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Payday Lending Access and Suicide*

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Abstract This paper examines whether access to payday loans affects mortality, focusing on suicide among working-age adults. Payday lending has ambiguous welfare effects: short-term credit may help liquidity-constrained households smooth shocks, but high fees and rollovers can generate debt cycles and financial stress. The analysis combines cause-specific mortality from the National Vital Statistics System Multiple Cause-of-Death files (1994-2004) with county-year population counts from SEER, disaggregated by age, sex, and race, to construct suicide and cancer death rates of adults aged 19-64. Following Melzer (2011)'s border-based approach, the empirical strategy exploits a quasi-experimental setting in which Massachusetts, New Jersey, and New York prohibit payday lending but border states that legalize it; counties within 25 miles of a payday-allowing neighbor are classified as having de facto access. Difference-in-differences models with county and year fixed effects, complemented by staggered adoption event-study estimates, show no statistically significant effects of payday access on suicide rates overall or by age group. Placebo regressions for cancer mortality rates likewise reveal no systematic relationship with payday access.

Keywords Payday loans, border-based identification, suicide, mortality, staggered adoption.

JEL Classification D14, I12, I18, G51.

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1. INTRODUCTION

The rapid expansion of the payday lending industry has generated substantial economic and policy controversy, as these high-cost, short-term loans are disproportionately used by financially vulnerable households who lack access to mainstream credit. The central debate concerns the welfare implications of expanding credit access for liquidity-constrained households. Proponents argue that payday loans provide valuable liquidity, enabling households to smooth consumption in the face of unexpected expenditure shocks and to meet immediate necessities. When households encounter sudden expenses, such as utility bills or rent arrears, additional credit can prevent missed payments and eviction, thereby mitigating short-run hardship (Morse, 2011; Karlan and Zinman, 2009). Critics, by contrast, emphasize that high fees and frequent rollovers embedded in payday loans can trap borrowers in "cycles of debt," ultimately exacerbating long-run financial distress (Wilson et al., 2010). Persistent financial hardship is a well-known risk factor for deteriorating mental health and, in turn, heightened suicidal ideation.

Existing empirical work reflects this theoretical ambiguity. A growing literature examines the effects of payday lending on financial outcomes such as overdue bills, bankruptcy, and other measures of hardship (Melzer, 2011), as well as on non-fatal health outcomes including suicide attempts and self-reported mental health (Lee, 2019). These studies generally find that access to payday loans is associated with increased financial strain and deteriorating subjective well-being. At the same time, survey and consumption-based evidence indicates that borrowers may experience short-run benefits when payday loans relax binding liquidity constraints during periods of acute need (Lawrence and Elliehausen, 2008; Dobridge, 2016). Taken together, the literature portrays payday lending as a mechanism that can both mitigate and exacerbate financial distress, leaving its net welfare impact ambiguous.

This paper examines whether access to payday lending affects mortality, with particular emphasis on suicide. Suicide is adopted as the primary outcome for two reasons. Substantively, it can be interpreted as a revealed-preference indicator of severe failures in economic and psychological well-being. Empirically, suicide mortality is consistently recorded over time using standardized ICD codes and is not subject to classical reporting error or survey nonresponse in the way that self-reported measures of distress are.

As a placebo outcome, the analysis also considers deaths from cancer, a cause of death that is unlikely to respond to short-run financial distress and for which the costs of diagnosis and treatment are large relative to the typical size of

a payday loan. Finding no effect on cancer mortality rates serves as a robustness check, helping to confirm that the estimates for suicide are not driven by broader shifts in mortality unrelated to payday lending.

The empirical analysis combines county-level mortality data from 1994 to 2004 with county-year population counts disaggregated by age, sex, and race. The primary outcome is the suicide rate, defined as suicide deaths per 100,000 adults aged 19-64, corresponding to the working-age population eligible for payday loans.

A key identification concern is that states' decisions to legalize payday lending may be endogenous to underlying economic conditions or trends in financial distress. A simple comparison of "payday-legal" and "payday-banned" states could therefore conflate the effect of loan access with the political or economic factors that induced some states to allow it. Following Melzer (2011), the paper addresses this concern using a geographic, border-based design. The analysis focuses on states that formally prohibit payday lending and exploits within-state differences in access arising from proximity to neighboring states that legalize payday loans. Counties close enough to a payday-allowing border, in which residents can plausibly cross to borrow, are classified as having access, whereas otherwise counties in the same prohibiting state that are farther from allowing borders, or adjacent only to non-allowing states, serve as controls. Because treated and control counties share the same home-state legal regime and macroeconomic environment, this design isolates plausibly exogenous variation in payday access generated by the location and timing of neighboring-state legalizations rather than by endogenous state policy choices.

Mortality rates are modeled using difference-in-differences specifications with county and year fixed effects, comparing changes in suicide and cancer mortality rates between counties with and without payday access before and after payday lending becomes available across the border. Across baseline and age-specific specifications for younger (ages 19-41) and older (ages 42-64) working-age adults, the estimated effects of payday access on suicide rates are small in magnitude and statistically indistinguishable from zero. Placebo regressions for cancer mortality rates similarly reveal no systematic relationship with payday access. These findings stand in contrast to prior work documenting sizable effects of payday lending on suicide attempts and self-reported mental health, and suggest that any adverse impacts on psychological well-being may not translate into detectable changes in suicide rates at the county-year level over the period studied.

This paper makes two contributions to the literature on high-cost credit and

health. First, it provides new evidence on the relationship between payday loan access and suicide by bringing a different institutional setting and a border-based identification strategy to bear. Using a measure of access for Massachusetts, New Jersey, and New York linked to age-sex-race-specific mortality rates, the analysis finds no statistically significant effects on suicide or cancer rates. These results offer a useful contrast to prior studies that document harmful impacts on subjective well-being. Second, the study contributes to the broader health economics literature on the consequences of financial shocks and credit conditions for health and survival by combining administrative mortality data with a quasi-experimental research design and robustness checks based on modern difference-in-differences estimators.

The remainder of the paper is organized as follows. Section 2 provides institutional background on the payday lending industry and develops a conceptual framework linking access to credit with economic hardship and suicide risk. Section 3 first introduces the border-based measure of payday loan access, then describes the construction of the mortality and population data, and finally details the outcome variables, covariates, and empirical strategy. Section 4 presents the main results, including heterogeneity analyses by age group and robustness checks. Section 5 concludes by discussing the implications of the findings for the welfare evaluation of payday lending.

2. INSTITUTIONAL BACKGROUND

2.1. BACKGROUND ON PAYDAY LOANS

Payday loans are typically short-term, high-cost loans that target households with limited access to mainstream financial services. In most states and over the period considered in this paper, typical loans carry principal balances below $500 and maturities of about two to four weeks, timed to coincide with the borrower's next paycheck. Melzer (2011) describes loan sizes in the $200–$1,000 range, while Lee (2019) and the CFPB (Consumer Financial Protection Bureau, 2024) report that most loans are under $500. Fees commonly range from $15 to $30 per $100 borrowed, implying annual percentage rates that can exceed 300 percent.

To be eligible for payday loans, borrowers are generally required to provide valid identification, proof of income, an active checking account, and a post-dated check or authorization for electronic withdrawal at the loan's maturity. These design features accomplish two objectives. First, they allow lenders to extend credit rapidly to individuals who are in desperate need. Second, they provide lenders with direct access to borrowers' cash flow at the next pay date,

mitigating default risk despite the absence of collateral or extensive screening. As a result, payday borrowers are disproportionately lower-income and more likely to be liquidity-constrained than the general population.

The payday lending industry is a relatively recent phenomenon. Stegman (2007) documents that storefront payday lenders were essentially absent prior to the early 1990s. Regulatory changes and rising demand for short-term credit allowed the sector to expand rapidly; by the mid-2000s, there were more than 20,000 payday outlets nationwide, exceeding the combined number of McDonald's and Starbucks locations (Zinman, 2010). Meller (2019) reports roughly 23,000 storefront payday lenders - around twice the number of McDonald's restaurants - highlighting the continued demand for this form of credit in 2019. Although the number of states that permit payday lending has fallen from more than forty in the mid-2000s to about thirty-two in 2023, payday credit remains widely available (Horowitz and Kravitz, 2023). At the same time, the market has been shifting from storefronts to online lenders: recent CFPB evidence indicates that roughly two-thirds of the short-term, small-dollar loan market is now digital. Taken together, the continued prevalence of storefront lenders and the rapid expansion of online payday credit indicate that both the supply of and demand for payday loans remain high.

2.2. SUICIDE AS A MEASURE OF SUBJECTIVE WELL-BEING

This paper regards suicide mortality as a key indicator of severe breakdowns in subjective well-being. Conventional measures of welfare in economics often rely on self-reported life satisfaction or mental health scales. Although informative, such survey-based outcomes can be difficult to compare objectively across individuals, regions, and time because of reporting heterogeneity, adaptation, and reference-group effects (Daly, Wilson, and Johnson, 2013). In contrast, suicide represents a rare but objectively recorded outcome that reflects severe psychological distress and the perceived absence of a valuable future.

Economic models of suicide provide a theoretical basis for interpreting suicide as a revealed-preference indicator of well-being. Hamermesh and Soss (1974) develop a life-cycle model in which an individual chooses to end life when the discounted value of expected future utility falls to zero. In this framework, adverse economic shocks that permanently depress income or increase the disutility of life can raise the hazard of suicide. Henry and Short (1968) frustration-aggression theory offers a complementary sociological mechanism: individuals experiencing persistent goal failure and blocked opportunities may internalize frustration in the form of self-directed violence or externalize it as

aggression toward others.

Empirically, suicide rates exhibit strong associations with macroeconomic conditions, unemployment, indebtedness, and other markers of financial hardship. A growing literature in health economics and economic epidemiology documents that suicide is counter-cyclical and especially responsive to shocks that deteriorate households' financial position or social status (Ruhm, 2000; Gerdtham and Johannesson, 2003). These properties make suicide a natural outcome for evaluating the welfare consequences of high-cost credit.

The analysis in this paper focuses on suicide rates among working-age adults. Restricting attention to adults aged 19-64 is natural because access to payday loans typically requires an active bank account and documented income, so the relevant population is the employed or recently employed adult population. As a robustness check, the same empirical strategy is also applied to deaths from cancer. Cancer mortality rates should be largely unrelated to short-run changes in financial stress over the horizons on which payday lending operates: although financial resources can influence access to cancer screening and treatment, the associated medical expenditures are typically large and long term, far exceeding the relatively small, short-maturity amounts provided by payday loans. The absence of effects on cancer mortality rates therefore serves as a placebo test, helping to verify that the estimates for suicide are not driven by broader shifts in mortality that are unrelated to payday lending.

2.3. CONCEPTUAL CHANNELS LINKING PAYDAY LENDING AND SUICIDE

The welfare implications of expanding access to loans are theoretically ambiguous. On one side, payday loans may enhance welfare by relaxing liquidity constraints and allowing households to smooth consumption in the face of transitory shocks. When individuals confront unexpected expenses, access to immediate cash can prevent evictions, utility shutoffs, or other sharp disruptions that would otherwise impose large short-run utility losses (Morse, 2011; Karlan and Zinman, 2009). In this "consumption-smoothing" channel, payday loans act as a form of informal insurance, potentially reducing psychological distress and, in extreme cases, mitigating suicide risk. Survey evidence is consistent with this view: Lawrence and Elliehausen (2008) find that payday borrowers report high satisfaction with the product and perceive it as providing a valuable service despite fully recognizing its high cost. Similarly, Dobridge (2016) shows that during periods of temporary financial distress, access to payday credit increases spending on food at home relative to similar households without such access,

suggesting that payday loans can help relax binding liquidity constraints in the short run.

On the other side, the structure of payday loans may amplify financial fragility and stress, particularly for borrowers with limited financial literacy or self-control. High fees, very short maturities, and the ease of repeated borrowing create conditions under which borrowers can become trapped in "cycles of debt" (Wilson et al., 2010). Evidence from Melzer (2011) indicates that access to payday loans is associated with increased financial hardship - such as difficulty paying bills and delaying medical or utility payments - rather than improved stability. Lee (2019) shows that easier access to payday credit raises the incidence of suicide attempts and self-reported mental health problems, suggesting that for many borrowers the net effect of credit access is detrimental to psychological well-being. Lu and Stabile (2020) further documents that restricting payday lending at the state level reduces suicides and drug overdoses, consistent with the view that payday credit can exacerbate behavioral health risks.

Behavioral models help rationalize these adverse effects. Present-biased preferences and limited self-control can lead individuals to overvalue immediate liquidity relative to the future debt burden (Laibson, 1997). Over-optimism and misperception of compound interest may cause borrowers to underestimate the cost of repeated rollovers or the difficulty of repaying in full (Ausubel, 1991; Stango and Zinman, 2009). Because lenders rely on automatic debits from checking accounts at the next payday, borrowers who misjudge their repayment capacity may face cascading overdraft fees, bounced payments, or the need to take out new loans to service old ones. These dynamics can generate persistent financial stress, conflict within households, and, in some cases, reliance on alcohol or other substances as coping mechanisms - factors that are known correlates of suicide risk.

Combining these insights, the net effect of payday lending on suicide is ultimately an empirical question. If the consumption-smoothing benefit dominates, expanded access could reduce extreme distress by helping households navigate shocks. If debt-trap and behavioral-bias mechanisms dominate, expanded access may increase sustained financial strain and thereby elevate suicide risk. The existing empirical literature tends to support the latter view, but evidence on completed suicide remains limited. The framework in this paper therefore treats payday lending as a policy-relevant shock to the distribution of financial stress among working-age adults and focuses on suicide rates as the primary outcome.

3. DATA AND EMPIRICAL STRATEGY

This section describes the construction of the payday-access measure, the mortality and population data, and the empirical strategy used to estimate the effect of payday lending on mortality among working-age adults. The core idea is to exploit county-year variation in exposure to payday lending generated by geographic proximity to state borders where payday loans become legal, while holding constant common trends in mortality through fixed effects.

3.1. MEASURING PAYDAY LOAN ACCESS

The key explanatory variable is a county-year measure of access to payday lending, constructed along the lines of Melzer (2011) distance-based design. During the sample period, Massachusetts, New Jersey, and New York all effectively prohibited storefront payday lending through interest-rate caps and related regulations, while their neighboring states - Delaware, New Hampshire, Pennsylvania, and Rhode Island - permitted payday loans. Following Melzer (2011), the analysis exploits this cross-state regulatory discontinuity to identify counties in prohibiting states whose residents can obtain payday loans by crossing the border. Although individual-level cross-border borrowing is not directly observed in our data, prior evidence suggests that this mechanism is meaningful. Melzer (2011) documents that payday lenders disproportionately locate near the borders of payday-prohibiting states: zip codes within 25 miles of a prohibiting-state border have roughly 16% more payday stores. Melzer also shows that access effects are larger in counties with a higher share of workers commuting to payday-allowing states, consistent with lower travel costs facilitating cross-border borrowing. In addition, Spiller (2006) discusses Massachusetts residents traveling to New Hampshire for payday loans. Taken together, these facts provide credible suggestive evidence that residents near prohibiting-state borders cross state lines to obtain payday loans.

For each county in Massachusetts, New Jersey, and New York, the distance from the county's geographic centroid to the border of the nearest payday-allowing state is computed. Counties whose centroids lie within 25 miles of such a border are classified as having access to payday lending, while all other counties in the same prohibiting states are classified as non-access counties. The 25-mile radius is intended to capture counties where it is plausibly feasible for residents to travel across the border to obtain loans, despite the absence of storefront payday lenders in their own state.

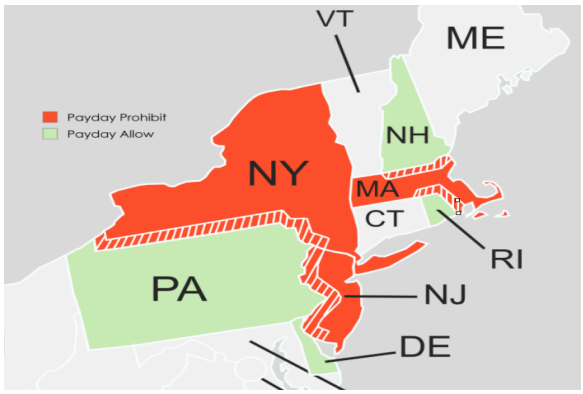

Figure 1: PAYDAY ACCESS MEASUREMENT. Red states prohibit payday lending; green states legalized it during 1994–2004. Cross-hatched counties in Massachusetts, New Jersey, and New York are within 25 miles of a payday-allowing border and are treated as having access; non-hatched counties in these states serve as controls. Source: Authors' illustration based on Lee (2019), created with mapchart.net.

Figure 1 illustrates the regional setting. The green states (Delaware, New Hampshire, Pennsylvania, and Rhode Island) allow payday lending, whereas the red states (Massachusetts, New Jersey, and New York) prohibit it. The cross-hatched counties in the prohibiting states fall within the 25-mile radius and are treated as having access to payday loans in the empirical analysis; non-hatched counties in the same three states serve as controls. In this region, neighboring states legalize payday lending at different times, so border counties in New Jersey and New York gain access in 1997, whereas those in Massachusetts do not gain access until 2000.

Formally, let $Treated_c$ be a time-invariant indicator equal to one for counties whose centroid lies within 25 miles of a payday-allowing state and zero otherwise. The timing of exposure is governed by the year in which the adjacent payday-allowing state first legalizes storefront payday loans. For each county-year (c,t), define a post-treatment indicator $Post_{ct}$ that equals one in years when at least one bordering allowing state has already legalized payday lending and zero otherwise. The treatment variable of interest is the interaction $PaydayAccess_{ct} = Treated_c \times Post_{ct}$, which equals one when a treated county is exposed to payday lending through a neighboring state's legalization and zero in all other county-year cells. This construction parallels Melzer's cross-border access measure, but is applied here to cause-specific mortality data for Massachusetts, New Jersey, and New York.

3.2. DATA AND SAMPLE CONSTRUCTION

The empirical analysis is based on a panel that links cause-specific mortality counts from the National Vital Statistics System (NVSS) Multiple Cause-of-Death (MCOD) files, produced by the National Center for Health Statistics (NCHS), to population denominators from the Surveillance, Epidemiology, and End Results (SEER) program.

The NVSS MCOD files provide universe data on deaths in the United States and report the underlying cause of death, coded according to the International Classification of Diseases (ICD). During the 1994-2004 sample period, causes are classified under ICD-9 (1994-1998) and ICD-10 (1999-2004). The shift from ICD-9 to ICD-10 raises the possibility of artificial breaks in cause-specific series. However, comparability studies reported in the CDC's "Injury in the United States: 2007 Chartbook" indicate that suicide and overall injury mortality are essentially unaffected by the revision, with comparability ratios very close to one. Suicide mortality is therefore treated as consistently measured over time, and no explicit bridging adjustments are applied (Bergen, Chen, Warner and Fingerhut, 2008).

The MCOD files contain detailed information on each decedent, including the county of residence, year of death, single-year age, sex, and race. Using these fields, deaths are aggregated to county-year-age-sex-race cells. Let c index counties, t years, a age in single years, s sex, and r race. For each (c,t,a,s,r), the analysis computes (i) deaths with suicide as the underlying cause and (ii) deaths with cancer as the underlying cause.

To convert counts into rates, these cell-level mortality counts are merged with SEER county-year population estimates disaggregated by single-year age, sex, and race. After harmonizing FIPS county codes and age variables across the two data sources, a common grid of (c,t,a,s,r) cells is constructed and the corresponding population is attached. Suicide and cancer rates are then defined as deaths per 100,000 residents in each cell. Working with rates rather than raw counts is essential for comparability: it nets out differences and changes in population size across counties, years, and demographic groups, so that mechanical population growth does not masquerade as an increase in mortality risk. It also aligns the analysis with standard epidemiological practice and allows regression coefficients to be interpreted directly as changes in deaths per 100,000 population.

The analysis focuses on three Northeastern states - Massachusetts, New Jersey, and New York - that prohibit payday lending throughout the 1994-2004 sample period but border states that legalize payday loans (Delaware, New Hamp-

Variables	Mean	Std. Dev.
Age	49.168	11.477
Female	0.365	0.481
Race		
White	0.745	0.436
Black	0.235	0.424
Other	0.019	0.138

Table 1: SUMMARY STATISTICS OF DECEASED POPULATION. This table provides descriptive statistics for the individual-level analysis sample. The sample consists of 230,782 deaths among adults aged 19–64 in New Jersey and New York between 1994 and 1996 and in Massachusetts between 1994 and 1999.

shire, Pennsylvania, and Rhode Island). Within each of these prohibiting states, counties are classified according to the payday access measure. The sample is restricted to the working-age population that is eligible to access payday loans.

Table 1 reports summary statistics for the main covariates. The mean age is 49.2 years, and 36.5 percent of observations are female. The racial composition is 74.5 percent White, 23.5 percent Black, and 1.9 percent Other races. These patterns closely reflect the underlying demographic structure of counties in the study states and the focus on adult mortality. In the empirical analysis below, all regressions control for age, sex, and race, in addition to county and year fixed effects, so any remaining differences in the observable composition of counties with and without payday access are absorbed within the regression framework.

3.3. OUTCOME VARIABLES AND COVARIATES

The primary outcome is the suicide mortality rate per 100,000 population. For each cell (c,t,a,s,r), suicide rate is defined as:

$$SuicideRate_{ctasr} = \frac{Suicides_{ctasr}}{Population_{ctasr}} \times 100{,}000.$$

As a placebo outcome, the cancer mortality rate is defined analogously:

$$CancerRate_{ctasr} = \frac{CancerDeaths_{ctasr}}{Population_{ctasr}} \times 100{,}000.$$

Suicide deaths are identified from the underlying cause of death using standard ICD codes for intentional self-harm under ICD-9 and ICD-10. These codes cover

suicides by multiple methods, including self-poisoning involving drugs or medications, gas inhalation, firearms, and other means of self-inflicted injury. We follow the conventional approach in the literature by defining suicide using ICD codes for intentional self-harm and excluding drug- and alcohol-related deaths without documented intent, which are coded separately (Schwandt, 2018; Steelesmith et al., 2019; Hoffmann et al., 2023). Accordingly, our measure captures deaths coded as intentional self-harm and does not encompass the broader category of "deaths of despair" Case and Deaton (2017). Cancer mortality serves as a comparison outcome that is unlikely to respond directly to changes in payday loan access.

The main covariates are basic demographic characteristics of the cell: single-year age, an indicator for female, and a categorical race variable for White, Black, and Other (with White as the reference group).

3.4. EMPIRICAL SPECIFICATION

The baseline empirical strategy is a difference-in-differences design estimated on the county-by-demographic-cell panel. For a given outcome Y_{ctasr}, the following specification is estimated:

$$Y_{ctasr} = \beta\, PaydayAccess_{ct} + X'_{ctasr}\gamma + \mu_c + \lambda_t + \varepsilon_{ctasr}, \tag{1}$$

where $PaydayAccess_{ct}$ is the treatment indicator; $X'_{ctasr}\gamma$ is the vector of demographic controls (age, sex, race); μ_c are county fixed effects, capturing time-invariant differences across counties in baseline mortality and demographic composition; λ_t are year fixed effects, capturing common shocks to mortality, such as macroeconomic conditions or national health policy changes; and ε_{ctasr} is an error term.

Because both μ_c and λ_t are included, the main effects of $Treated_c$ and $Post_{ct}$ are absorbed. The coefficient of interest, β, is identified from differential changes over time in suicide rates between counties that gain access to payday lending via neighboring-state legalization and counties that do not. Equation(1) is estimated separately for the full working-age sample and for two subgroups: younger adults and older working-age adults.

The same specification is then re-estimated using cancer mortality rates as the outcome variable. In this case, cancer serves as a placebo outcome: to check whether the empirical strategy is spuriously picking up broad shifts in mortality that are unrelated to payday lending.

The validity of the difference-in-differences design rests on the assumption that, absent changes in payday lending access, mortality in treated and control

counties would have followed parallel trends. Section 4 examines pre-trends and presents robustness checks - including specifications based on alternative estimators, age-group-specific analyses, and placebo outcomes - that support this identifying assumption.

4. EMPIRICAL RESULTS

4.1. BASELINE EFFECTS ON SUICIDE

This subsection presents the baseline difference-in-differences estimates of the effect of payday loan access on suicide mortality among working-age adults. Table 2 reports the estimated impact of payday loan access on suicide rates among working-age adults. All specifications are weighted by cell population and include county and year fixed effects; standard errors are clustered at the county level.

In the full working-age sample, the coefficient on payday access variable is small and imprecisely estimated. Without covariates (column 1), the point estimate is 0.075. Adding controls for age, sex, and race has essentially no effect: the estimate rises slightly to 0.079 in column 2. These estimates are small in magnitude, representing less than 1 percent of the sample mean.

Consistent with the descriptive statistics, the controls show that the age term is small and not statistically different from zero in the pooled 19-64 sample, suicide rates are substantially higher among males than females (female coefficient around -10.5), and rates are lower among non-White groups than Whites. Overall, the baseline difference-in-differences estimates provide no compelling evidence that gaining access to payday lending through neighboring-state legalization systematically increases or decreases suicide rates among working-age adults, and the pattern of coefficients across specifications is fully consistent with a negligible or zero causal effect in this setting.

4.2. HETEROGENEITY BY AGE GROUP

The next step examines whether the effect of payday access differs across the life cycle. This subsection estimates the same specification separately for younger adults (ages 19-41) and older working-age adults (ages 42-64).

Table 3 shows that the estimated effects of payday access are uniformly small and statistically insignificant in both age groups. For the younger adults (column 1), the coefficient is 0.181, again quantitatively modest relative to the mean suicide rate of 8.44 per 100,000 and far from conventional significance thresholds.

For the older adults (column 2), the estimate is slightly negative (-0.042) with a mean suicide rate of 8.92 per 100,000, and likewise statistically indistinguishable from zero. These point estimates represent a 2.15% increase relative to the mean for younger adults and a 0.47% decrease for the older cohort; however, both effects are economically negligible and statistically insignificant.

The covariates behave in expected ways. Age within each subgroup is positively associated with suicide among younger adults and negatively associated among older adults, consistent with the familiar hump-shaped profile of suicide risk over the life cycle. Female suicide rates are, on average, about 10 deaths per 100,000 lower than male rates in both age ranges, and Black and Other racial groups exhibit substantially lower suicide rates than Whites. These patterns, however, do not materially alter the main conclusion: there is no statistically

	Suicide Rate	
	(1)	(2)
Payday Access	0.075	0.079
	(0.413)	(0.406)
Age		0.019
		(0.012)
Female		-10.516***
		(0.419)
Black		-3.159***
		(0.372)
Other		-4.265***
		(0.270)
Constant	8.812***	14.263***
	(0.247)	(0.600)
Year FE	Yes	Yes
County FE	Yes	Yes
Observations	166,640	166,640
R-squared	0.005	0.051
Mean dep. var.	8.648	8.648

Table 2: THE EFFECTS OF PAYDAY LOAN ACCESS ON SUICIDE RATE. This table reports difference-in-differences estimates of the effect of payday loan access on suicide mortality per 100,000 adults aged 19–64. Regressions are weighted by cell population, include county and year fixed effects, and control for age, sex, and race (White is the reference group). Robust standard errors clustered at the county level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	(1) Younger	(2) Older
Payday Access	0.181	-0.042
	(0.479)	(0.634)
Age	0.046**	-0.070***
	(0.018)	(0.020)
Female	-10.622***	-10.385***
	(0.507)	(0.375)
Black	-1.709***	-5.405***
	(0.525)	(0.354)
Other	-3.555***	-5.323***
	(0.345)	(0.503)
Constant	13.300***	18.947***
	(0.641)	(1.159)
Year FE	Yes	Yes
County FE	Yes	Yes
Observations	83,446	83,194
R-squared	0.055	0.050
Mean dep. var.	8.436	8.919

Table 3: SUICIDE MORTALITY EFFECTS BY AGE GROUP. This table reports difference-in-differences estimates of the effect of payday loan access on suicide mortality per 100,000 adults. Column (1) restricts the sample to younger adults, while column (2) restricts the sample to older adults. All regressions include county and year fixed effects and control for age, sex, and race (White is the reference group). Observations are weighted by cell population. Robust standard errors clustered at the county level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

detectable effect of payday access on suicide rates in either age group.

4.3. PLACEBO OUTCOME: CANCER MORTALITY RATE

As a robustness check, this subsection considers cancer mortality - a cause of death that should be insensitive to payday loans - as a placebo outcome. If the empirical strategy is isolating the causal effect of payday lending, the estimates for cancer mortality rates should show no statistically significant change.

Across all specifications in Table 4, the estimated effects of payday access on cancer mortality rates are close to zero and show no statistical significance. In the full sample (column 1), the point estimate is 0.664 relative to a mean cancer mortality rate of 93.5 per 100,000, corresponding to less than a 1 percent change. For younger adults (column 2), the coefficient is -0.054 with a mean

	(1) Full	(2) Younger	(3) Older
Payday Access	0.664	-0.054	3.130
	(1.655)	(0.593)	(3.529)
Age	8.947***	1.582***	20.667***
	(0.150)	(0.044)	(0.427)
Female	-8.738***	1.850***	-23.696***
	(1.082)	(0.306)	(2.952)
Black	21.647***	3.362***	50.215***
	(4.484)	(1.035)	(8.814)
Other	-35.396***	-5.686***	-83.865***
	(2.789)	(0.706)	(4.564)
Constant	-260.001***	-33.944***	-862.113***
	(6.076)	(1.509)	(22.330)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	166,640	83,446	83,194
R-squared	0.455	0.106	0.462
Mean dep. var.	93.48	15.76	193.3

Table 4: EFFECTS OF PAYDAY LOAN ACCESS ON CANCER MORTALITY RATES. This table reports difference-in-differences estimates of the effect of payday loan access on cancer mortality rates. Column (1) uses the full working-age sample; columns (2) and (3) restrict the sample to younger and older adults, respectively. All regressions include county and year fixed effects and control for age, sex, and race (White is the reference group). Observations are weighted by cell population. Robust standard errors clustered at the county level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

of 15.8 per 100,000, implying a 0.3 percent decrease and again indicating no systematic impact. Among older adults (column 3), the estimate is 3.13 against a mean of 193.3 per 100,000, corresponding to a 1.6 percent increase; this effect is not statistically significant and remains small in relative terms.

The covariates capture well-known demographic gradients in cancer mortality - strongly increasing with age, higher for men than for women, and elevated among Black individuals - yet these patterns do not interact meaningfully with payday access. The high R-squared values in the cancer regressions confirm that age, sex, and racial composition explain much of the cross-cell variation in cancer deaths, whereas payday access itself explains little.

Taken together, the cancer results suggest that the research design is not

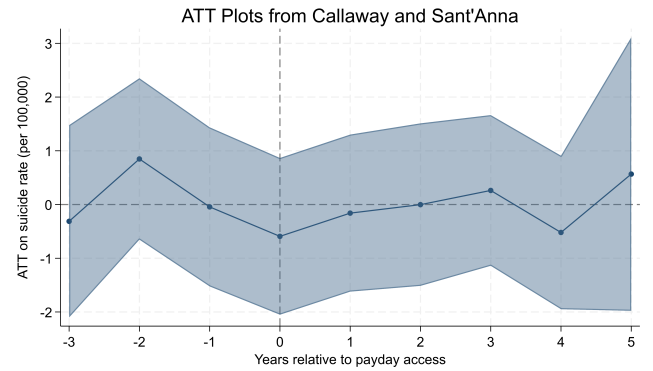

Figure 2: EVENT-STUDY ESTIMATES OF THE EFFECT OF PAYDAY LOAN ACCESS ON SUICIDE MORTALITY. This figure plots event-time treatment effects from Callaway and Sant'Anna (2021) for suicide death rates. The horizontal axis measures years relative to the first year a county gains access to payday lending, with event time -1 normalized to zero. Points show coefficient estimates and shaded bands show 95% confidence intervals. Estimates are based on county-year-age-sex-race cells, weighted by cell population, with standard errors clustered at the county level.

merely picking up broad shifts in mortality risk that are unrelated to payday lending. The absence of effects on cancer mortality is consistent with the null findings for suicide and supports the interpretation that the estimates are not driven by general changes in death rates.

4.4. EVENT-STUDY ESTIMATES

Finally, this subsection assesses the robustness of the baseline findings to alternative difference-in-differences estimators that are appropriate for staggered treatment timing. Because border counties in New Jersey and New York gain access earlier than those in Massachusetts, conventional two-way fixed-effects regressions may be biased when treatment effects vary across cohorts or over time. To address this concern, the analysis applies the difference-in-differences estimator with multiple time periods proposed by Callaway and Sant'Anna (2021), which yields event-time treatment effects that are robust to such heterogeneity. Figure 2 summarizes the dynamic estimates.

Figure 2 plots the event-time coefficients for suicide mortality among adults aged 19-64, normalizing the year immediately before a county first gains access (event time -1) to zero. The pre-treatment coefficients are small and statistically indistinguishable from zero, providing no evidence of differential pre-trends be-

tween counties that will gain payday access and those that never do. Following treatment, the point estimates remain close to zero in all post-treatment years, and the associated confidence intervals are wide but centered around zero, with no systematic upward or downward pattern. Scaling the post-treatment point estimates by the mean suicide rate at event time -1 (9.657) implies percentage changes ranging from roughly -6% to +6%. However, these estimates are imprecise and do not exhibit a consistent sign pattern over event time, consistent with confidence intervals that include both negative and positive values in each post-treatment year.

Overall, the Callaway-Sant'Anna event-study estimates support the parallel-trends assumption underlying the baseline design and reinforce the main conclusion from the preceding subsections: in this setting, the expansion of access to payday lending via cross-border borrowing does not generate statistically significant changes in suicide rates over the horizons observed in the data.

5. CONCLUSION

This paper examines whether expanded access to payday lending affects severe health outcomes, focusing on suicide mortality among working-age adults in three Northeastern states - Massachusetts, New Jersey, and New York - that prohibit storefront payday loans but border states that allow them. Following Melzer (2011)' s border-based identification strategy, we construct a measure of payday access that exploits cross-state policy discontinuities while differencing out time-invariant county characteristics and common state-level shocks. Using this border-based measure of payday access and county-year suicide rates constructed from NVSS MCOD and SEER data, we estimate population-weighted difference-in-differences models with county and year fixed effects. Across all specifications, the estimated effects are both statistically and economically negligible: point estimates are close to zero, and gaining access to payday loans from neighboring-state legalization has no statistically significant effect on suicide rates among adults aged 19-64. Splitting the sample into younger and older adults likewise yields no evidence of differential effects. Placebo regressions using cancer mortality rates and event-study estimates based on the Callaway and Sant'Anna (2021) estimator also reinforce the conclusion that cross-border expansions in payday loan availability do not translate into detectable changes in suicide mortality at the county-year level.

From a policy perspective, these findings offer a nuanced message. The absence of measurable effects on completed suicide may temper concerns that pay-

day lending dramatically increases the most extreme form of self-harm among working-age adults. At the same time, prior research documents increased financial hardship, deteriorations in mental health, and higher rates of suicide attempts associated with easier access to payday loans. Our results therefore should not be interpreted as evidence that payday lending is benign, but as suggesting that any adverse impacts on psychological well-being and financial stability may not manifest as large changes in mortality in this context. In addition, geographic constraints based on physical proximity (within 25 miles of a border) may be less binding in the digital era, so the effects of credit access today might differ.

Another possible explanation for the null results is that relatively strong state-level protections in our study states - such as limits on wage garnishment and certain asset-exemption rules - may attenuate the severity of financial distress, potentially dampening downstream effects on suicide mortality. For example, according to World Population Review (World Population Review, 2025), the three states we examine have comparatively low wage-garnishment limits, while homestead-exemption protections vary and are not uniformly high across them. In light of these institutional features, our null estimates should be interpreted as applying to this regulatory context and may differ in jurisdictions with weaker debtor protections.

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