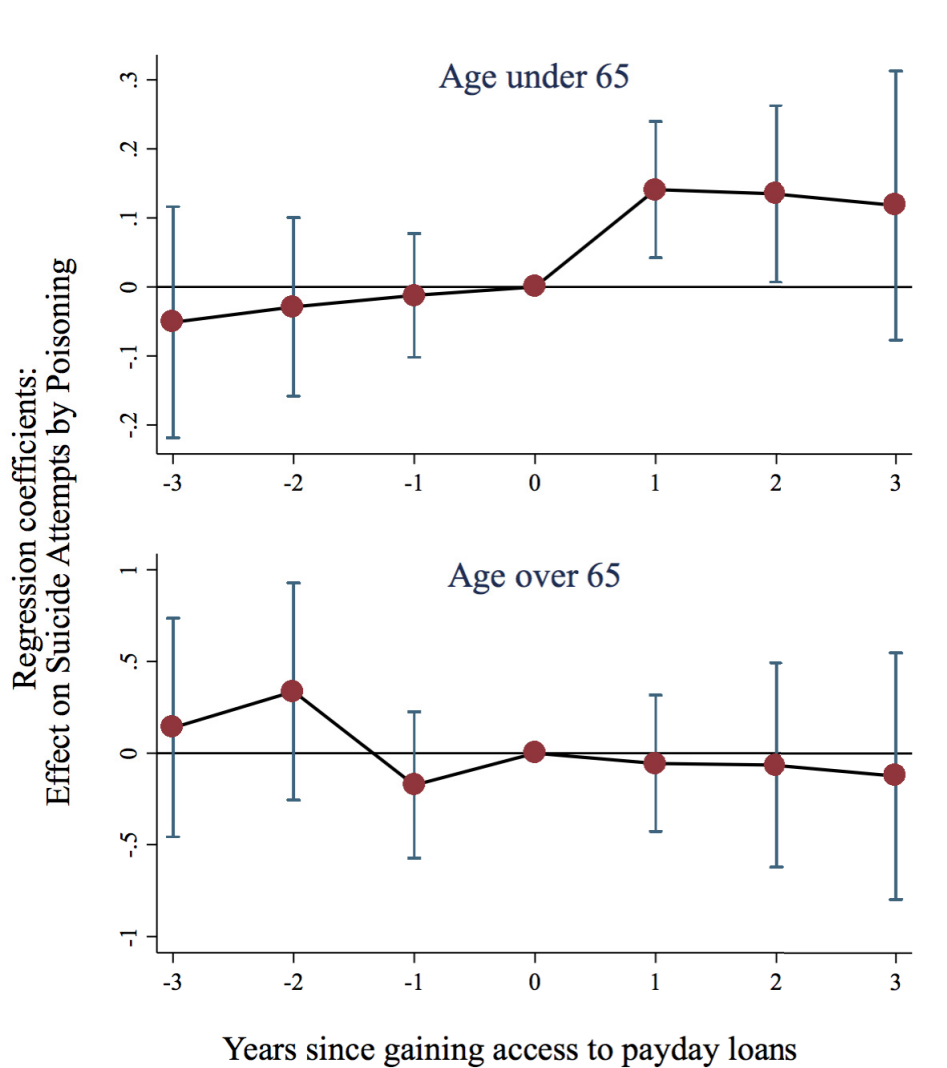
*Notes:* Pennsylvania (PA) and Delaware (DE) are the only two payday-allowing states that share borders with New York (NY) and New Jersey (NJ) during the years 1994–2000. No other neighboring states to New York and New Jersey allowed payday lending during the sample period 1994–2000.

Figure 3: Zip Codes with Close Proximity to Payday-Allowing States



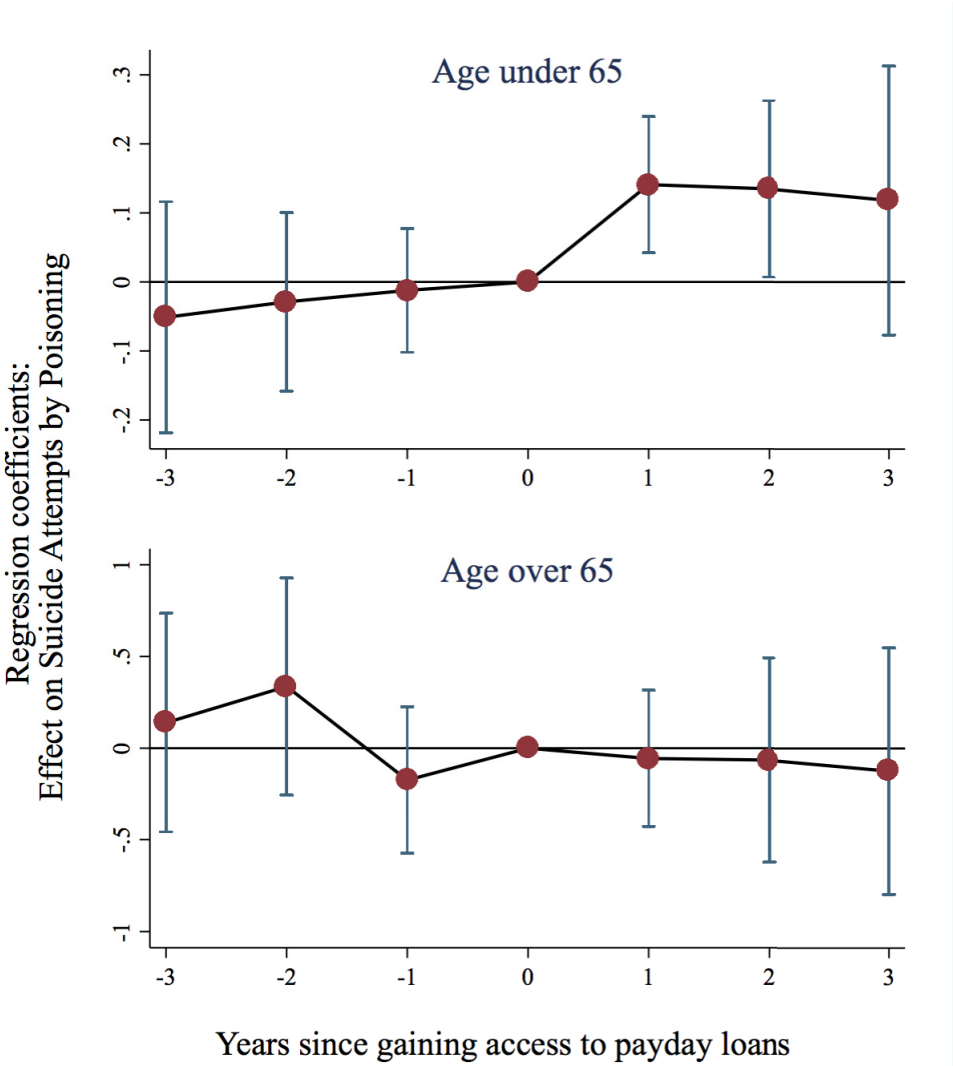
*Notes:* The shaded area represents zip code areas that are within 25 miles of the borders of Pennsylvania (PA) and Delaware (DE). The identification strategy assumes that populations in those zip codes have access to payday loans only after the emergence of payday lending in PA and DE, and that all other populations in zip codes in New York and New Jersey do not have access to payday loans during 1994–2000.

Figure 4: Dynamic Effects of Access to Payday Loans on Suicide Attempts



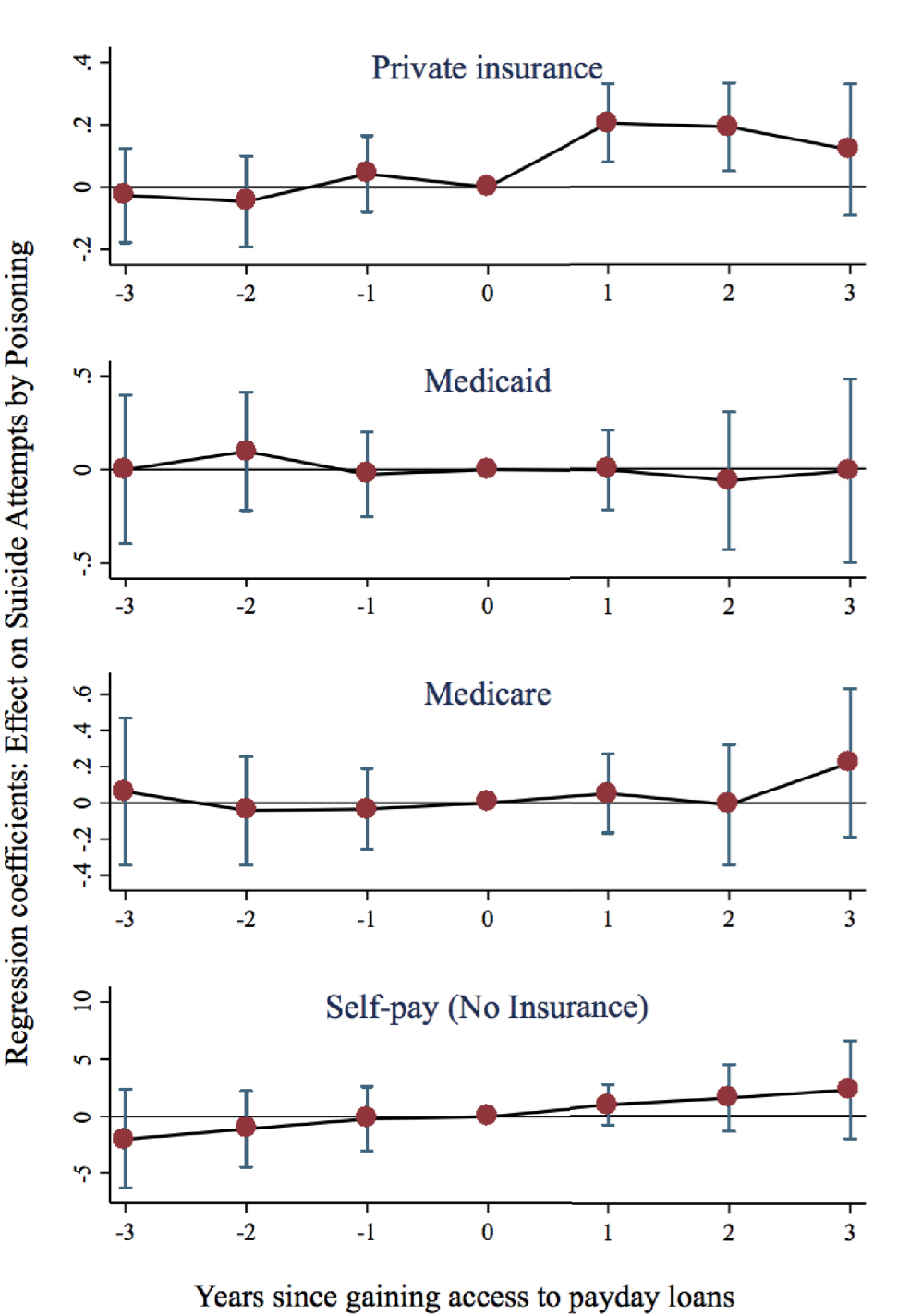
*Notes:* Each figure plots coefficients from regressions of suicide attempts on a series of indicator variables, including 3 years before and 3 years after gaining access to payday loans, as well as zip code-level controls, year fixed effects, zip code fixed effects, border trends, and county-specific trends. The vertical I-beams represent the 95% confidence interval. All estimates are by the Poisson quasi-maximum likelihood estimator (QMLE) with robust standard errors clustered by county.

Figure 5: Dynamic Effects of Access to Payday Loans on Suicide Attempts by Poisoning



*Note:* Each figure plots coefficients from regressions of suicide attempts on a series of indicator variables, including 3 years before and 3 years after gaining access to payday loans, as well as zip code-level controls, year fixed effects, zip code fixed effects, border trends, and county-specific trends. The vertical I-beams represent the 95% confidence interval. All estimates are by the Poisson quasi-maximum likelihood estimator (QMLE) with robust standard errors clustered by county.

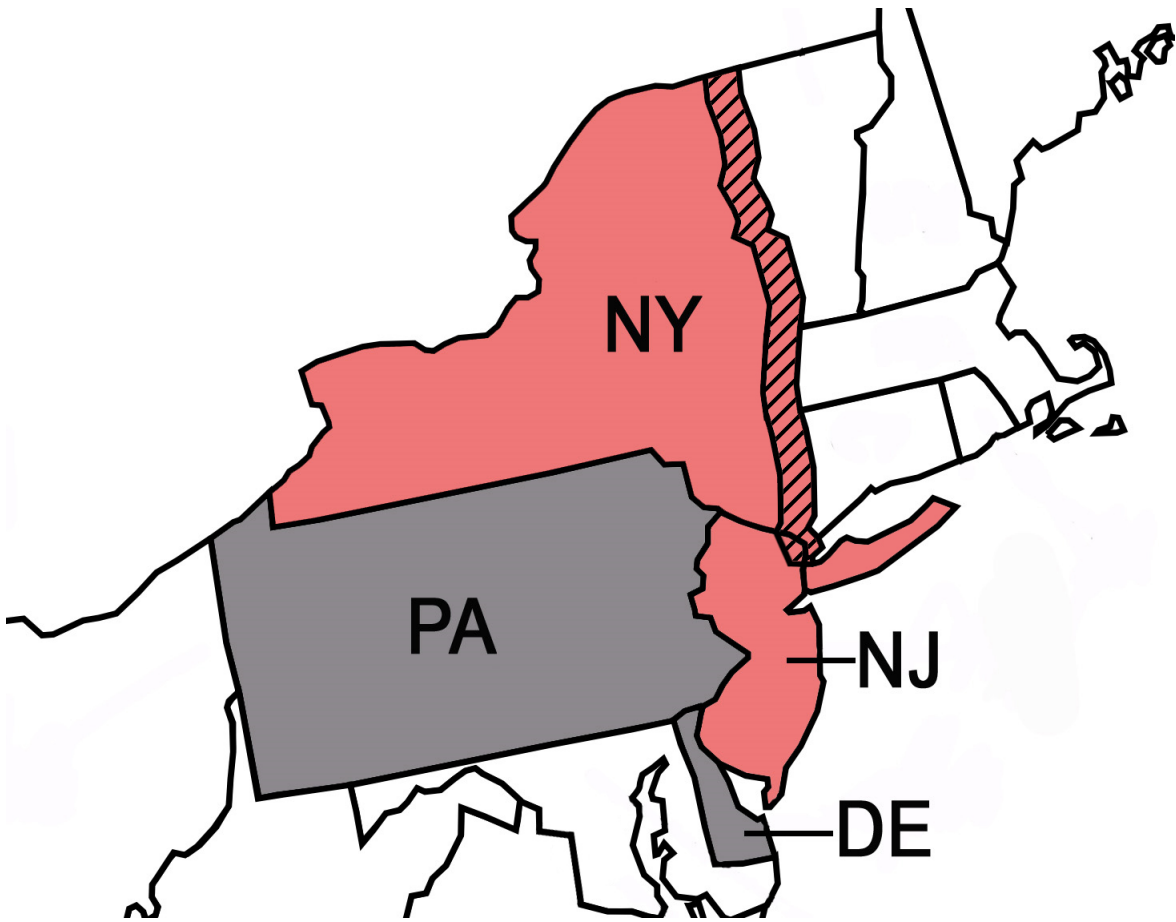
Figure 6: Dynamic Effects of Access to Payday Loans on Suicide Attempts by Poisoning



Notes: Each figure plots coefficients from regressions of suicide attempts on a series of indicator variables, including 3 years before and 3 years after gaining access to payday loans, as well as zip code-level controls, year fixed effects, zip code fixed effects, border trends, and county-specific trends. The vertical I-beams represent the 95% confidence interval. All estimates are by the Poisson quasi-maximum likelihood estimator (QMLE) with robust standard errors clustered by county.



Figure 7: Zip Codes with Close Proximity to Payday-Banning States: a Placebo Test



*Notes:* The shaded area represents zip code areas that are within 25 miles of the borders of payday-banning states (e.g., Vermont, Massachusetts, and Connecticut). Even after the emergence of payday lending in Pennsylvania and Delaware, these zip codes do *not* have real access to payday loans because none of their neighboring states allowed payday lending.

Table 1: Summary Statistics

	Payday Border = 1			Payday border = 0		
	Mean	Obs	Std. Dev.	Mean	Obs	Std. Dev.
<i>Zip code demographics</i>						
Population	19,450	2,618	12,777	34,553	12,215	23,457
Income per capita	16,615	2,618	5,441	17,240	12,215	8,740
Median household income	39,462	2,618	12,772	37,305	12,215	14,962
White (%)	0.87	2,618	0.18	0.74	12,215	0.28
Black (%)	0.09	2,618	0.15	0.16	12,215	0.23
All minorities (%)	0.13	2,618	0.18	0.26	12,215	0.28
Male (%)	0.49	2,618	0.24	0.48	12,215	0.24
Female (%)	0.51	2,618	0.24	0.52	12,215	0.24
High school dropouts (%)	0.21	2,618	0.09	0.26	12,215	0.12
High school graduates (%)	0.33	2,618	0.07	0.30	12,215	0.07
College graduates (%)	0.46	2,618	0.14	0.45	12,215	0.15
Married (%)	0.59	2,618	0.08	0.54	12,215	0.09
Divorced (%)	0.07	2,618	0.02	0.06	12,215	0.02
<i>Zip code business patterns</i>						
Number of establishments	488	2,603	397	789	12,137	796
Number of employees	7,460	2,513	7,568	10,853	11,755	14,129
Annual payroll (per employee)	27,984	2,513	8,779	28,342	11,755	9,032
<i>Discharge rates (per 100,000 people)</i>						
Suicide attempts	62.2	2,618	33.3	50.9	12,215	34.9
Poisoning (non-medicinal substances)	2.2	2,618	2.5	2.1	12,215	8.6
Poisoning (psychotropic drugs)	23.2	2,618	13.5	16.0	12,215	12.5
Poisoning (other drugs)	32.1	2,618	17.1	26.0	12,215	19.1

*Notes:* Annual payroll is calculated as the zip code-level aggregate payroll divided by the number of employees. A discharge rate is calculated as an annual average of each discharge type by zip code for the sample period, 1994–2000. Means are weighted by zip code population in 1990. Sources: zip code demographics are from the Decennial Census 1990, the zip code-level business pattern data are from the County Business Patterns, and discharge rates are based on HCUP–New York and New Jersey State Inpatient Databases (SID) 1994–2000.

Table 2: Relationship Between Suicide Attempts and Payday Access

This table represents zip code-level regressions of suicide attempts on the payday access indicator. In columns 1 and 2, the Poisson models are estimated by the Poisson quasi-maximum likelihood estimator (QMLE). In columns 3-6, the models are estimated by ordinary least squares (OLS). “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. “Average suicide attempts (per 100,000)” is the average rate of suicide attempts. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits coefficients of border trends, county-specific trends, zip code fixed effects and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

	(1)		(2)		(3)		(4)		(5)		(6)	
	Swicide attempts	Swicide attempts	Swicide attempts	Swicide attempts	ln(rate of suicide attempts)	ln(rate of suicide attempts)	ln(rate of suicide attempts)	ln(rate of suicide attempts)	ln(rate of 1+ suicide attempts)	ln(rate of 1+ suicide attempts)	ln(rate of 1+ suicide attempts)	ln(rate of 1+ suicide attempts)
	Poisson											
	Poisson						OLS					
payday	0.104** (0.045)	0.100** (0.044)	0.122*** (0.045)	0.108*** (0.041)	0.101** (0.041)	0.092** (0.038)						
log(wage)		0.003 (0.056)		-0.078 (0.064)		-0.042 (0.051)						
log(employment)		0.037 (0.055)		0.025 (0.050)		0.009 (0.036)						
log(establishment)		0.110 (0.150)		0.015 (0.146)		0.043 (0.112)						
total any discharges (per 1000)		0.000 (0.000)		0.006*** (0.001)		0.005*** (0.001)						
Effects of payday access	11.0%	10.5%	12.2%	10.8%	10.1%	9.2%						
Average suicide attempts (per 100k)	52.3	52.3	52.3	52.3	52.3	52.3						
Additional suicide attempts (per 100k)	5.7	5.5	6.4	5.6	5.3	4.8						
Observations	14,063	13,662	10,916	10,796	14,833	14,268						
Number of zip	2,009	1,976	2,009	1,978	2,119	2,086						
Controls		X		X		X						
Border Trends	X	X	X	X	X	X						
County Trends	X	X	X	X	X	X						
ZIP FEs & Year FEs	X	X	X	X	X	X						
Cluster	county	county	county	county	county	county						

Table 3: Relationship Between Suicide Mortality and Payday Access

This table represents county-level regressions of suicide deaths on the payday access indicator. In columns 1 and 2, the Poisson models are estimated by the Poisson quasi-maximum likelihood estimator (QMLE). In columns 3 and 4, the models are estimated by ordinary least squares (OLS). “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide deaths, and for columns 3 and 4, it is just the estimated coefficient of the “payday” variable. “Average suicide deaths (per 100,000)” is the average rate of suicide deaths. “Additional suicide deaths (per 100,000)” is the percentage change in the expected number of suicide deaths, attributable to access to payday loans. To save space, this table omits coefficients of border trends, county fixed effects, and year fixed effects. The data used to produce this table are the restricted version of compressed mortality files provided by the National Center for Health Statistics (NCHS). All models report standard errors clustered by county, which are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

	Poisson		OLS	
	(1) Suicide deaths	(2) Suicide deaths	(3) ln(rate of suicide deaths)	(4) ln(rate of suicide deaths)
Payday	0.168*** (0.051)	0.171*** (0.050)	0.185*** (0.056)	0.191*** (0.056)
Unemployed		-0.018 (0.018)		-0.020 (0.020)
log(income)		-0.017 (0.457)		0.090 (0.458)
Effects of payday access	18.3%	18.6%	18.5%	19.1%
Average suicide deaths (per 100,000)	7.0	7.0	7.0	7.0
Additional suicide deaths (per 100,000)	1.3	1.3	1.3	1.3
Observations	283	283	283	283
Number of counties	44	44	44	44
Controls		X		X
County trends	X	X	X	X
County fixed effects & Year fixed effects	X	X	X	X
Cluster	county	county	county	county

Table 4: Relationship Between Suicide Attempts and Payday Access: by Age

This table represents zip-level regressions of suicide attempts on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable of each column is the number of suicide attempts by the denoted age group. “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. “Average suicide attempts (per 100,000)” is the average of suicide attempts in the denoted group. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID) provided by the Agency for Healthcare Research and Quality (AHRQ). All models report standard errors clustered by county, which are reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Aged under 19	20–24	25–44	25–54	25–64	Under 45	Under 55	Under 65	65 and over
Payday	0.086 (0.088)	0.054 (0.107)	0.151*** (0.047)	0.119** (0.049)	0.113** (0.047)	0.134*** (0.040)	0.114*** (0.044)	0.109** (0.042)	0.018 (0.177)
log(wage)	-0.038 (0.104)	0.055 (0.131)	0.029 (0.096)	-0.001 (0.084)	0.003 (0.076)	0.005 (0.081)	-0.012 (0.074)	-0.008 (0.069)	0.025 (0.190)
log(employment)	0.046 (0.118)	0.027 (0.101)	0.022 (0.058)	0.023 (0.058)	0.050 (0.058)	0.033 (0.053)	0.032 (0.055)	0.052 (0.056)	-0.112 (0.191)
log(establishment)	0.133 (0.224)	0.484* (0.272)	0.123 (0.163)	0.103 (0.153)	0.039 (0.155)	0.110 (0.152)	0.093 (0.145)	0.043 (0.149)	0.483* (0.278)
Total number of discharges (per 1,000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.001 (0.001)
Effects of payday access	9.0%	5.5%	16.3%	12.6%	12.0%	14.3%	12.1%	11.5%	1.8%
Average suicide attempts (per 100,000)	38.6	84.9	75.6	70.9	62.7	51.8	52.3	49.4	24.6
Additional suicide attempts (per 100,000)	3.5	4.7	12.3	9.0	7.5	7.4	6.3	5.7	0.4
Observations	11,901	10,096	12,835	13,176	13,255	13,370	13,529	13,563	8,252
Number of zip codes	1,713	1,450	1,850	1,900	1,912	1,931	1,955	1,960	1,183
Controls	X	X	X	X	X	X	X	X	X
Border trends	X	X	X	X	X	X	X	X	X
County trends	X	X	X	X	X	X	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X	X	X	X
Cluster	county	county	county	county	county	county	county	county	county

Table 5: Relationship Between Suicide Attempts and Payday Access: by Age and Sex

This table represents zip-level regressions of suicide attempts on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable of each column is the number of suicide attempts by the denoted age-sex group. “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. “Average suicide attempts (per 100,000)” is the average rate of suicide attempts in the denoted group. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits the coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

	(1)	(2)	(3)	(4)
	Aged under 65		Aged 65 and over	
	male	female	male	female
Payday	0.147***	0.083*	0.082	-0.029
	(0.050)	(0.050)	(0.223)	(0.209)
log(wage)	-0.005	-0.007	-0.238	0.243
	(0.079)	(0.081)	(0.303)	(0.247)
log(employment)	-0.067	0.123*	-0.242	-0.075
	(0.062)	(0.072)	(0.262)	(0.201)
log(establishment)	0.005	0.073	0.241	0.531
	(0.197)	(0.157)	(0.408)	(0.388)
Total number of discharges (per 1,000)	0.000	0.001	0.007***	0.000
	(0.000)	(0.000)	(0.002)	(0.000)
Effects of payday access	15.8%	8.7%	8.5%	-2.9%
Average suicide attempts (per 100,000)	36.6	52.2	25.6	22.2
Additional suicide attempts (per 100,000)	5.8	4.5	2.2	-0.6
Observations	12,274	13,137	6,095	6,655
Number of zip codes	1,766	1,894	874	953
Controls	X	X	X	X
Border trends	X	X	X	X
County trends	X	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X	X
Cluster	county	county	county	county

Table 6: Relationship Between Suicide Attempts and Payday Access: by Race

This table represents zip-level regressions of suicide attempts on the payday access indicator. The dependent variable of each column is the number of suicide attempts by the denoted age-race group. "Effects of payday access" is the exponentiated coefficient of the "payday" variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. "Average suicide attempts (per 100,000)" is the average rate of suicide attempts in the denoted group. "Additional suicide attempts (per 100,000)" is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits the coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

	(1)		(2)		(3)		(4)		(5)		(6)	
	White		White		Aged under 65		Black		Aged under 65		All Minority	
	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over
Payday	0.119*** (0.046)	0.053 (0.184)	0.144 (0.149)	-0.519 (0.553)	0.101 (0.094)	-0.501 (0.606)						
log(wage)	0.085 (0.081)	0.157 (0.217)	-0.169 (0.161)	-0.795 (1.028)	-0.170** (0.085)	0.010 (0.432)						
log(employment)	0.072 (0.066)	-0.178 (0.195)	0.255* (0.151)	0.712 (0.777)	0.024 (0.079)	0.015 (0.505)						
log(establishment)	0.098 (0.129)	0.729** (0.295)	-0.525** (0.262)	-0.437 (1.784)	-0.022 (0.220)	-0.445 (0.839)						
Total number of discharges per 1,000	0.000 (0.000)	0.000 (0.001)	0.000 (0.000)	0.016** (0.007)	0.000 (0.000)	0.012** (0.005)						
Effects of payday access	12.6%	5.4%	15.5%	-40.5%	10.6%	-39.4%						
Average suicide attempts (per 100,000)	42.4	23.3	46.0	13.6	61.4	30.7						
Additional suicide attempts (per 100,000)	5.4	1.3	7.1	-5.5	6.5	-12.1						
Observations	13,457	7,695	5,650	1,057	7,790	2,225						
Number of zip codes	1,944	1,103	810	151	1,117	319						
Controls	X	X	X	X	X	X						
Border trends	X	X	X	X	X	X						
County trends	X	X	X	X	X	X						
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X						
Cluster	county	county	county	county	county	county						

Table 7: Relationship Between Suicide Attempts and Payday Access: by Insurance Type

This table represents zip-level regressions of suicide attempts on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable of each column is the number of suicide attempts by patients of the denoted insurance type. “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. To save space, this table omits the coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

	(1) Private insurance	(2) Medicaid	(3) Medicare
Payday	0.124** (0.058)	-0.046 (0.093)	0.072 (0.089)
log(wage)	-0.029 (0.082)	0.031 (0.108)	0.066 (0.147)
log(employment)	0.099 (0.068)	0.056 (0.096)	-0.056 (0.120)
log(establishment)	0.131 (0.139)	-0.210 (0.216)	0.128 (0.216)
Total number of discharges per 1,000	0.000** (0.000)	0.001 (0.001)	0.002 (0.001)
Effects of payday access	13.2%	-4.5%	7.5%
Observations	12,874	11,238	10,391
Number of zip codes	1,856	1,615	1,492
Controls	X	X	X
County trends	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X
Cluster	county	county	county



Table 8: Relationship Between Suicide Attempts and Payday Access: by Household Income

This table represents zip-level regressions of suicide attempts on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable of each column is the number of suicide attempts by the denoted characteristics of zip codes. I split zip codes into those above (“Top 50 pct”) and below (“Bottom 50 pct”) the median share of household income in three income ranges: \$20,000–\$40,000, \$12,000–\$50,000, and \$20,000–\$50,000. For example, column 1 is a regression among zip codes whose share of household income in the range of \$20,000–\$40,000 is above the median share of that income range. The median share of household income for \$20,000–\$40,000 range is 0.28, \$12,5000–\$50,000 is 0.51, and \$20,000–\$50,000 is 0.40 (the Decennial Census 1990). “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. “Average suicide attempts (per 100,000)” is the average rate of suicide attempts in the denoted group. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits the coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

	(1)		(2)		(3)		(4)		(5)		(6)	
	Share of households		Share of households		Share of households		Share of households		Share of households		Share of households	
	w/ Income	\$20,000–\$40,000	w/ Income	\$20,000–\$40,000	w/ Income	\$12,000–\$50,000	w/ Income	\$20k–\$50k	w/ Income	\$20k–\$50k	w/ Income	\$20k–\$50k
	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct
Payday	0.159** (0.072)	0.053 (0.058)	0.140** (0.071)	0.057 (0.060)	0.189*** (0.058)	0.033 (0.059)						
log(wage)	-0.005 (0.105)	-0.008 (0.074)	-0.005 (0.093)	-0.010 (0.076)	0.040 (0.093)	-0.028 (0.069)						
log(employment)	0.020 (0.080)	0.037 (0.073)	0.068 (0.073)	0.011 (0.076)	0.032 (0.076)	0.037 (0.083)						
log(estabishment)	0.210 (0.187)	-0.001 (0.196)	0.162 (0.172)	0.009 (0.200)	0.258 (0.183)	-0.072 (0.225)						
Total number of discharges per 1,000	0.000 (0.000)	0.003** (0.001)	0.000 (0.000)	0.004*** (0.002)	0.000 (0.000)	0.004** (0.001)						
Effects of payday access	17.2%	5.4%	15.0%	5.9%	20.8%	3.4%						
Average suicide attempts (per 100,000)	56.0	50.4	56.2	50.6	53.9	51.5						
Additional suicide attempts (per 100,000)	9.7	2.7	8.4	3.0	11.2	1.7						
Observations	6,540	7,063	6,506	7,097	6,540	7,004						
Number of zip codes	955	1,018	953	1,020	955	1,011						
Controls	X	X	X	X	X	X						
Border & county trends	X	X	X	X	X	X						
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X						
Cluster	county	county	county	county	county	county						

Table 9: Relationship Between Suicide Attempts and Payday Access: by Other Demand Factors

This table represents zip-level regressions of suicide attempts on the payday access indicator. The dependent variable of each column is the number of suicide attempts by the denoted characteristics group. For each demand factor, “Top 50 pct” (“Bottom 50 pct”) zip codes have above (below) the median share of each demand factor. The median share of single parents in zip code level is 0.05, that for divorced people is 0.06, and that for high-school graduates is 0.34 (the Decennial Census 1990). “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to payday loans. “Average suicide attempts (per 100,000)” is the average rate of suicide attempts in the denoted group. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits the coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

	(1)		(2)		(3)		(4)		(5)		(6)	
	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct	Top 50 pct	Bottom 50 pct
Payday	0.115** (0.059)	0.064 (0.068)	0.087* (0.048)	0.075 (0.096)	0.107* (0.056)	0.084 (0.058)						
log(wage)	-0.076 (0.050)	-0.003 (0.110)	-0.048 (0.051)	0.048 (0.123)	-0.014 (0.098)	0.018 (0.069)						
log(employment)	-0.098 (0.087)	0.137 (0.089)	-0.016 (0.082)	0.084 (0.080)	0.067 (0.070)	-0.010 (0.078)						
log(estabishment)	-0.228 (0.213)	0.343** (0.153)	-0.065 (0.213)	0.130 (0.164)	0.207 (0.176)	0.103 (0.256)						
Total numberdischarges per 1,000	0.006*** (0.001)	0.000* (0.000)	0.006*** (0.001)	0.000* (0.000)	0.000 (0.000)	0.001 (0.001)						
Effects of payday access	12.2%	6.6%	9.1%	7.8%	11.3%	8.8%						
Average suicide attempts (per 100,000)	59.8	43.7	59.8	44.3	59.0	43.4						
Additional discharges (per 100,000)	7.3	2.9	5.4	3.5	6.7	3.8						
Observations	6,853	6,750	6,905	6,705	6,667	6,943						
Number of zip codes	995	978	998	976	972	1,002						
Controls	X	X	X	X	X	X						
Border trends	X	X	X	X	X	X						
County trends	X	X	X	X	X	X						
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X						
Cluster	county	county	county	county	county	county						

Table 10: Relationship Between Suicide Attempts (Poisoning) and Payday Access: by Age and Insurance Type

This table represents zip code-level regressions of suicide attempts on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable of each column is the number of suicide attempts (by poisoning) by patients of the denoted age group (Panel A) or insurance type (Panel B). “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to access to payday loans. “Average suicide attempts (per 100,000)” is the average rate of suicide attempts in the denoted group. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits the coefficients of controls, border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Panel A: By Age Category

	(1) All population	(2) Aged under 65	(3) Aged 65 and over
Payday	0.127*** (0.047)	0.132*** (0.046)	-0.008 (0.193)
Effects of payday access	13.5%	14.1%	-0.9%
Average suicide attempts (per 100,000)	40.6	44.5	15.7
Additional discharges (per 100,000)	5.5	6.3	-0.1
Observations	13,522	13,487	7,308
Number of zip codes	1,952	1,947	1,047
Controls	X	X	X
Border trends	X	X	X
County trends	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X
Cluster	county	county	county

Table 10: Relationship Between Suicide Attempts (Poisoning) and Payday Access: by Age and Insurance Type (Continued)

Panel B: By Insurance Type

	(1) Private insurance	(2) Medicaid	(3) Medicare	(4) Self-pay
Payday	0.182*** (0.058)	-0.019 (0.114)	0.070 (0.095)	0.409 (0.924)
Effects of payday access	19.8%	-1.9%	6.9%	50.5%
Observations	12,674	10,685	9,718	2,573
Number of zip codes	1,824	1,535	1,394	369
Controls	X	X	X	X
Border trends	X	X	X	X
County trends	X	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X	X
Cluster	county	county	county	county

Table 11: Relationship Between Suicide Attempts (Poisoning) and Payday Access: by Substance Type

This table represents zip-level regressions of suicide attempts on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable of each column is the number of suicide attempts (by poisoning) by patients of the denoted age group and substance type used for suicide attempts. “Effects of payday access” is the exponentiated coefficient of the “payday” variable, which is the percentage change in the expected number of suicide attempts due to payday loans. “Average suicide attempts (per 100,000)” is the average rate of suicide attempts in the denoted group. “Additional suicide attempts (per 100,000)” is the percentage change in the expected number of suicide attempts attributable to access to payday loans. To save space, this table omits the coefficients of border trends, county-specific trends, zip code fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). All models report standard errors clustered by county, which are reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		
	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	All ages	Aged under 65	
Payday	0.202*** (0.070)	0.198*** (0.065)	0.276 (0.312)	0.134*** (0.050)	0.127*** (0.047)	0.134*** (0.050)	-0.085 (0.241)	-0.108 (0.166)	-0.104 (0.178)	-0.882 (1.311)									
log(wage)	-0.020 (0.096)	-0.014 (0.101)	-0.165 (0.294)	-0.064 (0.063)	-0.054 (0.062)	-0.064 (0.063)	0.093 (0.341)	-0.089 (0.214)	-0.035 (0.193)	-1.727 (1.677)									
log(employment)	0.138* (0.075)	0.161** (0.078)	-0.160 (0.308)	0.120** (0.053)	0.121** (0.050)	0.120** (0.053)	0.008 (0.233)	-0.297 (0.187)	-0.257 (0.181)	-0.670 (1.036)									
log(establishment)	0.137 (0.153)	0.094 (0.165)	0.748 (0.554)	0.126 (0.127)	0.153 (0.123)	0.126 (0.127)	0.806 (0.536)	0.908** (0.371)	1.067*** (0.413)	-0.345 (1.599)									
Total number of discharges (per 1,000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.002)	0.001 (0.000)	0.001 (0.000)	0.001 (0.000)	0.000 (0.000)	0.004 (0.003)	0.003 (0.002)	0.010 (0.011)									
Effects of payday access	22.4%	21.9%	31.8%	14.3%	13.5%	14.3%	-8.1%	-10.2%	-9.9%	-58.6%									
Average suicide attempts (per 100,000)	16.9	18.3	7.8	29.6	26.8	29.6	8.6	2.1	2.3	1.2									
Additional suicide attempts (per 100,000)	3.8	4.0	2.5	4.2	3.6	4.2	-0.7	-0.2	-0.2	-0.7									
Observations	12,479	12,444	5,431	13,102	13,172	13,102	5,744	7,384	7,118	1,474									
Number of zip codes	1,796	1,791	778	1,888	1,898	1,888	823	1,058	1,020	211									
Controls	X	X	X	X	X	X	X	X	X	X									
Border trends	X	X	X	X	X	X	X	X	X	X									
County trends	X	X	X	X	X	X	X	X	X	X									
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X	X	X	X	X									
Cluster	county	county	county	county	county	county	county	county	county	county									

Table 12: Robustness Tests

This table represents zip-level regressions of suicide risks on the payday access indicator. All regressions report coefficients from Poisson regressions estimated by the Poisson quasi-maximum likelihood estimator (QMLE). The dependent variable is suicide attempts by age group. Panels A and B perform subsample analyses. Panel A restricts the sample by zip code population, and Panel B by the number of non-zero suicide counts of zip codes (out of 7 year-counts). Panel C reports the results from a falsification test, where “FakePayday” equals 1 if a zip code is within 25 miles of a border of states that do not allow payday loans (e.g., Vermont, Massachusetts, and Connecticut) after 1997, and 0 otherwise (See Figure 7). To save space, this table omits the coefficients of controls, border trends, county-specific trends, ZIP fixed effects, and year fixed effects. The data used to produce this table are State Inpatient Data (SID). Standard errors are clustered by county and reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Panel A: Dropping Sparsely Populated Zip Codes

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline sample: all zip codes					
	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over	Aged under 65	Aged 65 and over
Payday	0.109** (0.042)	0.018 (0.177)	0.095** (0.041)	-0.021 (0.162)	0.102** (0.040)	0.023 (0.164)
Observations	13,563	8,252	12,033	8,064	9,213	7,339
Number of zip codes	1,960	1,183	1,728	1,155	1,321	1,051
Controls	X	X	X	X	X	X
Border trends	X	X	X	X	X	X
County trends	X	X	X	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X
Cluster	county	county	county	county	county	county

Table 12: Robustness Tests (Continued)

	(1)		(2)		(3)		(4)		(5)		(6)	
	No zero suicide attempt count		Aged 65 and over		Aged under 65		Number non-zero >= 5		Aged under 65		Number non-zero >= 2	
	Aged under 65		Aged 65 and over		Aged under 65	Aged 65 and over	Aged 65 and over		Aged under 65		Aged 65 and over	
Payday	0.140*** (0.049)		0.017 (0.183)		0.103** (0.043)		0.025 (0.177)		0.106** (0.043)		0.026 (0.173)	
Observations	7,172		6,320		9,914		7,654		12,942		8,186	
Number of zip codes	1,028		905		1,422		1,096		1,864		1,173	
Controls	X		X		X		X		X		X	
Border trends	X		X		X		X		X		X	
County trends	X		X		X		X		X		X	
ZIP fixed effects & Year fixed effects	X		X		X		X		X		X	
Cluster	county		county		county		county		county		county	

Table 12: Robustness Tests (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	All ages	All ages	Aged under 65	Aged under 65	Aged 65 and over	Aged 65 and over
FakePayday	-0.004 (0.032)	-0.004 (0.033)	0.027 (0.037)	0.028 (0.037)	-0.048 (0.075)	-0.045 (0.074)
log(wage)		0.004 (0.056)		-0.006 (0.070)		0.024 (0.190)
log(employment)		0.038 (0.056)		0.053 (0.057)		-0.112 (0.193)
log(establishment)		0.109 (0.150)		0.041 (0.149)		0.489* (0.279)
Total number of discharges per 1,000		0 (0.000)		0.000 (0.000)		0.001 -0.001
Observations	14,063	13,662	13,951	13,563	8,330	8,252
Number of zip codes	2,009	1,976	1,993	1,960	1,190	1,183
Controls		X		X		X
Border trends	X	X	X	X	X	X
County trends	X	X	X	X	X	X
ZIP fixed effects & Year fixed effects	X	X	X	X	X	X
Cluster	county	county	county	county	county	county