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Payday Lending and the Opioid Epidemic

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ABSTRACT

State payday lending laws are among the most contested consumer financial regulations in the United States, and the debate over whether permitting payday lending helps or harms households has been conducted almost entirely in terms of household financial outcomes. We examine whether the consequences of these laws extend beyond the consumer financial system to public health. Exploiting state statutes that allow or prohibit payday lending in a stacked difference-in-differences design, we find that opioid-related deaths rise by 17.5% per year in states that legalize payday lending relative to states whose laws remain unchanged. The increase appears only after legalization and not before, is absent for six other leading causes of death, persists after excluding states with concurrent changes in legislative control, and is also observed in border counties of prohibiting states that are adjacent to permitting states. Opioid pill shipments also increase after legalization, which is consistent with greater access to opioid drugs as one channel, although we regard this evidence as suggestive. The results indicate that the spillovers of payday lending regulation documented within consumer finance may extend to health outcomes, a consideration relevant to the state-level debate over whether to permit, restrict, or prohibit payday lending.

Keywords: payday lending, consumer financial regulation, credit access, opioid epidemic, drug overdose mortality, public health

JEL Codes: G23, G28, I18

1. Introduction

Payday lending is one of the most contested domains in consumer financial regulation, and the decision over whether to permit it rests almost entirely with the states. The resulting patchwork ranges from permissive frameworks that impose few constraints on loan terms to outright prohibitions of storefront payday lending: by 2017 there were 14,348 payday loan storefronts in the United States—roughly one-fifth of the 78,196 bank branches operating that year (Federal Reserve Bank of St. Louis, 2019; Statista, 2020)—even though payday lending was prohibited in 14 of the 50 states. Whether permitting payday lending helps or harms households remains deeply contested (Bhutta et al., 2016; Melzer, 2011; Morse, 2011; Zinman, 2010), but the debate has been conducted almost entirely in terms of household financial outcomes. This paper examines whether the consequences of state payday lending laws extend beyond household balance sheets to public health: specifically, whether legalizing payday lending is associated with a change in opioid-related mortality.

The opioid epidemic is one of the defining public policy problems of the period. More than one million people have died from drug overdoses in the United States since 1999 (Centers for Disease Control and Prevention, 2021); opioids were involved in two-thirds of those deaths by 2018 (Wilson et al., 2020); the number of opioid overdose deaths in 2021 was more than six times the 1999 level (Centers for Disease Control and Prevention, 2023), after a surge of nearly 30% in 2020 alone (The Wall Street Journal, 2021); and the crisis has been described as 'the biggest public health epidemic of a generation' (Davenport et al., 2019).[1] Its economic burden is estimated at no

[1] The term 'epidemic' usually refers to a communicable or transmissible disease. However, in the context of the opioid crisis, 'epidemic' signifies the widespread misuse of both prescription and non-prescription

less than $631 billion over 2015–2018 (Davenport et al., 2019),[2] and the U.S. Department of Health and Human Services declared it a public health emergency in October 2017 (Centers for Medicare & Medicaid Services, 2020). Research on the causes of the epidemic has focused on healthcare practices, government regulation, and social factors; the role of household access to credit, and of the financial regulation that governs it, has not been examined. Yet there are reasons to expect a link, and its direction is not obvious. Opioids are costly, and a payday loan may relax the financial constraints that limit their purchase; the financial difficulties associated with payday borrowing documented by Melzer (2011, 2018)—a cascade of missed bills, delayed medical care, and strained household finances—could also raise stress and, in turn, drug overdoses. Alternatively, expanded access to credit may ease financial constraints and reduce borrowers' stress, lowering reliance on opioid drugs. Whether access to payday lending aggravates or mitigates opioid mortality is therefore an empirical question.

We exploit the variation in access generated by state statutes that allow or prohibit payday lending. Over 1999–2014, seven states legalized payday lending while seven others prohibited it throughout. Using a stacked difference-in-differences (DID) design in line with the recommendations of Baker et al. (2022), we compare county-level opioid-related deaths in legalizing states with those in states whose laws remain unchanged. Although payday lending laws might be correlated with consumer behavior or macroeconomic conditions within a state, Bhutta,

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opioids. This misuse became particularly alarming before the widespread recognition of the high addictive potential of these medications (U.S. Department of Health and Human Services, 2021).

[2] This estimated economic burden includes costs associated with additional healthcare services for individuals affected by opioid use disorder, premature mortality, criminal justice activities, child and family assistance programs, education programs, and lost productivity. The estimate does not account for impacts in areas with inadequate data, such as household productivity, reduced productive output at work, and diminished quality of life for affected individuals.

Goldin, and Homonoff (2016) show that macroeconomic trends (such as unemployment rates and income per capita) and consumer behavior (including credit card use) are similar between states that changed their payday lending laws and states that have always been restrictive. We find that opioid-related deaths rise by 17.5% per year in states that legalize payday lending relative to states whose payday lending laws remain unchanged throughout the sample period.

Four sets of results make it less likely that this association reflects something other than the change in law. First, the increase in opioid-related deaths appears only after legalization, not before, which is inconsistent with pre-existing differences in trends between the two groups of states. Second, falsification tests show no comparable increase for any of six other leading causes of death, so the result is unlikely to reflect broad changes in state healthcare systems that coincide with legalization—for example, a state reducing healthcare spending at the same time it authorizes payday lending. Third, to address time-varying state-level factors, including the possibility that payday lending laws respond to lobbying by payday lenders, we examine border counties of prohibiting states that are adjacent to permitting states; with state-by-year fixed effects absorbing time-varying conditions in the home state, access to payday lending across the state border is also associated with higher opioid-related deaths. Fourth, excluding states where the change in payday lending law coincided with a change in control of the state legislature—and hence possibly with other policy shifts, such as reductions in funding to treat opioid addiction—leaves the findings similar.

We then examine whether the relationship is accompanied by greater access to opioid drugs. Using county-level data on opioid pill shipments, we find that the distribution of opioid pills also increases after states allow payday lending. This evidence is consistent with greater access to opioid drugs being one channel through which payday lending access relates to opioid mortality,

although the shipment data cover only part of the sample period and do not identify who ultimately purchases the pills, so we view this channel as suggestive rather than conclusive.

This paper contributes to the literature on the consequences of access to high-cost consumer credit and, more broadly, on the spillovers of financial regulation. One body of work emphasizes the liquidity and insurance benefits of payday credit for households with few alternatives: access to payday loans can expand credit availability and improve household financial conditions (Karlan & Zinman, 2010; Morgan et al., 2012; Zinman, 2010), mitigate financial distress after income shocks (Morse, 2011), help households absorb expenditure shocks (Wilson et al., 2010), and reduce borrowers' depression and stress (Karlan & Zinman, 2010). A contrasting body of work emphasizes the debt-trap costs of high-cost credit: access to payday loans makes it harder for households to pay mortgages, rent, and utility bills, leads them to delay needed medical care, spills over into other consumer credit markets, and pushes marginal households out of the banking system (Campbell et al., 2012; Melzer, 2011, 2018). Nearly all of this evidence evaluates payday lending policy through financial outcomes—bill payment, overdrafts, credit access, and bank account closures. We extend the literature along the outcome dimension: the effects associated with permitting payday lending appear to reach beyond the direct borrower–lender relationship, and beyond the consumer financial system, to a public-health outcome. For the state-level debate over whether to permit, cap, or prohibit payday lending, this points to a cost that falls outside the financial domain.

The paper also contributes to the interdisciplinary literature on the causes and consequences of the opioid epidemic. Research on its consequences has examined social, economic, and financial effects (Aliprantis et al., 2023; Cornaggia et al., 2022; Jansen, 2023; Li & Zhu, 2019; Ouimet et al., 2019), and research on its causes has focused on healthcare practices

(Damiescu et al., 2021; Saloner et al., 2018; Stoicea et al., 2019), government regulation (Jones et al., 2019; Soelberg et al., 2017; Waldrop, 2017), and social factors (Eitan et al., 2017; Jalali et al., 2020). The finance studies in this literature treat the epidemic as a shock to financial outcomes; we reverse the direction of inquiry and ask whether a financial-sector policy is related to the epidemic itself. To the best of our knowledge, this is the first evidence linking household access to credit, through payday lending, to opioid overdose mortality.

The remainder of the paper is organized as follows. Section 2 presents the data and summary statistics; Section 3 reports the main empirical results; Section 4 provides additional analyses and robustness checks; and Section 5 concludes.

2. Data and key variables

2.1 Opioid mortality

We obtain opioid-related mortality data at the county level from 1999 to 2014 from the NCHS. The restricted vital statistics data we accessed begin in 1989. However, deaths were classified under the ninth revision of the International Classification of Diseases (ICD) before 1999 and under the tenth revision from 1999 onward. To ensure consistency, we begin the sample in 1999. The sample ends in 2014 because the most recent change in payday lending law in our sample occurred in 2009. Following Cornaggia et al. (2022) and Li & Zhu (2019), we use drug overdose (or poisoning) deaths as a proxy for opioid deaths. Specifically, deaths in the ICD-10 categories X40–X44 (unintentional overdose), X60–X64 (suicide by self-poisoning), Y10–Y14 (undetermined intent), and X85 (homicide) are classified as opioid-related. Wilson et al. (2020) show that more than two-thirds of drug overdose deaths involve opioids, which supports the use of drug overdose deaths as a proxy for opioid-related deaths. The key dependent variable, Opioid

Deaths, is the total number of opioid-related deaths in a county in a year.

2.2 Payday lending statutes

Payday loans are an alternative financial service: short-term, high-cost loans in small amounts (usually $500 or less) that are typically due on the borrower's next payday and are repaid through a post-dated check or electronic access to the borrower's bank account. Borrowers can obtain a payday loan without collateral or a minimum credit score. The industry emerged in the mid-1990s and has expanded rapidly, with 14,348 storefronts in the United States as of 2017. Payday loans have become a common financing option in the United States, used by 12 million Americans annually (Federal Reserve Bank of St. Louis, 2019).

We rely on the variation in the regulation of payday lending across states and over time for identification. Before 1990, most states prohibited payday lending, with the exceptions of New Mexico, Nevada, and Delaware, which allowed payday lending in 1916, 1984, and 1987, respectively. In the 1990s, many states, including Alabama, California, the District of Columbia, Florida, Hawaii, Kansas, Louisiana, Minnesota, Mississippi, Montana, Nebraska, North Carolina, Ohio, Oregon, South Carolina, South Dakota, Tennessee, Utah, Washington, and Wyoming, all legalized payday lending. Together with New Mexico, Nevada, and Delaware, these states are considered 'always allowed' states. During our sample period (1999 to 2014), seven states—Missouri (2002), Virginia (2002), New Hampshire (2003), Oklahoma (2003), Alaska (2004), Michigan (2005), and Kentucky (2009)—legalized payday lending. These are our 'treated' states. Finally, seven states, including Connecticut, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and West Virginia, never allowed payday lending during our sample period, making them 'never allowed' states.

2.3 Other variables

To control for any time-varying local socioeconomic conditions, we include controls at the state and county levels. We obtain the number of jobs in a county, county population, and the percentage of African Americans in the state population from the U.S. Census Bureau. We also obtain state median household income, GDP per capita, percentage of the unemployed labor force, and the ratio of the poverty population from the Federal Reserve Bank of St. Louis. The detailed definitions of these variables and their data sources are described in **Table 1**.

2.4 Sample construction

To examine the impact of payday lending access on opioid deaths, we construct a stacked sample. We first create five cohorts corresponding to the years of payday lending law changes in the seven treated states. For each treated state, we retain observations from five years before to five years after the state payday law changes. We use only the seven 'never allowed' states as our control group and also retain observations from five years before to five years after the state payday law changes. The construction of the stacked sample is summarized in **Table 2**.

2.5 Summary statistics

Table 3 presents the summary statistics for the variables. The average number of opioid-related deaths is 48 per county per year. For other causes of death in the United States, the average number of deaths is 99 from cerebrovascular disease, 70 from accidents, 65 from influenza and pneumonia, 30 from suicide, 27 from hypertension, and 21 from Parkinson's disease. On average, each prescription contains 2.34 oxycodone or hydrocodone pills, and 0.23 pills per resident are distributed annually. The average natural logarithms of the number of jobs and the population at

the county-year level are 10.33 and 11.05, respectively. The average natural logarithms of the median household income and GDP per capita are 10.73 and 10.62, respectively. In the sample, the average unemployment rate is 5.51%, 12.3% of the population lives in poverty, and 12.9% of the state population is African American.

3. Regression analyses and main results

To estimate the effect of payday lending access on opioid-related deaths, we employ a Poisson fixed-effects model as follows,

$$E[Opioid\ Death_{i,j,t}] = \exp\{\beta Allowed_{i,t} + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_i\ (or\ \alpha_j) + \alpha_t\}, \quad (1)$$

where $Opioid\ Deaths_{i,j,t}$ represents the opioid-related deaths in state *i,* county *j,* and year *t*. The key independent variable of interest, *Allowed*, equals one if state *i* allows payday lending in year *t*, and zero otherwise. We include either state (α_i) or county (α_j) fixed effects to control for time-invariant local economic conditions and year fixed effects (α_t) to absorb the effects of macroeconomic conditions. With state (county) and year fixed effects, the variation in *Allowed* only comes from states that change their payday lending laws. County-level controls ($X_{j,t}$) include the natural logarithm of the number of jobs and population. The state-level controls ($Z_{i,t}$) include the natural logarithm of median household income and GDP per capita, and the percentage of unemployed, those living in poverty, and African American population. These time-varying county- and state-level characteristics control for local socioeconomic conditions. We cluster the standard errors at the state level because the payday lending laws vary by state.

3.1 Main results

Table 4 presents the baseline results of the impact of payday lending access on opioid-related deaths. In column (1), we include only state and year fixed effects, intentionally omitting control variables to address concerns that some controls could be influenced by payday lending access. The coefficient estimate for *Allowed* is positive and statistically significant, indicating that access to payday lending is associated with 11.1% more opioid-related deaths. In column (2), we add county-level controls to account for time-varying county-level socioeconomic conditions. The coefficient estimate for *Allowed* remains positive and statistically significant. In column (3), the results are similar after adding state-level control variables.

In columns (4)–(6), we include county and year fixed effects. The coefficient estimates for *Allowed* remain positive and statistically significant at the 1% level, though the magnitudes of these estimates are slightly lower than those in columns (1)–(3). After a state allows payday lending, opioid-related deaths are 17.5% higher per year, after accounting for socioeconomic, macroeconomic, and local economic conditions. In economic terms, the estimate in column (6) implies roughly 861 additional opioid-related deaths per year across the states that allowed payday lending during the sample period. [3]

3.2 Dynamics

One concern is that the results might be influenced by systematic differences between states that allow and those that do not allow payday lending. Specifically, states permitting payday

[3] The economic significance is calculated using the equation:
$\sum(number\ of\ counties\ in\ each\ treated\ state\ \times$
$number\ of\ years\ after\ payday\ lending\ was\ allowed\ during\ the\ sample\ period \times 0.175)$.

lending may be experiencing worsening economic conditions. In such scenarios, states could be motivated by fiscal concerns to legalize payday lending. These deteriorating economic conditions could also contribute to increases in opioid drug overdose and, consequently, opioid-related deaths. However, if this were the case, we would expect to see increases in opioid-related deaths even before the enactment of payday lending laws. To investigate this possibility, we examine the dynamics of the effect. Specifically, we estimate the following specification:

$$E[Opioid\ Death_{i,j,t}] = \exp\{\sum_{k=-5}^{5} \beta_k Allowed_i^k + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_j + \alpha_t\}, \qquad (2)$$

where $Allowed_i^k$ equals one if the state is k years before or after it allows payday lending, and zero otherwise. If there is a trend of opioid-related deaths even before the state allows payday lending, $\beta_k's$ will be positive for some $k<0$. Conversely, if the effect is driven by states allowing payday lending, $\beta_k's$ will be close to zero for all $k<0$, but will be positive for some $k>0$.

We plot the coefficient estimates of β_k and their 95% confidence intervals in **Figure 1**. The $\beta_k's$ are all negative and close to zero for $k<0$. In contrast, the $\beta_k's$ are positive in every post-legalization year and statistically distinguishable from zero in most of them. These results suggest that the positive association between payday lending access and opioid-related deaths is less likely to reflect pre-existing trends or systematic differences between the two groups of states. Instead, the increase coincides with the legalization of payday lending.

3.3 Falsification tests

Next, we address the concern that the results might be influenced by confounding events occurring around the same time as the changes in payday lending laws. For instance, if the change in law is prompted by states' incentives to increase revenue in the face of a deteriorating economy, this could lead to a reduction in funding available for the healthcare system. Consequently, the

diminished healthcare funding could escalate opioid-related deaths. However, if this were the scenario, we should observe a similar effect on other causes of death. Conversely, if the results reflect access to payday lending rather than a general deterioration in health conditions, we would expect no comparable effect on other causes of death. We conduct falsification tests by examining the impact of payday lending access on other causes of death in the U.S., such as deaths from cerebrovascular disease, accidents, influenza and pneumonia, suicide, hypertension, and Parkinson's disease, using the following model specification:

$$E[Other\ Cause\ Death_{i,j,t}] = \exp\{\beta Allowed_{i,t} + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_j + \alpha_t\}, \quad (3)$$

where the controls and fixed effects are the same as those used in the baseline analysis.

Table 5 presents the results of the falsification tests. The coefficient estimate for Allowed is insignificant for all six other causes of death. These results make it less likely that the positive association between payday lending and opioid-related deaths reflects confounding events that affect mortality broadly; the association appears to be specific to opioid-related deaths.

3.4 The effects on opioid pill prescriptions

If access to payday lending raises opioid-related deaths by making opioid drugs easier to purchase, access to payday lending should also be associated with greater distribution of opioid drugs. To examine whether this is the case, we focus on county-level opioid pill prescriptions as an alternative dependent variable. The Washington Post provides the Drug Enforcement Administration (DEA) pain pill database to foster a deeper understanding of the opioid crisis. This dataset includes all transactions of oxycodone and hydrocodone pills shipped from manufacturers to pharmacies in the United States between 2006 and 2012. It does not cover all types of opioid pills, as other opioids are in low quantities and have rarely been prescribed in recent years. In

addition, the data do not account for pills that are not dispensed directly to patients but are diverted elsewhere, such as to the black market. Using these data, we estimate the following regression specification:

$$E[OpioidPills_{i,j,t}] = \exp\{\beta_p Allowed_{i,t} + \delta_p' X_{j,t} + \theta_p' Z_{i,t} + \alpha_i \ (or\ \alpha_j) + \alpha_t\}, \quad (4)$$

where the controls and fixed effects are the same as those used in the baseline analysis.

Table 6 shows the impact of payday lending on opioid pill prescriptions. In column (1), we regress the total number of opioid pills shipped to pharmacies in a county each year, scaled by the number of transactions, on payday lending access, with state and year fixed effects. The coefficient estimate for *Allowed* is positive and statistically significant, suggesting that payday lending access is associated with higher opioid pill prescriptions. In column (2), we incorporate county and year fixed effects instead, and the results remain similar. In columns (3) and (4), we scale the total number of opioid pills by the county population. The results are similar. Taken together, these estimates are consistent with greater access to opioid drugs being one possible pathway through which payday lending access relates to opioid-related deaths, although the shipment data cover only 2006–2012 and shipments to pharmacies do not identify who ultimately purchases the pills.

4. Additional analyses and robustness checks

4.1 Neighboring states' payday lending access

To mitigate the concern that the results are influenced by time-varying factors at the state level, such as unobserved socioeconomic conditions or the possibility that payday lending laws respond to lobbying by payday lenders within the state, we examine states that do not allow payday lending but are adjacent to states that do. Specifically, we focus on border counties whose home

states prohibit payday lending but whose neighboring states allow it. Following Melzer (2011), we estimate the following regression specification:

$$E[Opioid\ Death_{i,j,t}] = \exp\{\gamma Border_{i,j,t} + \delta_b' X_{j,t} + \alpha_{it} + \alpha_j\}, \quad (5)$$

where $Border_{i,j,t}$ equals one if county j is in state i that does not allow payday lending but is adjacent to a state that does, and zero otherwise. We include state-by-year fixed effects to control for the home states' time-varying economic conditions. The identifying variation of *Border* stems solely from the neighboring states' payday lending access, not from payday lending laws in their own states.

Table 7 presents the impact of neighboring states' payday lending laws on opioid-related deaths. In column (1), we exclude the bordering counties adjacent to states that are not included in our stacked sample. In column (2), we further exclude all other border counties from each state, thus only including the interior counties as clean controls in this identification strategy. The coefficient estimates for *Border* in both specifications are positive and statistically significant at the 1% level, though the magnitudes are smaller than those from the baseline analysis. Access to payday lending in neighboring states is associated with 7.1% more opioid-related deaths per year. These findings suggest that the relationship between payday lending access and opioid-related deaths is less likely to be driven by time-varying factors at the state level.

4.2 State legislature

One concern is that changes in payday lending laws may coincide with changes in the state legislature. For instance, one party may be more likely to permit payday lending while simultaneously reducing funding available for treating the opioid epidemic. To address this concern, we collect information on the partisan composition of the state legislature from the

National Conference of State Legislatures (NCSL). There are four types of partisan compositions: both legislative chambers have Republican majorities, both chambers have Democratic majorities, neither party holds majorities in both legislative chambers (split), and a non-partisan unicameral legislature (Nebraska). Two states experienced a change in the state legislature concurrent with a change in payday lending law. Oklahoma changed from Democrat to split, and Michigan changed from Republican to split. To mitigate this concern, we exclude these states from the baseline analysis.

Table 8 displays the impact of payday lending on opioid-related deaths after excluding states that concurrently experienced changes in the state legislature. In column (1), we include state and year fixed effects, and in column (2), we incorporate county and year fixed effects. Both coefficient estimates for *Allowed* are positive and statistically significant at the 1% level, though the magnitudes are slightly smaller than those in the baseline analysis. These results suggest that the positive association between payday lending and opioid-related deaths is less likely to be driven by concurrent changes in the state legislature.

4.3 The effects on subcategories of opioid-related deaths

To examine which subcategories of opioid-related deaths account for the baseline result, we estimate the effect of payday lending access on each subcategory separately. In this analysis, we focus on deaths from unintentional drug overdose (ICD-10 codes X40-44), suicide by drug self-poisoning (ICD-10 codes X60-64), and deaths with undetermined intent (ICD-10 codes Y10-14), which are typical causes of drug overdose deaths. This analysis faces a data limitation. The opioid-related deaths, the primary dependent variable in the main analysis, include deaths from all three subcategories. Many counties have data suppressed due to having fewer than ten deaths.

When disaggregating into these finer categories, an increased number of counties are marked as suppressed due to confidentiality concerns.

Table 9 presents the impact of payday lending access on each subcategory of opioid-related mortality. The dependent variable in column (1) is deaths from unintentional overdose. The coefficient estimate for *Allowed* is positive and statistically significant at the 1% level, indicating that payday lending access is associated with 29.3% more deaths from unintentional overdose. In column (2), the cause of death is suicide by drug self-poisoning, where the coefficient estimate for *Allowed* is insignificant. In column (3), the cause of death is undetermined intent, with the coefficient estimate for *Allowed* also being insignificant. Overall, the association is concentrated in deaths from unintentional drug overdoses rather than in suicides by drug self-poisoning or deaths of undetermined intent, although the latter two categories are estimated on far fewer observations because of data suppression.

4.4 Robustness checks

To assess the robustness of the analysis to different outcome definitions, we estimate regressions using two measures: opioid-related deaths and crude death rates per 100,000 population. For both measures, we exclude homicide deaths and, separately, account for suppressed counties by imputing zero deaths. We also examine whether another potential confounder, the adoption of prescription drug monitoring programs, influences the relationship between access to payday lending and opioid-related deaths.

The literature typically counts homicide by drug poisoning as part of drug overdose deaths, and we follow this definition in the baseline analysis. Because such deaths are involuntary from the victim's perspective, we exclude this category as a robustness check. Table 10, column (1),

reports the coefficient estimate for Allowed when homicides are excluded from opioid-related deaths; the magnitude is very similar to that in the main analysis. A similar pattern holds for the crude opioid-related mortality rate (deaths per 100,000 population) in Table 10, column (2), where the point estimate is positive and statistically significant at the 5% level. The findings therefore do not depend on the definition of the outcome.

We use publicly available data on opioid-related deaths from the NCHS. If a county reports fewer than ten deaths, the count is suppressed for confidentiality reasons. In the baseline analysis, we treat these suppressed county-years as missing. To check that the results are not driven by this exclusion, we replace the missing values with zero deaths and include them in the analysis. Columns (3) and (4) of Table 10, which use deaths and mortality rates as outcomes, show that the coefficient estimates for Allowed remain positive and statistically significant. The magnitude under this imputation is larger than in the baseline analysis, with the coefficient in column (3) implying a 28.2% increase in opioid-related deaths.

Prescription Drug Monitoring Programs (PDMPs) are among the most prominent state-level interventions aimed at improving opioid prescribing practices and protecting patients at risk. To check that the results are not confounded by the enactment of PDMPs, we add an indicator for PDMP enactment as an additional control. Table 10, column (5), reports the coefficient estimate for Allowed after controlling for PDMP. The estimate remains positive and statistically significant, with a magnitude similar to that in the main analysis, which suggests that the results are not driven by the staggered adoption of PDMPs.

5. Conclusions

State decisions to permit or prohibit payday lending are usually evaluated by their effects on household finances. This paper examines whether their consequences extend beyond the consumer financial system to public health. Using the variation in access to payday lending created by state statutes that permit or prohibit it, we find that opioid-related deaths rise by approximately 17.5% per year after a state allows payday lending, relative to states whose laws do not change; scaled by the counties and years affected, the estimate implies roughly 861 additional deaths per year across the states that legalized payday lending during our sample period. The increase appears only after legalization, is absent for other causes of death, persists when states with concurrent changes in legislative control are excluded, is also present in border counties of prohibiting states adjacent to permitting states, and is accompanied by an increase in opioid pill shipments, which is consistent with greater access to opioid drugs as one channel. We are cautious in interpretation. The effect is identified from the seven states that changed their laws during the sample period, the pill-shipment evidence covers only part of that period, and our county-level data do not allow us to observe individual borrowers. Within these limits, these results complement the existing payday lending literature by documenting a dimension of the regulation's consequences that credit-based outcome measures do not capture. The effects associated with permitting payday lending are not confined to the direct borrower–lender relationship or to the consumer financial system; they appear to reach public health as well. These spillover effects may be relevant when evaluating payday lending regulation: the trade-off between the liquidity benefits of credit access and its costs is not confined to household balance sheets, and a health dimension belongs in the state-level debate over whether to permit, restrict, or prohibit payday lending.

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Figure 1: Dynamic effect of payday lending on opioid-related deaths

Notes: The figure presents the coefficient estimates of β_k and their 95% confidence intervals from Equation (2). The controls and fixed effects are the same as those in Table 4 column (6).

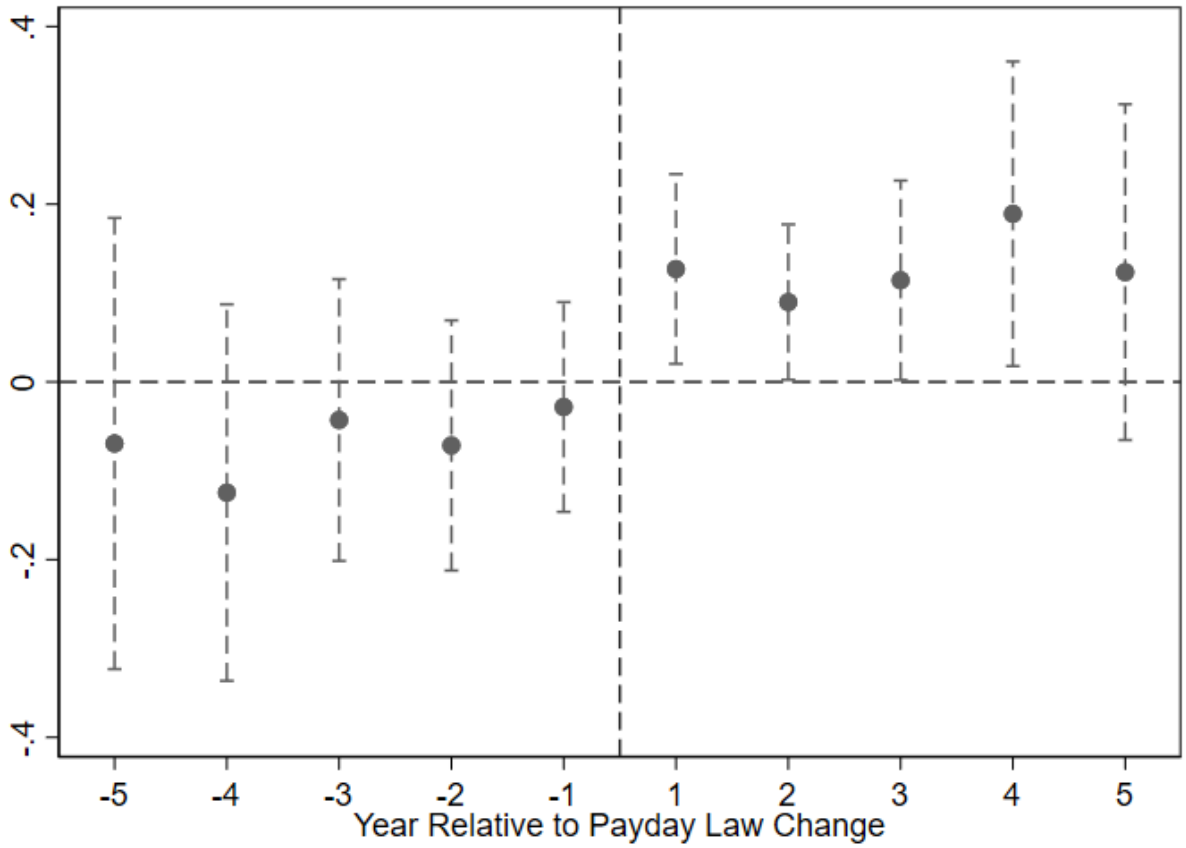


Table 1: Variable definitions

This table provides the definition of the variables used in the paper.

Variables	Definition	Data Source
Dependent Variable		
Opioid Deaths	Opioid deaths at county and year level. Opioid-related deaths are defined based on the ICD-10 underlying cause of death codes X40-44 (unintentional), X60-64 (suicide), Y10-Y14 (undetermined intent), or X85(homicide).	National Center for Health Statistics (NCHS)
Payday Lending Access		
Allowed	Dummy variable equals one if the state *i* allows payday lending in year *t,* and zero Otherwise.	
County-Level Controls		
Ln(No. of Jobs)	Natural logarithm of the number of jobs in a county.	U.S. Census Bureau
Ln(Population)	Natural logarithm of the total population in a county.	U.S. Census Bureau
State-Level Controls		
Ln(Median Income)	Natural logarithm of median household Income.	Federal Reserve Bank of St Louis
Ln(GDP Per Capita)	Natural logarithm of state GDP per Capita.	Federal Reserve Bank of St Louis
Unemployed	Percentage of the labor force that is unemployed.	Federal Reserve Bank of St Louis
Poverty	The ratio of the number of people in each age group whose income falls below the poverty line.	Federal Reserve Bank of St Louis

African American	The proportion of the African American population in a state.	Federal Reserve Bank of St Louis
Other Variables		
Opioid Pills Prescriptions	Total number of opioid pills prescribed and shipped to a county scaled by the number of transactions or population. Only shipments of oxycodone and hydrocodone pills are available in the dataset. Other opioid pills are not included in the data set due to low quantities.	The Washington Post
Cerebrovascular Death	Cerebrovascular disease deaths at the county and year level. Cerebrovascular deaths are defined based on the underlying cause of death codes I60-I69 of ICD-10.	NCHS
Accidents (Unintentional Injuries) Death	Accidents (Unintentional injuries) deaths at county and year level. CVD deaths are defined based on the underlying cause of death codes V01-X59,Y85-Y86 of ICD-10.	NCHS
Influenza and pneumonia Death	Influenza and pneumonia deaths at county and year level. Influenza and pneumonia deaths are defined based on the underlying cause of death codes J09-J18 of ICD-10.	NCHS
Suicide (Intentional self-harm) Death	Suicide (Intentional self-harm) deaths at county and year level. Suicide deaths are defined based on the underlying cause of death	NCHS

	codes *U03,X60-X84,Y87.0 of ICD-10.	
Hypertension Death	Hypertension (Essential hypertension and hypertensive renal disease) deaths at county and year level. Hypertension deaths are defined based on the underlying cause of death codes I10,I12,I15 of ICD-10.	
Parkinson Death	Parkinson deaths at county and year level. Parkinson deaths are defined based on the underlying cause of death codes G20-G21 of ICD-10.	
PDMP	Dummy variable equals one if the state has enacted the prescription drug monitoring program (PDMP) in a year and zero otherwise.	www.pdmpassist.org/State

Table 2: Stacked sample construction

Notes: This table presents the details of the stacked sample construction.

Cohort	Treated States	Control States
2002	Missouri, Virginia	CT, MA, MD, NJ, NY, PA, WV
2003	New Hampshire, Oklahoma	CT, MA, MD, NJ, NY, PA, WV
2004	Alaska	CT, MA, MD, NJ, NY, PA, WV
2005	Michigan	CT, MA, MD, NJ, NY, PA, WV
2009	Kentucky	CT, MA, MD, NJ, NY, PA, WV

Table 3: Summary statistics

Notes: This table presents the summary statistics of the variables used in the paper. Definitions for all variables are provided in Table 1.

	N	Mean	SD	p25	p50	p75
Dependent Variable						
Opioid Death	6211	47.965	55.304	15.000	26.000	57.000
Other Dependent Variables						
Cerebrovascular Death	15632	99.230	141.126	21.000	40.000	107.000
Accidents Death	14896	70.206	96.650	18.000	32.000	77.000
Influenza and pneumonia Death	11434	64.804	98.954	14.000	27.000	68.000
Suicide Death	8520	29.568	29.767	12.000	19.000	39.000
Hypertension Death	8282	26.671	37.799	0.000	15.000	34.000
Parkinson Death	8356	20.556	24.243	0.000	14.000	31.000
Total pills/prescription	6138	2.336	0.656	1.922	2.240	2.645
Total pills/population	6110	0.233	0.144	0.148	0.204	0.279
County-Level						
Ln(No. of Jobs)	18269	10.325	1.568	9.198	10.126	11.329
Ln(Population)	18269	11.054	1.451	10.058	10.870	11.977
State-Level						
Ln(Median Income)	18757	10.729	0.186	10.631	10.730	10.842
Ln(GDP Per Capita)	18757	10.623	0.246	10.469	10.612	10.801
Unemployed	18757	5.506	1.615	4.500	5.200	6.000
Poverty	18757	0.123	0.031	0.096	0.117	0.147
African American	18757	0.129	0.069	0.082	0.117	0.181

Table 4: The effects of payday lending access on opioid-related deaths

Notes: This table displays the coefficient estimates from regressions analyzing the impact of access to payday lending (*Allowed*) and various controls on opioid-related deaths from 1999 to 2014. The dependent variable across all regression models is opioid-related deaths in county *j* in year *t*. Columns (1) and (4) exclude control variables, while Columns (2) and (5) incorporate county-level controls, and Columns (3) and (6) add state-level controls. Definitions for all variables are provided in Table 1. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Allowed	0.111*	0.238***	0.197***	0.228***	0.231***	0.175***
	(1.93)	(4.31)	(3.52)	(4.19)	(3.21)	(2.60)
Ln(No. of Jobs)		-0.149	-0.156		-0.372	-0.743
		(-1.01)	(-1.06)		(-0.54)	(-0.79)
Ln(Population)		1.157***	1.167***		1.329***	1.866**
		(5.31)	(5.39)		(2.80)	(2.13)
Ln(Median Income)			0.014			-0.231
			(0.06)			(-1.37)
Ln(GDP Per Capita)			-0.131			-0.086
			(-0.28)			(-0.15)
Unemployed			-0.003			-0.004
			(-0.08)			(-0.10)
Poverty			0.234			-0.120
			(0.22)			(-0.12)
African American			-14.256*			-13.879
			(-1.90)			(-1.18)
Adj R^2	0.118	0.690	0.691	0.836	0.836	0.837
Obs.	6211	6182	6182	6082	6055	6055
Fixed Effects	State and Year	State and Year	State and Year	County and Year	County and Year	County and Year

Table 5: Falsification tests

Notes: This table presents coefficient estimates from regressions examining the impact of payday lending access (*Allowed*) and other controls on the leading causes of deaths in the United States from 1999 to 2014. The dependent variables analyzed are deaths from cerebrovascular diseases in column (1), accidents (unintentional injuries) in column (2), influenza and pneumonia in column (3), suicide (intentional self-harm) in column (4), and hypertension in column (5), and Parkinson's disease in column (6). The controls employed are the same as those specified in Table 4, columns (3) and (6). All variables are defined in Table 1. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dep. Var.	Cerebrovascular	Accidents	Influenza and pneumonia	Suicide	Hypertension	Parkinson's
Allowed	0.017	0.039	0.061	-0.027	-0.075	0.025
	(1.02)	(1.41)	(1.50)	(-1.27)	(-1.51)	(0.71)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.951	0.923	0.928	0.763	0.817	0.695
Obs.	15131	14496	10746	7281	5639	5273
Fixed Effects	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year

Table 6: The effects of payday lending on opioid pill prescriptions

Notes: This table presents coefficient estimates derived from equation (4), analyzing the relationship between payday lending access (*Allowed*) and opioid pill distribution from 2006 to 2012. In columns (1) and (2), the dependent variable is the total number of opioid pills divided by the number of prescriptions shipped to county *j* in year *t*. In columns (3) and (4), it is the total number of opioid pills divided by the county's population in year *t*. The controls employed are the same as those specified in Table 4, columns (3) and (6). Definitions for all variables can be found in Table 1. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Dep. Var.	**(1)** **Total pills/ transactions**	**(2)** **Total pills/ transactions**	**(3)** **Total pills/ population**	**(4)** **Total pills/ population**
Allowed	0.029**	0.032***	0.109***	0.106***
	(2.55)	(2.70)	(4.53)	(4.52)
County controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes
Adj R^2	0.022	0.053	0.021	0.069
Obs.	6110	6108	6110	6108
Fixed Effects	State and Year	County and Year	State and Year	County and Year

Table 7: The effects of neighbor states' payday lending on opioid-related deaths

Note: This table presents the coefficient estimates derived from equation (5) for the years 1999 to 2014. The dependent variable is the number of opioid-related deaths in state *i*, county *j*, in year *t*. Column (1) excludes border counties adjacent to states not included in the stacked sample, while Column (2) further excludes all other border counties from each state, thus only including the interior counties. Definitions for all variables are provided in Table 1. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	**(2)**
Border	0.073***	0.071***
	(3.83)	(3.70)
County controls	Yes	Yes
Adj R^2	0.850	0.852
Obs.	5824	2863
Fixed Effects	State×Year and County	State×Year and County

Table 8: The effects of payday lending on opioid-related deaths excluding states changing the legislature

Notes: This table presents coefficient estimates from regressions examining the impact of payday lending access (*Allowed*) and other controls on opioid-related deaths from 1999 to 2014, excluding states that have changed their state legislature. The states excluded are Michigan and Oklahoma. The dependent variable, opioid-related deaths in county *j* in year *t*, remains consistent across all regression specifications. The controls employed are the same as those specified in Table 4, columns (3) and (6). Definitions for all variables are provided in Table 1. t-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	**(2)**
Allowed	0.153***	0.144***
	(3.70)	(3.42)
County controls	Yes	Yes
State controls	Yes	Yes
Adj R^2	0.685	0.836
Obs.	5894	5779
Fixed Effects	State and Year	County and Year

Table 9: The effects of payday lending access on each category of opioid-related deaths

Notes: This table presents coefficient estimates from regressions analyzing the impact of payday lending access (*Allowed*) and controls on subcategories of opioid-related deaths from 1999 to 2014. Column (1) covers unintentional overdose deaths, Column (2) focuses on suicide deaths by drug self-poisoning, and Column (3) examines deaths with undetermined intent. The controls employed are the same as those specified in Table 4, columns (3) and (6). Definitions for all variables are provided in Table 1. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Dep. Var.	**(1) Unintentional Overdose**	**(2) Suicide by Drug Self-poisoning**	**(3) Undetermined Intent**
Allowed	0.293***	-0.102	0.127
	(3.30)	(-1.54)	(0.28)
County controls	Yes	Yes	Yes
State controls	Yes	Yes	Yes
Adj R^2	0.807	0.149	0.829
Obs.	4490	969	1037
Fixed Effects	County and Year	County and Year	County and Year

Table 10: Robustness checks

Notes: This table displays coefficient estimates from robustness analyses spanning 1999 to 2014. It examines opioid-related deaths excluding homicides in column (1), opioid-related death rates also excluding homicides in column (2), opioid-related deaths including counties with fewer than ten deaths in column (3), opioid-related death rates including counties with fewer than ten deaths in column (4), and opioid-related deaths as analyzed in the main study but with an additional adjustment for PDMP programs in column (5). The controls employed are the same as those specified in Table 4, columns (3) and (6). Definitions for all variables are provided in Table 1. t-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Dep. Var.	**Opioid Death without Homicide**	**Opioid Death Rate without Homicide**	**Opioid Death with Imputation for Suppressed Counties**	**Opioid Death Rate with Imputation for Suppressed Counties**	**Opioid Death Adjust for PDMP**
Allowed	0.175***	0.124**	0.282***	0.570***	0.183**
	(2.58)	(1.98)	(2.79)	(3.35)	(2.36)
County controls	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.837	0.487	0.837	0.393	0.838
Obs.	6026	3775	9721	5819	5951
Fixed Effects	County and Year	County and Year	County and Year	County and Year	County and Year