

Current Draft: September 2025

Supply-Side Opioid Restrictions and the Retail Pharmacy Market

Anne M. Burton and Brandyn F. Churchill*

Abstract

While policymakers routinely limit the sale of goods thought to be of risk to public health, relatively less is known about whether and how these policies affect firm performance. Using 2000-2018 National Establishment Time-Series data and a difference-in-differences strategy, we show that state “pill mill” laws intended to reduce the overprescribing of opioids reduced retail pharmacy sales and employment. These reductions were most pronounced in highly competitive areas and for standalone pharmacies – two characteristics associated with pharmacy drug diversion. Meanwhile, pharmacies located across the border in states without a pill mill law experienced increases in sales and employment. Next, we show that state pill mill laws were associated with an increase in standalone pharmacy closures, though the total number of pharmacies was unchanged. Our results are consistent with these laws adversely affecting pharmacies filling inappropriate opioid prescriptions without meaningfully altering patient access to retail pharmacies.

JEL Codes: I18; K23; M20

Key words: opioids; pharmacy; pill mill

* Burton is an Assistant Professor at University of Texas at Dallas (anne.burton@utdallas.edu). Churchill is an Assistant Professor at American University and a Faculty Research Fellow at NBER (bchurchill@american.edu). We thank Bokyoung Kim, Lilly Springer, and Xuan Zhang, seminar participants at American University, San Diego State University, and the University of Tennessee at Knoxville, and conference participants at APPAM 2024, SEA 2024, and ASHEcon 2025 for helpful comments on earlier versions of this manuscript. We are also grateful to Georgina Cisneros, Carolyn Ouellet, and Sophie Rogers Churchill for excellent editorial assistance. Some of the results in this paper are based on restricted-use and/or proprietary data. Readers interested in obtaining access can contact the authors. We are grateful to Don Walls and Ken Perez for assistance with the NETS data and to Nilesh Shinde for assistance with GIS. All interpretations, errors, and omissions are our own.

1. INTRODUCTION

Governments limit the sale of goods thought to be of risk to public health under the rationale that these products generate negative externalities that are not otherwise internalized by consumers (Conlon and Rao 2023). To reduce consumption of such goods, policymakers have adopted numerous strategies, including raising prices through excise taxes (Cawley et al. 2019; DeCicca et al. 2022); requiring a license to buy, sell, or use a product (Dee et al. 2005; Depew and Swensen 2022); and outright prohibiting sales to at least some consumers (Carpenter and Dobkin 2011; Adda et al. 2012; Knight 2013; Dobkin et al. 2014). Despite the widespread adoption of these policies and large literatures studying how these interventions affect consumer outcomes (e.g., Carpenter and Dobkin 2009; Buchmueller and Carey 2018; Hansen et al. 2023), relatively less is known about whether and how these policies affect firm decisions and outcomes.

This paper provides new evidence on how supply-side drug interventions affect firm performance by studying the relationship between state laws intended to curtail excessive opioid prescribing by pain management clinics, known as “pill mills,” and retail pharmacy market outcomes. Drug overdose is the leading cause of injury mortality in the U.S.; over 70 percent of these deaths are attributable to opioids (NCHS 2023). To combat the ongoing opioid epidemic, state and local officials have adopted numerous measures aimed at limiting the supply of prescription opioids. Broadly speaking, state pill mill laws establish legal authority for state inspections and set training requirements for clinic owners and associated physicians (Mallatt 2017; Maclean et al. 2021; Ziedan and Kaestner 2024). The goal of these policies is to reduce the supply of prescription opioids by (i) closing the most egregious pain management clinics and (ii) reducing the volume of prescribing at remaining facilities. As such, we use the adoption of these

state pill mill laws as natural experiments to study how firms are affected by government policies limiting product sales.

The relationship between state pill mill laws and pharmacy sales depends on the extent to which establishments were previously filling inappropriate opioid prescriptions, whether the laws were effective at reducing inappropriate prescribing, and whether the laws inadvertently discouraged medically justified prescribing. To the first point, a recent paper by Janssen and Zhang (2023) using data on opioid shipments found evidence of drug diversion among small, independent pharmacies, in part due to competitive pressures and the financial incentives of owner-operator pharmacists. Moreover, existing evidence shows that state pill mill laws reduced opioid prescribing (Kaestner and Ziedan 2023), and other prior work suggests that policies discouraging inappropriate prescribing can also reduce the volume of prescriptions for legitimate medical reasons (Buchmueller et al. 2020; Sacks et al. 2021; Alpert et al. 2024).¹ Indeed, while some pill mill pain-management clinics prescribed and filled prescriptions on-site, others directed patients to off-premises establishments whose pharmacists may or may not have known they were filling inappropriate prescriptions (Twilman 2012; Committee on Energy and Commerce of the 115th Congress 2018; IRS 2022). So, while the existing literature suggests that state pill mill laws may have adversely affected pharmacies, the degree to which establishments were affected remains an open empirical question.

We examine the relationship between state pill mill laws and changes in the retail pharmacy industry using 2000-2018 National Establishment Time-Series (NETS) data and a difference-in-

¹ Sacks et al. (2021) found that laws requiring physicians to access a prescription drug monitoring program reduced opioids dispensed to new users. Likewise, Buchmueller et al. (2020) found that Kentucky's prescription drug monitoring program led to substantial declines in opioids prescribed to single-use patients, though Alpert et al. (2024) found that these policies reduced opioid prescriptions among patients presenting with diagnoses for which an opioid prescription would be inappropriate.

differences identification strategy that accounts for the staggered adoption of the policies and potential dynamic treatment effects (Borusyak et al. 2024). First, we find that state pill mill laws were associated with an approximate 5-percent reduction in pharmacy sales and a 3-percent reduction in the number of pharmacy employees. The reductions were limited to the post-adoption period and are robust to alternative controls for time-varying spatial heterogeneity, sample restrictions, and difference-in-differences estimators. Second, we show that these reductions were driven by pharmacies located in highly competitive areas, which is consistent with prior evidence that pharmacies engage in drug diversion to offset competition-induced reductions in revenue (Janssen and Zhang 2023). We also find evidence that pharmacies located across the border in states without a pill mill law experienced an increase in sales and employment, suggesting that some individuals crossed state lines to obtain their prescription opioids, providing an additional example of behavioral responses aimed at evading regulation (Lovenheim and Slemrod 2010; Knight 2013; Hansen et al. 2020; Deiana and Giua 2021; Shakya and Ruseski 2023). Third, we show that these reductions in sales and employment were driven by an increase in pharmacy closures, particularly among standalone (i.e., non-chain) establishments, with surviving establishments experiencing, at most, small increases in sales and employment from these policies.

Prior work showed that state pill mill laws led to sizable reductions in the supply of prescription opioids, increases in illicit opioid prices, and reductions in overall opioid mortality (Meinhofer 2018; Kaestner and Ziedan 2023), though these public health improvements were partially offset by increases in heroin deaths (Meinhofer 2018; Kim 2021). However, given that pharmacies are the most frequent service-delivery touchpoint within the U.S. health care system (Berenbrok et al. 2020; Trygstad 2020; Valliant et al. 2022), and given that the role of pharmacies in delivering health care has expanded over the last several decades (Manolakis and Skelton 2010;

Abouk et al. 2019; Viscari et al. 2021; Smart et al. 2024), it is worth considering whether these policies may have affected pharmacy access for non-opioid users. We do not detect any evidence that state pill mill laws were related to changes in the likelihood that counties had no retail pharmacies or in the number of available pharmacies. That we do not detect a change in overall pharmacy access is consistent with our finding that state pill mill laws harmed pharmacies in locally competitive markets. Moreover, while we find evidence that state pill mill laws reduced pharmacy employment, prior work found that these policies were associated with modest but statistically significant increases in the aggregate labor market outcomes of younger adults. Overall, both our results and findings from prior work are consistent with state pill mill laws adversely affecting pharmacies that were previously filling inappropriate opioid prescriptions without meaningfully altering broader access to retail pharmacies.

This paper contributes to several notable literatures. By showing that state pill mill laws adversely affected the retail pharmacy industry, we add to existing research connecting public health interventions to changes in firm behaviors and outcomes (Adda et al. 2012; Cornelsen and Norman 2012; Nguyen et al. 2019; Butters et al. 2022; Dickson et al. 2025). We also build on work in health economics exploring firm behaviors and outcomes, including advertising (de Frutos et al. 2013; Lawler and Skira 2022), product sales (Bedard and Kuhn 2015; DeCicca et al. 2021; Cotti et al. 2022), employment (Clark and Milcent 2011; Raja 2023), and market structure (Carpenter and Tello-Trillo 2015; Dalton and Bradford 2019). Finally, we most directly add to a literature studying policies intended to curtail the excessive prescribing of prescription opioids (Buchmueller and Carey 2018; Meinhofer 2018; Kim 2021; Mallatt 2022; Shakya and Hodges 2022; Kaestner and Ziedan 2023; Neumark and Savych 2023; Ukert and Polsky 2023). In the study perhaps most related to ours, a working paper by Mallatt (2017) found that state pill mill laws were

associated with a 6.5-percent reduction in the number of establishments categorized as “all other outpatient care centers” – a category that includes pain management clinics – in the 2004-2015 Quarterly Census of Employment and Wages (QCEW) data.²

The rest of this paper proceeds as follows: Section 2 discusses the policy background and summarizes the existing evidence on the effects of state drug policies. Section 3 describes the National Establishment Time-Series data and our difference-in-differences identification strategy that accounts for the staggered adoption of state pill mill laws. Section 4 presents our results on the relationship between these laws and changes in retail pharmacy market outcomes. Finally, Section 5 discusses the policy implications and limitations of our results.

2. POLICY BACKGROUND

Opioid overdoses caused nearly 727,000 deaths between 1999 and 2022. During the earliest years of the epidemic, these deaths were primarily attributable to prescription opioids (CDC 2025). Responding to evidence that rising opioid overdose rates were driven by high-volume prescribing, state governments adopted pill mill laws to identify and penalize inappropriate prescribing. Typical provisions of these laws include (i) requiring pain management clinics to designate a licensed physician as responsible for clinic operations, (ii) setting limits on the supply of opioids that can be dispensed to a patient in a single visit, (iii) capping patient-to-prescriber ratios, (iv) prohibiting opioids from being dispensed at the site of care, (v) permitting routine inspections, and (vi) increasing civil and criminal penalties for those involved in drug diversion (Kennedy-Hendricks

² Mallatt (2017) did not find evidence that OxyContin reformulation or state PDMP laws were related to changes in the number of retail pharmacies. While her QCEW estimates suggested that state pill mill laws were associated with a statistically insignificant 2.2-2.9 percent reduction in the number of pharmacies ($\hat{\beta} = -0.022$ and $SE = 0.014$ in Table 4 column 7; $\hat{\beta} = -0.029$ and $SE = 0.022$ in Table 5 column 7), she found a marginally significant *increase* when using 2004-2015 County Business Patterns data ($\hat{\beta} = 0.018$ and $SE = 0.009$ in Table A2 column 7).

et al. 2016; Brighthaupt et al. 2019). These laws seek to reduce inappropriate prescribing by directly targeting high-risk prescribers and facilities (Rutkow et al. 2015).

During our sample period, 12 states adopted a pill mill law; we report the states and adoption years in Table 1.³ Figure 1 shows that these laws were primarily enacted in states where the majority of pill mills were located: southern and midwestern states, particularly in the Appalachian region (Langford and Feldman 2024). For instance, 90 of the 100 doctors purchasing the most oxycodone nationwide were practicing in Florida in 2010 (Kennedy-Hendricks et al. 2016). Likewise, a bipartisan congressional committee found that one pharmacy in Kermit, West Virginia (population 400) received 9 million opioids over only two years (Committee on Energy and Commerce of the 115th Congress 2018).

3. DATA AND METHODS

3.1 Pharmacy Outcomes: National Establishment Time-Series 2000-2018

To study the retail pharmacy market, we use data from the 2000-2018 National Establishment Time-Series (NETS). The NETS data include time-series information on over 60-million total establishments in the United States from the Duns Marketing Information file. For our purposes, a key feature of the NETS data is that they include Standard Industrial Classification (SIC) codes, which allow us to identify retail pharmacies (SIC 5912). These data include the business name and GPS location, as well as estimated annual sales and employment for each establishment. Critically, we can follow the same establishments over time, which – in combination with information on the years the firm reports being active – allows us to examine pharmacy openings and closures.⁴ The

³ Rutkow et al. (2017) provides a breakdown of the provisions included within each state law.

⁴ If pharmacies do not verify their DUNS information, it is possible that they will incorrectly be classified as closed. However, for this mismeasurement to bias our estimates, it would have to be the case that pharmacies differentially stopped updating their DUNS information at the same time that the state passed a pill mill law.

NETS data have been used previously in studies similar to ours (e.g., Currie et al. 2010; Neumark and Kolko 2010; Neumark et al. 2011; Kolko 2012; Orrenius et al. 2020; Carpenter et al. 2023).

Table 2 reports the summary statistics for our main outcomes of interest over the full sample period.⁵ Column 1 reports summary statistics for the full sample, while columns 2 and 3 limit the sample to include observations from states which did and did not adopt a pill mill law during our sample period. Column 4 reports the t-statistics and corresponding p-values from tests of whether the values in columns 2 and 3 are equal. Panel A shows outcomes that are measured at the establishment level (i.e., sales and employment), and Panel B shows outcomes that are measured at the county level (i.e., openings and closures). On average, we see that establishments in states which adopted pill mill laws during our sample period had about \$3.3 million in sales per year, while establishments in states not adopting these laws had approximately \$3.8 million in sales per year. Similarly, we find that establishments in states adopting pill mill laws had approximately 1.4 fewer employees than establishments located in non-adopting states. We also find weaker evidence that states adopting pill mill laws had fewer pharmacy openings and more pharmacy closures. While these statistics do not speak to when these differences emerged in relation to the adoption of a state pill mill law, they indicate that pharmacies in states adopting such policies performed worse than those in states never adopting these laws.

3.2 Empirical Specification: Difference-in-Differences

We explore the relationship between state pill mill laws and pharmacy outcomes using the 2000-2018 NETS data and a difference-in-differences identification strategy, leveraging within-state changes in retail pharmacy performance around the adoption of a pill mill law. Prior work examining the effects of state pill mill laws operationalized this approach using a variant of the

⁵ Appendix Table 1 reports summary statistics for the covariates.

following two-way fixed effects specification (Meinhofer 2018; Mallatt 2022; Kaestner and Ziedan 2023):

$$Y_{isct} = \alpha + \beta \cdot \text{PILL MILL LAW}_{st} + Z_{sct}'\gamma + \theta_s + \tau_t + \varepsilon_{isct} \quad (1)$$

where the dependent variable, Y_{isct} , is the market outcome for establishment i , located in state s and county c , in year t (e.g., the natural log of the real value of annual sales). Meanwhile, the independent variable of interest, $\text{PILL MILL LAW}_{st}$, is an indicator variable taking on the value of one in years in which a state has an active pill mill law and zero in all other years. However, recent econometric advances have highlighted the potential pitfalls of including earlier treated states in the comparison group for later treated states (de Chaisemartin and D'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021). To overcome this issue, we adopt an imputation procedure (Borusyak et al. 2024) that first fits a two-way-fixed-effects regression using non-treated observations. It then uses the results from that model to impute the non-treated potential outcomes and aggregates them to the level of interest. This procedure assures that our coefficient of interest, β , is being identified from “clean” comparisons between treated and untreated units.

It is possible that states adopting pill mill laws may have also adopted other measures related to opioid prescribing and consumption. As such, the vector Z includes several state-level, time-varying drug policies, including whether the state had a prescription drug monitoring program (PDMP) and whether the state mandated the use of the PDMP (Buchmueller and Carey 2018; Meinhofer 2018).⁶ Given existing evidence linking changes in state marijuana policies to changes in opioid-related outcomes, the vector Z also includes indicators for whether the state had a medical

⁶ We focus on evaluating state pill mill laws, rather than simultaneously examining a broader collection of opioid restrictions, given recent advances in the difference-in-differences literature highlighting the difficulties of evaluating multiple treatments when there is variation in treatment timing (de Chaisemartin and D'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021; Borusyak et al. 2024).

marijuana law, active medical marijuana dispensaries, a recreational marijuana law, and active recreational marijuana dispensaries (Bradford et al. 2018; Powell et al. 2018; Hollingsworth et al. 2022).

To address the possibility that states may have chosen whether to adopt pill mill laws based on their local economic conditions, the vector Z also includes the state unemployment rate, the natural log of the value of initial unemployment claims, the natural log of the real value of residential building permits, and the natural log of real state product per capita. We also include the natural log of the real effective minimum wage, given the possible relationship between minimum wage changes, demand for opioids, and pharmacy employment (Dow et al. 2020). Finally, we account for demographic differences between states which did and did not adopt pill mill laws by controlling for the share of the county population comprised of Black individuals, the share of the county population comprised of Hispanic individuals, the share of the county population comprised of adults aged 65 or older, the share of the county population comprised of adults aged 18-64, and the natural log of the county population.⁷ Our baseline specification accounts for time-invariant factors related to pharmacy sales using state fixed effects, θ_s , and national shocks to the pharmacy industry using year fixed effects, τ_t . However, in alternative models we replace the state fixed effects with more granular county- and establishment-level fixed effects. Finally, we cluster standard errors at the state level (Bertrand et al. 2004).

In the presence of the covariates and fixed effects, our identifying assumption is that – in the absence of the policy change – outcomes for pharmacies in states adopting pill mill laws would have evolved similarly to outcomes for pharmacies in states not adopting pill mill laws. We assess the validity of this assumption by estimating a dynamic version of the Borusyak et al. (2024)

⁷ Accounting for the share of the population comprised of elderly adults also accounts for the fact that the introduction of Medicare Part D led to increases in the supply of opioids (Powell et al. 2020).

specification that estimates pre-period coefficients for treated states relative to non-treated states, where the earliest period is normalized to zero. Our first policy change occurred in 2005; therefore, we can estimate at most five pre-periods for establishments in all states adopting a pill mill law, with the reference group consisting of all pre-periods greater than five years before passage, as well as all never-treated observations. We also use this flexible specification to estimate dynamic post-period effects. Because the final state to adopt a pill mill law during our sample period, Wisconsin, did so in 2016, we would be able to estimate at most 3 post-periods for all states. To allow for a longer post-period, we drop Wisconsin from our event-study analyses, which allows us to estimate five post-period years.⁸ In contrast to the traditional two-way-fixed-effects event-study specification where pre- and post-period data are estimated from the same equation with a common reference period, to efficiently estimate the post-period coefficients, the Borusyak et al. (2024) specification uses the average of all the pre-period estimates as the reference group. When reporting dynamic estimates, we use separate marker symbols and colors for the pre- and post-period estimates to emphasize this asymmetry (Roth 2024).

4. RESULTS

4.1 Results: Changes in Sales and Employment

We begin by exploring the relationship between the adoption of state pill mill laws and changes in market outcomes for retail pharmacies. The relative importance of prescription opioids in pharmacy operations depends on the volume of opioids being prescribed, the reimbursement rate per pill, the value of all other sales occurring at the pharmacy, and whether prescription opioids are economic substitutes or complements for other goods sold at the pharmacy. While prior work

⁸ We show in the appendix that the patterns are robust to including Wisconsin and estimating a shorter post-period. Our static difference-in-differences estimates include observations from Wisconsin.

showed that state pill mill laws reduced opioid prescribing, our goal is to explore the net effect of these policies on pharmacy performance. The dependent variables in Table 3 are the natural log of the real value of annual sales (column 1) and the natural log of the number of employees (column 2). We find that state pill mill laws were associated with a 5.3-percent reduction in annual sales and a 2.7-percent reduction in the number of employees.⁹ Appendix Table 2 shows that the results are robust to controlling for additional time-varying spatial heterogeneity, excluding the smallest and largest establishments from the sample (Neumark et al. 2007; Barnatchez et al. 2017), and using alternative estimators.¹⁰

Figure 2 assesses the likely validity of the parallel trends assumption by plotting estimates from the dynamic event-study specification.¹¹ Because we use the Borusyak et al. (2024) estimator to address staggered treatment adoption, the light-gray circles denote the changes in outcomes prior to the adoption of a state pill mill law, and the reference group is comprised of (i) event-time observations occurring more than five years prior to adoption and (ii) the never-treated observations. Meanwhile, the dark-gray triangles denote the changes in the years following adoption of a pill mill law relative to the average of the pre-period, assuming the parallel trends

⁹ Appendix Figure 1 shows how the estimates change when we iteratively exclude each treated state. The sales and employment reductions are larger when including Florida in the sample. One explanation for this pattern is that Florida was home to a relatively large number of pill mill pain-management clinics (Kennedy-Hendricks et al. 2016; Meinhofer 2018). Florida’s pill mill law was also adopted around the time that the state conducted an audit of all pharmacies applying to open in the state, several high-volume prescribers lost their licenses, and several major distributors were shut down (Donahoe 2024).

¹⁰ Neumark et al. (2007) found that the correlation between employment levels in the NETS data and the Quarterly Census of Employment and Wages was 0.994, though the correlation was only 0.817 with the Statistics of Business because the NETS has higher coverage of smaller establishments. To further test the sensitivity of the results to the exclusion of the smallest and largest establishments, in Appendix Table 3, we report results where we exclude the bottom and top 5 percent of the distribution (i.e., where we restrict the sample to establishments with 3-39 employees) and we also exclude the bottom and top 10 percent of the distribution (i.e., we restrict the sample to establishments with 4-29 employees). We continue to find a statistically significant 5.7-6.2-percent reduction in sales and a 3.2-3.7-percent reduction in the number of employees. Relatedly, Barnatchez et al. (2017) found that the NETS data reports significantly more employment among establishments with 1-4 employees than the County Business Patterns data. Appendix Table 4 shows that the results are robust to excluding establishments with fewer than 5 employees.

¹¹ The estimates are reported in Appendix Table 5.

assumption holds.¹² There is no evidence that pharmacy market outcomes were differentially trending in treated states relative to the comparison states prior to the adoption of the laws. Indeed, the point estimates are small in magnitude and statistically insignificant. However, after states began cracking down on the overprescribing of opioids through pill mill laws, we find sizable reductions in both pharmacy sales and employment relative to the pre-period average.¹³

In a recent paper, Janssen and Zhang (2023) showed that pharmacies facing competitive pressure were more likely to engage in drug diversion to increase their revenue.¹⁴ As such, we would expect state pill mill laws to be associated with larger sales reductions for establishments located in more competitive markets. To test this possibility, we leverage the fact that the NETS data contains the GPS coordinates of each establishment. While there is relatively little evidence on how distance affects pharmacy choice (Atal et al. 2024), Medicare Part D retail pharmacy “network adequacy” standards require that 90 percent of urban beneficiaries reside within 2 miles of a network pharmacy, 90 percent of suburban beneficiaries reside within 5 miles, and 70 percent of rural beneficiaries reside within 15 miles (CMS 2006).¹⁵ As such, for each pharmacy we tabulate the number of other pharmacies located within a 5,000-meter radius (~3.1 miles), and we explore

¹² We adopted Roth’s (2022) test of our ability to reject non-parallel trends in the event study assuming 50 percent power. Because the test requires that the period prior to adoption be normalized to zero, which is not the case for our Borusyak et al. (2024) estimator, Appendix Figure 2 reports results obtained using a two-way-fixed-effects specification (Panels A and B). First, we note that the event-study estimates are qualitatively similar to our Borusyak et al. (2024) event studies, which is consistent with the fact that our sample includes many “never treated” states. The likelihood ratios are 5.86 and 3.73, indicating that our results are not driven by erroneously assuming parallel trends relative to the hypothesized trend. Second, we show that our post-period estimates are outside of the values expected from the hypothesized worst-case-scenario pre-trend. Finally, we used Roth and Rambachan (2023) to examine the post-period estimates after imposing parallel trends (i.e., $M = 0$). We continue to find evidence of reductions in pharmacy sales and employment (Panels C and D).

¹³ The event-study estimates exclude Wisconsin to allow for a longer post-period with a balanced state-year event window. We show in Appendix Figure 3 that the results are unchanged when including Wisconsin and estimating a shorter post-period.

¹⁴ Janssen and Zhang (2023) estimated the effect of competition on opioid dispensing using nine different radii (see Figure 6 on page 26). While the authors found large increases when pharmacies faced an additional competitor within one or two miles, the estimates largely converged when the radius is increased beyond four miles.

¹⁵ Researchers have examined the importance of the number of competitors within a given radius (Janssen and Zhang 2023), the distance between a pharmacy and its five closest competitors (Chen 2019), and patients’ travel times to preferred and in-network pharmacies (Starc and Swanson 2021).

the robustness to alternative radii. We classify establishments in the bottom quartile of this distribution (i.e., those with at most 3 nearby establishments) as being in a “low-competition area,” those in the middle 50 percent of the distribution (i.e., those with 4 to 19 nearby establishments) as being in a “moderate-competition area,” and those in the top quartile of the distribution (i.e., those with 20 or more nearby establishments) as being in a “high-competition area.”

In Table 4 we provide evidence that state pill mill laws resulted in larger reductions in sales and the number of employees for pharmacies facing stronger competitive pressure. Column 1 reprints our baseline results showing a 5.3-percent reduction in sales and a 2.7-percent reduction in the number of employees when using the full sample. Yet column 2 shows that pharmacies in low-competition areas only experienced a 1.3-percent reduction in sales and no change in the number of employees, though neither estimate is statistically distinguishable from zero. These results suggest that state pill mill laws had at most a modest effect on pharmacies in low-competition areas. In contrast, column 3 shows that state pill mill laws were associated with a 5.9-percent reduction in sales and a 2.7-percent reduction in the number of employees in areas with a moderate level of competition. Finally, column 4 shows that pharmacies in high-competition areas experienced a 7.1-percent reduction in sales and a 3.5-percent reduction in the number of employees.¹⁶

While the evidence indicates that pharmacies located within states adopting pill mill laws experienced a reduction in sales and employment, these policies may have benefited nearby pharmacies in states not adopting a pill mill law (Hodges and Shakya 2025). Indeed, prior evidence

¹⁶ Appendix Table 6 shows similar results when defining competition based on the total sales volume from other pharmacies within a 5,000-meter radius. Appendix Table 7 documents a similar pattern when increasing the radius to 10,000 meters (~6.2 miles). Likewise, Appendix Table 8 shows that state pill mill laws resulted in larger reductions in sales and employment for pharmacies in high-competition areas when we decrease the radius to only 1,000 meters (~0.62 miles).

indicates that individuals will cross state borders to purchase products that are more heavily regulated within their own states, including firearms (Knight 2013), alcohol (Lovenheim and Slemrod 2010), marijuana (Hansen et al. 2020), and opioids (Deiana and Giua 2021; Shakya and Ruseski 2023). To test this possibility, in Table 5 we limit the sample to pharmacies in states that never themselves adopted a pill mill law. Our independent variable of interest is an indicator for whether the pharmacy was located in a border county and the bordering state had adopted a pill mill law. Column 1 shows that state pill mill laws were associated with a 5.8-percent increase in annual sales for pharmacies located across the border in states not adopting a pill mill law. Similarly, column 2 shows an 8.7-percent increase in the number of pharmacy employees. Together, these results suggest that state pill mill laws encouraged individuals to travel across state lines for their prescription opioids.¹⁷

One benefit of the NETS data is that we observe the same establishments over time, so in Table 6 we include increasingly granular levels of geographic fixed effects. Our results remain largely unchanged after including county fixed effects (columns 3 and 4). Interestingly, though, the direction of the effect changes sign after including establishment fixed effects (columns 5 and 6). Rather than reducing sales and employment, these models indicate that state pill mill laws were associated with a statistically insignificant 1.2-percent increase in sales and a statistically significant 1.4-percent increase in the number of employees, conditional on the establishment remaining open. As such, Table 6 suggests that the reductions in sales and employment were driven by extensive-margin adjustments in whether establishments remained open, while surviving establishments appear to have modestly benefited from these laws.

¹⁷ Appendix Table 9 shows that the baseline results are robust to excluding border counties from the sample.

4.2 Results: Changes in Pharmacy Openings and Closures

In the prior section, we showed that state pill mill laws were associated with reductions in pharmacy sales and employment, and we provided suggestive evidence that these changes were driven by a reduction in the number of establishments. Using our NETS data, we now formally test whether these laws were associated with changes in the size of the retail pharmacy market by examining changes in establishment openings and closures. In any given year, the median county had zero pharmacy openings and zero pharmacy closures, with 75 percent of counties experiencing at most one opening and at most one closure. As such, the relevant margin of interest for most counties would be moving from experiencing no change in a given year to experiencing a single opening or closure. While Table 7 shows no evidence that state pill mill laws were associated with an increase in the likelihood that counties experienced a pharmacy opening (column 1) we find a 3.7-percentage-point increase in the likelihood of a pharmacy closure (column 2) – a 12-percent increase relative to the mean.¹⁸

We report event-study results in Figure 3. The light-gray circles plot estimates examining whether the outcomes were differentially trending prior to the adoption of a pill mill law. The dark-gray triangles plot estimates examining changes in the post-period relative to the average of the pre-period (Borusyak et al. 2024). We do not find evidence that the likelihood of experiencing a

¹⁸ We also explored whether state pill mill laws were associated with intensive-margin changes in the number of openings and closures. Because most counties do not experience any opening or closure in a given year, a common approach is to first add one and then take the natural log of these values. However, recent evidence has drawn attention to the difficulty in interpreting estimates obtained via this transformation (Mullahy and Norton 2024; Chen and Roth 2024). Instead, in Appendix Table 10, we report results from the Borusyak et al. (2024) specification where the dependent variables are the natural log of the number of openings among counties experiencing a pharmacy opening (column 1) and the natural log of the number of closures among counties experiencing a closure (column 2) and the sample is limited to counties with any openings or closures, respectively. Additionally, we also report results where the dependent variables are the number of openings (column 3) and the number of closures (column 4) and the estimates are obtained from Poisson regression by pseudo-maximum likelihood through a stacked difference-in-differences framework (Cengiz et al. 2019; Correia et al. 2020). We continue to find that state pill mill laws were associated with an increase in pharmacy closures without any change in pharmacy openings.

pharmacy opening (Panel A) or closure (Panel B) was trending in the pre-period. Nor do we find any systematic evidence that state pill mill laws were associated with changes in the likelihood of counties experiencing a pharmacy opening in the post-period. However, following the adoption of a state pill mill law, we find an increase in the likelihood of experiencing a pharmacy closure.^{19,20} We show in Appendix Figure 6 that these patterns are robust to iteratively excluding each of the treated states. Likewise, we show in Appendix Table 11 that the results are robust to alternative controls for spatial heterogeneity, sample restrictions, and difference-in-differences estimators.²¹ Prior work has shown that rural counties and those with higher poverty rates have less access to retail pharmacies (Klepser et al. 2011; Catalano et al. 2024; Kwan 2024; Wittenauer et al. 2024). We show in Appendix Table 13 that state pill mill laws were associated with a 4.0-percentage-point (21.6 percent) increase in the likelihood that non-metropolitan counties experienced a pharmacy closure compared to a 2.8-percentage-point (5.3 percent) increase in metropolitan counties.

There is evidence that independent pharmacies were more likely than chain pharmacies to dispense excessive quantities of prescription opioids. For example, a bipartisan congressional investigation found that a local pharmacy in Oceana, West Virginia received 600 times as many oxycodone pills as the Rite Aid drugstore eight blocks away (Committee on Energy and Commerce of the 115th Congress 2018). Systematically exploring this phenomenon using data from the 2006-2012 Automation of Reports and Consolidated Orders System (ARCOS) maintained by the U.S.

¹⁹ Appendix Figure 4 shows that the results are robust to including observations from Wisconsin and estimating a shorter post-period. As a reminder, the static difference-in-differences estimate includes observations from Wisconsin.

²⁰ In Appendix Figure 5 we report the Roth (2022) test of our ability to reject parallel trends using the two-way-fixed-effects estimator previously described in footnote 12. In contrast to our sales and employment results, we find that our event-study estimates showing a post-period increase in pharmacy closures are in line with the worst-case scenario trend assuming 50-percent power.

²¹ We also explored whether there were differential changes in openings and closures for low, moderate, and high competition areas. The results are inconclusive but reported in Appendix Table 12 for completeness.

Drug Enforcement Agency, Janssen and Zhang (2023) showed that (i) independent pharmacies dispensed approximately 39 percent more opioids and 61 percent more OxyContin than chain pharmacies within the same zip code, and (ii) nearly 40 percent of this difference was due to drug diversion. Given this finding, we would expect state pill mill laws to more adversely affect the sales of independent pharmacies.

The NETS data allow us to distinguish between standalone establishments and those connected to other establishments (i.e., headquarters and branches).²² In Table 8, we leverage this feature of the data by exploring whether state pill mill laws were associated with differential changes in the number of openings and closures among standalone and non-standalone pharmacies. Consistent with prior evidence that standalone pharmacies are more likely to engage in drug diversion, column 2 shows that state pill mill laws were associated with a 4.2-percent increase in the number of standalone pharmacy closures. In contrast, column 4 indicates that the relationship for non-standalone pharmacies was over 85-percent smaller in magnitude, opposite signed, and statistically insignificant.²³ Collectively, these results suggest that state pill mill laws influenced the retail pharmacy market by increasing the number of standalone pharmacy closures.²⁴ However, we do not find any evidence in Appendix Table 17 that state pill mill laws were associated with changes in the number of openings or closures in nearby border counties, overall or by standalone status.²⁵

²² We also explored heterogeneity in sales and employment by standalone status. While the results were generally inconclusive, we report them in Appendix Table 14 for completeness.

²³ Event-study estimates, shown in Appendix Figure 7, confirm that the increase in pharmacy closures was limited to standalone pharmacies in the post-period. Meanwhile, Appendix Table 15 shows that these patterns are robust to replacing the state fixed effects with county fixed effects (Panel A), replacing our dependent variable with the inverse hyperbolic sine of the number of openings and closures (Panel B), and replacing our dependent variable with the number of openings and closures per 100,000 (Panel C).

²⁴ Appendix Table 16 reports inconclusive results by standalone status and competition area, likely due to insufficient statistical power for such small subsamples of the data.

²⁵ Consistent with our finding that state pill mill laws most adversely affected pharmacies located in areas with more nearby establishments, Appendix Table 18 shows that these policies were unrelated to changes in the likelihood that

5. CONCLUSION

This paper provides new evidence on how public policies that limit the sale of goods that pose a risk to public health affect the market outcomes of establishments selling those goods. Over the last two decades, federal and state lawmakers have adopted a variety of policies aimed at reducing prescription opioid abuse and mortality (Alpert et al. 2018; Buchmueller and Carey 2018; Ruhm 2019; Alpert et al. 2024). One group of policies, known as pill mill laws, sought to reduce excessive opioid prescribing by closing the most egregious pain management clinics and reducing the volume of prescribing at the remaining facilities (Mallatt 2017; Maclean et al. 2021; Ziedan and Kaestner 2024). In this paper, we leverage the staggered adoption of these laws by 12 states between 2005 and 2016 to study how firms are affected by government policies limiting product sales.

Using establishment-level data from the 2000-2018 National Establishment Time-Series (NETS) and a difference-in-differences identification strategy, we show that state pill mill laws, which were intended to reduce excessive opioid prescribing by pain management clinics, resulted in a 5.3-percent reduction in pharmacy sales and a 2.7-percent reduction in the number of pharmacy employees. These reductions were most pronounced for pharmacies in more competitive areas, which is consistent with evidence that pharmacies may engage in drug diversion to offset revenue losses (Janssen and Zhang 2023). We then show that these reductions were driven by increases in pharmacy closures, particularly among standalone establishments that are more likely than chain pharmacies to engage in drug diversion (Committee on Energy and Commerce of the 115th Congress 2018). We also find evidence that surviving establishments experienced modest

a county did not have a pharmacy (column 1), the number of pharmacies per 100,000 people (column 2), or the total number of pharmacies (columns 3 and 4). These results suggest that state pill mill laws likely do not explain a rise of pharmacy deserts where individuals do not have access to any retail pharmacies (Pednekar and Peterson 2018; Guadamuz et al. 2021).

improvements in market outcomes. These findings highlight a previously unknown role of policies limiting access to prescription opioids in explaining increases in independent pharmacy closures and industrywide consolidation that occurred throughout our sample period (Guadamuz et al. 2020).

While our results indicate that state pill mill laws reduced pharmacy sales, it is worth emphasizing that this effect was driven by establishments most likely to engage in drug diversion (i.e., standalone pharmacies and those with more nearby competitors). Moreover, while we also found evidence of a reduction in pharmacy employment, prior work has shown that state pill mill laws lead to labor market improvements (Kaestner and Ziedan 2023), and numerous other papers have found that reducing opioid use improves population-level labor force participation, employment, and firm outcomes (Aliprantis et al. 2023; Beheshti 2023; Kim et al. 2024; Langford and Feldman 2024).²⁶ In terms of patient access, we found no evidence of a relationship between state pill mill laws and the likelihood that a county had no pharmacy, the number of pharmacies per capita, or the total number of pharmacies. Overall, these patterns are consistent with pill mill laws adversely affecting pharmacies filling inappropriate opioid prescriptions without meaningfully altering patient access to retail pharmacies.

This study is subject to some limitations. For one, we are unable to disentangle the extent to which the market changes are due to state pill mill laws reducing the number of opioid prescriptions filled for illicit purposes versus medically justified reasons. However, prior evidence indicates that independent pharmacies dispense substantially more opioids than chain pharmacies

²⁶ Kaestner and Ziedan (2023) found evidence that state pill mill laws resulted in a greater reduction in prescription-opioid availability than PDMPs. Using a difference-in-differences identification strategy and data on shipments of prescription opioids from the DEA's Automated Reports and Consolidated Ordering System (ARCOS), they found that state pill mill laws were associated with a 15-40-percent reduction in the volume of prescription opioids, compared to a more modest 5-20-percent reduction attributable to PDMPs.

due to drug diversion (Janssen and Zhang 2023), and the increases in pharmacy closures that we detect are concentrated among these standalone establishments. Additionally, we are unable to identify which specific aspects of state pill mill laws, or their subsequent enforcement, resulted in changes in retail pharmacy outcomes. Finally, we do not know the extent to which the reduction in revenue for retail pharmacies was due to reductions in sales of prescription opioids, and to what extent reductions in the volume of prescription-opioids purchases by consumers were replaced with purchases of economic substitutes such as heroin and fentanyl. Such substitution is a potential negative effect of pill mill laws as prior research has documented the substitutability of prescription and illicit opioids, which resulted in increases in heroin and fentanyl overdose deaths, transmission of blood-borne diseases from increases in intravenous drug use, crime, and other negative externalities (e.g., Alpert et al. 2018; Meinhofer 2018; Beheshti 2019; Evans et al. 2019; Balestra et al. 2021; Deiana and Giua 2021; Mallatt 2022). Despite these limitations, this study offers important new evidence on how firms are affected by government efforts to limit the supply of their products.

6. REFERENCES

- About, Rahi, Rosalie Liccardo Pacula, and David Powell (2019). “Association of State Naloxone Access Laws with Naloxone Distribution and Opioid Overdose Deaths,” *JAMA Internal Medicine*, 179(6): 805-811.
- Act 128, 2013 Biennium, 2013 Regular Session (Georgia 2013). Accessed at https://www.legis.ga.gov/api/document/docs/default-source/general-statutes/13sumdoc.pdf?sfvrsn=2df8a369_2 (January 8, 2025).
- Act 265, 2015 Biennium, 2015 Regular Session (Wisconsin 2015). Accessed at <https://docs.legis.wisconsin.gov/2015/related/acts/265.pdf> (January 8, 2025).
- Adda, Jérôme, Samuel Berlinski, and Stephen Machin (2012). “Market Regulation and Firm Performance: The Case of Smoking Bans in the United Kingdom,” *Journal of Law and Economics*, 55(2): 365-391.
- Aliprantis, Dionissi, Kyle Fee, and Mark E. Schweitzer (2023). “Opioids and the Labor Market,” *Labour Economics*, 85: 102446.
- Alpert, Abby E., David Powell, and Rosalie Liccardo Pacula (2018). “Supply-Side Drug Policy in the Presence of Substitutes: Evidence from the Introduction of Abuse-Deterrent Opioids,” *American Economic Journal: Economic Policy* 10(4): 1-35.
- Alpert, Abby, Sarah Dykstra, and Mireille Jacobson (2024). “Hassle Costs Versus Information: How Do Prescription Drug Monitoring Programs Reduce Opioid Prescribing?” *American Economic Journal: Economic Policy*, 16(1): 87-123.
- Atal, Juan Pablo, José Ignacio Cuesta, Felipe González, and Cristóbal Otero (2024). “The Economics of the Public Option: Evidence from Local Pharmaceutical Markets,” *American Economic Review*, 114(3): 615-644.
- Balestra, Simone, Helge Liebert, Nicole Maestas, and Tisamarie B. Sherry (2021). “Behavioral Responses to Supply-Side Drug Policy During the Opioid Epidemic,” *NBER Working Paper No. 29596*.
- Barnatchez, Keith, Leland Dod Crane, and Ryan Decker (2017). “An Assessment of the National Establishment Time Series (NETS) Database,” Finance and Economics Discussion Series 2017-110. Washington: Board of Governors of the Federal Reserve System, Accessed at: <https://doi.org/10.17016/FEDS.2017.110>.
- Bedard, Kelly and Peter Kuhn (2015). “Micro-Marketing Healthier Choices: Effects of Personalized Ordering Suggestions on Restaurant Purchases,” *Journal of Health Economics*, 39: 106-122.

- Beheshti, David (2019). “Adverse Health Effects of Abuse-Deterrent Opioids: Evidence from the Reformulation of OxyContin,” *Health Economics*, 28(12): 1449-1461.
- Beheshti, David (2023). “The Impact of Opioids on the Labor Market: Evidence from Drug Rescheduling,” *Journal of Human Resources*, 58(6): 2001-2041. Accessed at: <https://doi.org/10.3368/jhr.0320-10762R1>.
- Berenbrok, Lucas A., Gabriel Nico, Kim C. Coley, and Immaculada Hernandez (2020). “Evaluation of Frequency of Encounters with Primary Care Physicians vs. Visits to Community Pharmacies among Medicare Beneficiaries,” *JAMA Network Open*, 3(7): e209132.
- Bertrand, Marianne, Esther Duflo, and Sendil Mullainathan (2004). “How Much Should We Trust Difference-in-Differences Estimators?” *The Quarterly Journal of Economics*, 119(1): 249-275.
- Borusyak, Kirill, Xavier Jaravel, and Jann Speiss (2024). “Revisiting Event Study Designs: Robust and Efficient Estimation,” *The Review of Economic Studies*, 91(6): 3253-3285
- Bradford, Ashley C., W. David Bradford, Amanda Abraham, and Grace Bagwell Adams (2018). “Association Between US State Medical Cannabis Laws and Opioid Prescribing in the Medicare Part D Population,” *JAMA Internal Medicine*, 178(5): 667-672.
- Brighthaupt, Sherri-Chanelle, Elizabeth M. Stone, Lainie Rutkow, and Emma E. McGinty (2019). “Effect of Pill Mill Laws on Opioid Overdose Deaths in Ohio & Tennessee: A Mixed-Methods Case Study,” *Preventive Medicine*, 126: 105736.
- Buchmueller, Thomas C. and Colleen Carey (2018). “The Effect of Prescription Drug Monitoring Programs on Opioid Utilization in Medicare,” *American Economic Journal: Economic Policy*, 10(1): 77-112.
- Buchmueller, Thomas C., Colleen M. Carey, and Giacomo Meille (2020). “How Well Do Doctors Know Their Patients? Evidence from a Mandatory Access Prescription Drug Monitoring Program,” *Health Economics*, 29(9): 957-974.
- Butters, R. Andrew, Daniel W. Sacks, and Boyoung Seo (2022). “How Do National Firms Respond to Local Cost Shocks?” *American Economic Review*, 112(5): 1737-1772.
- Callaway, Brantly, and Pedro H.C. Sant’Anna (2021). “Difference-in-Differences with Multiple Time Periods,” *Journal of Econometrics*, 225(2): 200-230.
- Carpenter, Christopher S., Brandyn F. Churchill, and Michelle M. Marcus (2023). “Bad Lighting: Effects of Youth Indoor Tanning Prohibitions,” *Journal of Health Economics*, 88: 102738.
- Carpenter, Christopher S. and Carlos Dobkin (2009). “The Effect of Alcohol Consumption on Mortality: Regression Discontinuity Evidence from the Minimum Drinking Age,” *American Economic Journal: Applied Economics*, 1(1): 164-182.

- Carpenter, Christopher S. and Carlos Dobkin (2011). “The Minimum Legal Drinking Age and Public Health,” *Journal of Economic Perspectives*, 25(2): 133-156.
- Carpenter, Christopher S. and D. Sebastian Tello-Trillo (2015). “Do Cheeseburger Bills Work? Effects of Tort Reform for Fast Food,” *Journal of Law and Economics*, 58(4), 805-827.
- Catalano, Giovanni, Muhammad Muntazir Mehdi Khan, Odysseas P. Chatzipanagiotou, and Timothy M. Pawlik (2024). “Pharmacy Accessibility and Social Vulnerability,” *JAMA Network Open*, 7(8): e2429755.
- Cawley, John, David Frisvold, Anna Hill, and David Jones (2019). “The Impact of the Philadelphia Beverage Tax on Purchases and Consumption by Adults and Children,” *Journal of Health Economics*, 67: 10225.
- Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer (2019). “The Effect of Minimum Wages on Low-Wage Jobs,” *Quarterly Journal of Economics*, 134(3): 1405-1454.
- Centers for Disease Control and Prevention (2025). “Understanding the Opioid Overdose Epidemic,” Accessed at: <https://www.cdc.gov/overdose-prevention/about/understanding-the-opioid-overdose-epidemic.html> (January 2, 2025).
- Centers for Medicare and Medicaid Services (2006). “Memorandum Re: Pharmacy Network Adequacy,” Accessed at: https://www.cms.gov/medicare/prescription-drug-coverage/prescriptiondrugcovcontra/downloads/memopharmacynetworkadequacy_071006.pdf (January 2, 2025).
- Chen, Jiafeng and Jonathan Roth (2024). “Logs with Zeros? Some Problems and Solutions,” *Quarterly Journal of Economics*, 139(2): 891-936.
- Chen, Jihui (2019). “The Effects of Competition on Prescription Payments in Retail Pharmacy Markets,” *Southern Economic Journal*, 85(3): 865-898.
- Clark, Andrew E. and Carine Milcent (2011). “Public Employment and Political Pressure: The Case of French Hospitals,” *Journal of Health Economics*, 30(5): 1103-1112.
- Committee on Energy and Commerce of the House of Representatives for the 115th Congress (2018). “Combating the Opioid Epidemic: Examining Concerns about Distribution and Diversion,” Accessed at: <https://www.govinfo.gov/content/pkg/CHRG-115hhrg31601/html/CHRG-115hhrg31601.htm>.
- Conlon, Christopher and Nirupama L. Rao (2023). “The Cost of Curbing Externalities with Market Power: Alcohol Regulations and Tax Alternatives,” *NBER Working Paper No. 30896*.

- Cornelsen, Laura and Charles Normand (2012). “Impact of the Smoking Ban on the Volume of Bar Sales in Ireland – Evidence from Time Series Analysis,” *Health Economics*, 21(5): 551-561.
- Correia, Sergio, Paulo Guimarães, and Thomas Zylkin (2020). “Fast Poisson Estimation with High-Dimensional Fixed Effects,” *The Stata Journal*, 20: 95-115.
- Cotti, Chad, Charles Courtemanche, Joanna Catherine Maclean, Erik Nesson, Michael F. Pesko, and Nathan W. Tefft (2022). “The Effects of E-Cigarette Taxes on E-Cigarette Prices and Tobacco Product Sales: Evidence from Retail Panel Data,” *Journal of Health Economics*, 86: 102676.
- Currie, Janet, Stefano DellaVigna, Enrico Moretti, and Vikram Pathania (2010). “The Effect of Fast Food Restaurants on Obesity and Weight Gain,” *American Economic Journal: Economic Policy*, 2: 32-63.
- Dalton, Christina Marsh and W. David Bradford (2019). “Better Together: Coexistence of For-Profit and Nonprofit Firms with an Application to the U.S. Hospice Industry,” *Journal of Health Economics*, 63: 1-18.
- de Chaisemartin, Clément and Xavier D’Haultfoeuille (2020). “Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects,” *American Economic Review*, 110(9): 2964-2996.
- DeCicca, Philip, Donald Kenkel, Feng Liu, and Jason Somerville (2021). “Quantifying Brand Loyalty: Evidence from the Cigarette Market,” *Journal of Health Economics*, 79: 102512.
- DeCicca, Philip, Donald Kenkel, and Michael F. Lovenheim (2022). “The Economics of Tobacco Regulation: A Comprehensive Review,” *Journal of Economic Literature*, 60(3): 883-970.
- Dee, Thomas S., David Grabowski, and Michael A. Morrissey (2005). “Graduated Driver Licensing and Teen Traffic Fatalities,” *Journal of Health Economics*, 24(3): 571-589.
- de Frutos, Maria-Angeles, Carmine Ornaghi, and George Siotis (2013). “Competition in the Pharmaceutical Industry: How Do Quality Differences Shape Advertising Strategies?” *Journal of Health Economics*, 32(1): 268-285.
- Deiana, Claudio and Ludovica Giua (2021). “The Intended and Unintended Effects of Opioid Policies on Prescription Opioids and Crime,” *B.E. Journal of Economic Analysis & Policy*, 21(2): 751-792.
- Depew, Briggs and Isaac Swensen (2022). “The Effect of Concealed-Carry and Handgun Restrictions on Gun-Related Deaths: Evidence from the Sullivan Act of 1911,” *Economic Journal*, 132(646): 2118-2140.

- Dickson, Alex, Markus Gehrsitz, and Jonathan Kemp (2025). “Does a Spoonful of Sugar Levy Help the Calories Go Down? An Analysis of the UK Soft Drink Industry Levy,” *Review of Economics and Statistics*. Accessed at: https://doi.org/10.1162/rest_a_01345.
- Dobkin, Carlos, Nancy Nicosia, and Matthew Weinberg (2014). “Are Supply-Side Drug Control Efforts Effective? Evaluating OTC Regulations Targeting Methamphetamine Precursors,” *Journal of Public Economics*, 120: 48-61.
- Donahoe, J. Travis (2024). “Supplier Enforcement and the Opioid Crisis,” *Working Paper*, Accessed at: <https://static1.squarespace.com/static/632f3a2e33b0e9116c23e95a/t/660c28dda996cc5781ba1336/1712072926680/Supplier+Enforcement.pdf> (October 23rd, 2024).
- Dow, William H., Anna Godøy, Christopher Lowenstein, and Michael Reich (2020). “Can Labor Market Policies Reduce Deaths of Despair?” *Journal of Health Economics*, 74: 102372.
- Evans, William N., Ethan M.J. Lieber, and Patrick Power (2019). “How the Reformulation of OxyContin Ignited the Heroin Epidemic,” *Review of Economics and Statistics*, 10(1): 1-15.
- Goodman-Bacon, Andrew (2021). “Difference-in-Differences with Variation in Treatment Timing,” *Journal of Econometrics*, 225(2): 254-277.
- Guadamuz, Jenny S., G. Caleb Alexander, Shannon N. Zenk, and Dima M. Qato (2020). “Assessment of Pharmacy Closures in the United States from 2009 Through 2015,” *JAMA Internal Medicine*, 180(1): 157-160.
- Guadamuz, Jenny S., Jocelyn Wilder, Morgane C. Mouslim, Shannon N. Zenk, Caleb Alexander, and Dima Mazen Qato (2021). “Fewer Pharmacies in Black and Hispanic/Latino Neighborhoods Compared with White or Diverse Neighborhoods, 2007-2015,” *Health Affairs*, 40(5): 802-811.
- Hansen, Benjamin, Joseph J. Sabia, Drew McNichols, and Calvin Bryan (2023). “Do Tobacco 21 Laws Work?” *Journal of Health Economics*, 92: 102818.
- Hansen, Benjamin, Keaton Miller, and Caroline Weber (2020). “Federalism, Partial Prohibition, and Cross-Border Sales: Evidence from Recreational Marijuana,” *Journal of Public Economics*, 187: 104159.
- Hodges, Collin D. and Shishir Shakya (2025). “Prescription Opioid Spillovers: Retail Pharmacy Level Analysis,” *Journal of Substance Use and Addiction Treatment*, 175: 209725.
- Hollingsworth, Alex, Coady Wing, and Ashley Bradford (2022). “Comparative Effects of Medical and Recreational Marijuana Laws on Drug Use Among Adults and Adolescents,” *Journal of Law and Economics*, 65(3): 515-554.

- IRS (2022). “Gynecologist and Pharmacist Plead Guilty to Operating Massive “Pill Mill” Network,” Accessed at: <https://www.irs.gov/compliance/criminal-investigation/gynecologist-and-pharmacist-plead-guilty-to-operating-massive-pill-mill-network> (August 5, 2025).
- Janssen, Aljoscha and Xuan Zhang (2023). “Retail Pharmacies and Drug Diversion During the Opioid Epidemic,” *American Economic Review*, 113(1): 1-33.
- Kaestner, Robert, and Engy Ziedan (2023). “Effects of Prescription Opioids on Employment, Earnings, Marriage, Disability, and Mortality: Evidence from State Opioid Control Policies,” *Labour Economics*, 82: 102358.
- Kennedy-Hendricks, Alene, Matthew Richey, Emma E. McGinty, Elizabeth A. Stuart, Colleen L. Barry, and Daniel W. Webster (2016). “Opioid Overdose Deaths and Florida’s Crackdown on Pill Mills,” *American Journal of Public Health*, 106(2): 291-297.
- Kim, Bokyung (2021). “Must-Access Prescription Drug Monitoring Programs and the Opioid Overdose Epidemic: The Unintended Consequences,” *Journal of Health Economics*, 75: 102408.
- Kim, Bokyung, Minseog Kim, and Geunyoung Park (2024). “The Opioid Crisis and Firm Skill Demand: Evidence from Job Posting Data,” *Working Paper*, Accessed at: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4825126.
- Klepser, Donald G., Liyan Xu, Fred Ullrich, and Keith J. Mueller (2011). “Trends in Community Pharmacy Counts and Closures Before and After the Implementation of Medicare Part D,” *Journal of Rural Health*, 27(2): 168-175.
- Knight, Brian (2013). “State Gun Policy and Cross-State Externalities: Evidence from Crime Gun Tracing,” *American Economic Journal: Economic Policy*, 5(4): 200-229.
- Kolko, Jed (2012). “Broadband and Local Growth,” *Journal of Urban Economics*, 71(1): 100-113.
- Kwan, Noelle (2024). “The Impact of Pharmacy Deserts,” *U.S. Pharmacist*, 49(4): 32-36.
- Langford, W. Scott and Maryann P. Feldman (2024). “We’re Not in Dreamland Anymore: The Consequences of Community Opioid Use on Local Industrial Composition,” *Journal of Regional Science*, 64: 1811-1831. Accessed at: <https://doi.org/10.1111/jors.12727>.
- Lawler, Emily C. and Meghan M. Skira (2022). “Information Shocks and Pharmaceutical Firms’ Marketing Efforts: Evidence from the Chantix Black Box Warning Removal,” *Journal of Health Economics*, 81: 102557.
- Lovenheim, Michael F. and Joel Slemrod (2010). “The Fatal Toll of Driving to Drink: The Effect of Minimum Legal Drinking Age Evasion on Traffic Fatalities,” *Journal of Health Economics*, 29(1): 62-77.

- Maclean, Johanna Catherine, Justine Mallatt, Christopher J. Ruhm, and Kosali Simon (2021). “Economic Studies on the Opioid Crisis: Costs, Causes, and Policy Responses,” *Oxford Research Encyclopedia of Economics and Finance*.
- Mallatt, Justine (2017). “The Effect of Supply-Side Opioid Legislation on Opioid-Related Business Establishments,” *Working Paper*, Accessed at: https://drive.google.com/file/d/1Odj8isoMU-F7uWC_rJkKjOmUv8jjiUWY/view
- Mallatt, Justine (2022). “Policy-Induced Substitution to Illicit Drugs and Implications for Law Enforcement Activity,” *American Journal of Health Economics*, 8(1): 30-64.
- Manolakis, Patti G. and Jann B. Skelton (2010). “Pharmacists’ Contributions to Primary Care in the United States Collaborating to Address Unmet Patient Care Needs: The Emerging Role for Pharmacists to Address the Shortage of Primary Care Providers,” *American Journal of Pharmaceutical Education*, 74(10): S7.
- Meinhofer, Angélica (2018). “Prescription Drug Monitoring Programs: The Role of Asymmetric Information on Drug Availability and Abuse,” *American Journal of Health Economics*, 4(4): 504-526.
- Mullahy, John and Edward C. Norton (2024). “Why Transform Y? The Pitfalls of Transformed Regressions with a Mass at Zero” *Oxford Bulletin of Economics and Statistics*, 86(2):417-447.
- National Center for Health Statistics (2023). “Drug Overdose Deaths,” Accessed at: <https://www.cdc.gov/nchs/hus/topics/drug-overdose-deaths.htm> (October 3, 2024).
- Neumark, David and Jed Kolko (2010). “Do Enterprise Zones Create Jobs? Evidence from California’s Enterprise Zone Program,” *Journal of Urban Economics*, 68(1): 1-19.
- Neumark, David and Bogdan Savych (2023). “Effects of Opioid-Related Policies on Opioid Utilization, Nature of Medical Care, and Duration of Disability,” *American Journal of Health Economics*, 9(3): 331-373.
- Neumark, David, Brandon Wall, and Junfu Zhang (2011). “Do Small Businesses Create More Jobs? New Evidence for the United States from the National Establishment Time Series,” *Review of Economics and Statistics*, 93(1): 16-29.
- Neumark, David, Junfu Zhang, and Brandon Wall (2007). “Employment Dynamics and Business Relocation: New Evidence from the National Establishment Time Series,” *Research in Labor Economics*, 26: 39-83.
- Nguyen, Thuy D., W. David Bradford, and Kosali I. Simon (2019). “How Do Opioid Prescribing Restrictions Affect Pharmaceutical Promotion? Lessons from the Mandatory Access Prescription Drug Monitoring Programs,” *NBER Working Paper* No. 26356.

- Orrenius, Pia M., Madeline Zavodny, and Alexander T. Abraham (2020). “The Effect of Immigration on Business Dynamics and Employment,” *FRB of Dallas Working Paper* No. 2004.
- Pednekar, Priti and Andrew Peterson (2018). “Mapping Pharmacy Deserts and Determining Accessibility to Community Pharmacy Services for Elderly Enrolled in a State Pharmaceutical Assistance Program,” *PLOS One*, 13(6): e0198173.
- Powell, David, Rosalie Liccardo Pacula, and Mireille Jacobson (2018). “Do Medical Marijuana Laws Reduce Addictions and Deaths Related to Pain Killers?” *Journal of Health Economics*, 58: 29-42.
- Powell, David, Rosalie Liccardo Pacula, and Erin Taylor (2020). “How Increasing Medical Access to Opioids Contributes to the Opioid Epidemic: Evidence from Medicare Part D,” *Journal of Health Economics*, 71: 102286.
- Public Act 257, 2013 Biennium, 2013 Regular Session (Alabama 2013). Accessed at <https://alison.legislature.state.al.us/summaries-2013-general-acts#act2013257> (January 8, 2025).
- Raja, Chandni (2023). “How Do Hospitals Respond to Input Regulation? Evidence from the California Nurse Staffing Mandate,” *Journal of Health Economics*, 92: 102826.
- Rambachan, Ashesh and Jonathan Roth (2023). “A More Credible Approach to Parallel Trends,” *Review of Economic Studies*, 90(5): 2555-2591.
- Roth (2022). “Pretest with Caution: Event-Study Estimates After Testing for Parallel Trends,” *American Economic Review: Insights*, 4(3): 305-322.
- Roth, Jonathan (2024). “Interpreting Event Studies from Recent Difference-in-Differences Methods,” *Working Paper*, accessed at <https://www.jonathandroth.com/assets/files/HetEventStudies.pdf> (September 11, 2025).
- Ruhm, Christopher J. (2019). “Drivers of the Fatal Drug Epidemic,” *Journal of Health Economics*, 64: 25-42.
- Rutkow, Lainie, Hsien-Yen Chang, Matthew Daubresse, Daniel W. Webster, Elizabeth A. Stuart, and G. Caleb Alexander (2015). “Effect of Florida’s Prescription Drug Monitoring Program and Pill Mill Laws on Opioid Prescribing and Use,” *JAMA Internal Medicine*, 175(10): 1642-1649.
- Rutkow, Lainie, Jon S. Vernick, and G. Caleb Alexander (2017). “More States Should Regulate Pain Management Clinics to Promote Public Health,” *American Journal of Public Health*, 107(2): 240-243.

- Sacks, Daniel W., Alex Hollingsworth, Thuy Nguyen, and Kosali Simon (2021). “Can Policy Affect Initiation of Addictive Substance Use? Evidence from Opioid Prescribing,” *Journal of Health Economics*, 76: 102397.
- Senate Enrolled Act 246, 2013 Biennium, 2013 Regular Session (Indiana 2013). Accessed at <https://archive.iga.in.gov/2013/bills/SE/SE0246.1.html> (January 8, 2025).
- Shakya, Shishir and Collin Hodges (2022). “Must-Access Prescription Drug Monitoring Programs and Retail Opioid Sales,” *Contemporary Economic Policy*, 41(1): 146-165.
- Shakya, Shishir and Jane E. Ruseski (2023). “The Effect of Prescription Drug Monitoring Programs on County-Level Opioid Prescribing Practices and Spillovers,” *Contemporary Economic Policy*, 41(3): 435-454.
- Smart, Rosanna, David Powell, and Rosalie Liccardo Pacula (2024). “Investigating the Complexity of Naloxone Distribution: Which Policies Matter for Pharmacies and Potential Recipients?” *Journal of Health Economics*, 97:102917.
- Starc, Amanda and Ashley Swanson (2021). “Preferred Pharmacy Networks and Drug Costs,” *American Economic Journal: Economic Policy*, 13(3): 406-446.
- Sun, Liyang, and Sarah Abraham (2021). “Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects,” *Journal of Econometrics*, 225(2): 175-199.
- Trygstad, Troy (2020). “A Sleeping Giant: Community Pharmacy’s Potential is Unrivaled,” *Journal of Managed Care & Specialty Pharmacy*, 26(6): 705-707.
- Twilman, Robert K. (2012). “Pill Mills Are Not Pain Clinics: The Challenges of Addressing One Without Harming the Other,” *Journal of Medical Regulation*, 98(2): 7-11.
- Ukert, Benjamin and Daniel Polsky (2023). “How Do “Must-Access” Prescription Drug Monitoring Programs Address Opioid Misuse?” *American Journal of Health Economics*, 9(3): 374-404.
- Valliant, Samantha N, Sabree C. Burbage, Shweta Pathak, and Benjamin Y. Urick (2022), “Pharmacists as Accessible Health Care Providers: Quantifying the Opportunity,” *Journal of Managed Care and Specialty Pharmacy*, 28(1): 85-90.
- Viscari, Marília Berlofa, Isabel Vitória Figueiredo, and Táacio de Mendonça Lima (2021). “Role of Pharmacist During the COVID-19 Pandemic: A Scoping Review,” *Research in Social and Administrative Pharmacy*, 17(1): 1799-1806.

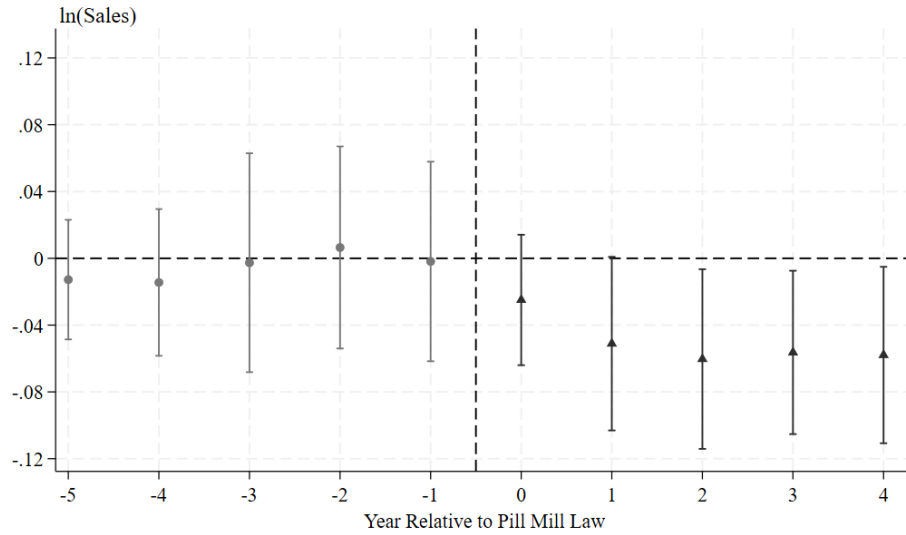
Wittenauer, Rachel, Parth D. Shah, Jennifer L. Bacci, and Andy Stergachis (2024). "Locations and Characteristics of Pharmacy Deserts in the United States: A Geospatial Study," *Health Affairs Scholar*, 2(4): qxae035.

Ziedan, Engy and Robert Kaestner (2024). "Effect of Prescription Opioid Control Policies on Infant Health," *Southern Economic Journal*, 90: 828-877.

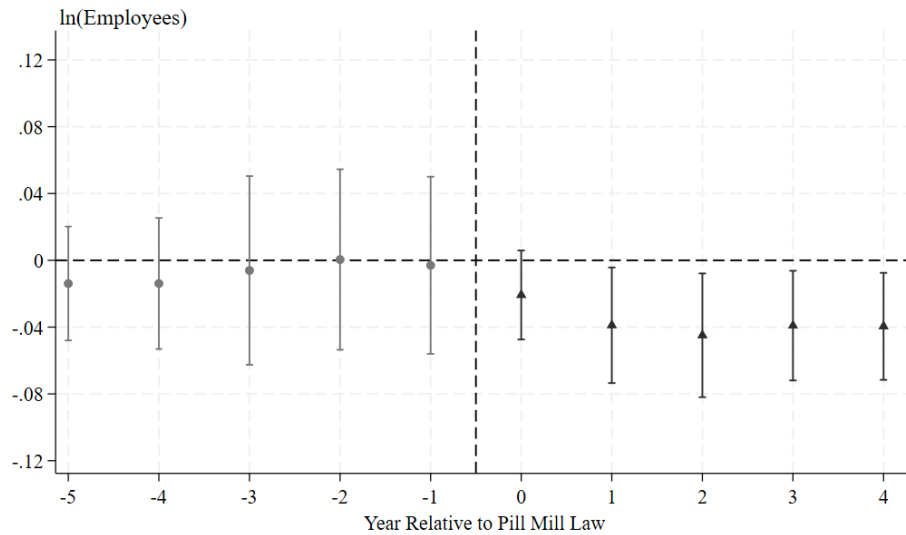
A map of the United States where each state is represented by a hexagon. The hexagons are arranged in a honeycomb pattern. The states are labeled with their two-letter abbreviations. The following states are shaded gray: WI, IN, OH, KY, WV, TN, LA, MS, AL, GA, TX, FL, HI, and AK.

Note: The shaded states indicate states that adopted pill mill laws during our sample period.

Figure 2: Pharmacy Sales and Employment Fell Following the Adoption of a State Pill Mill Law



(A) Pharmacy Sales

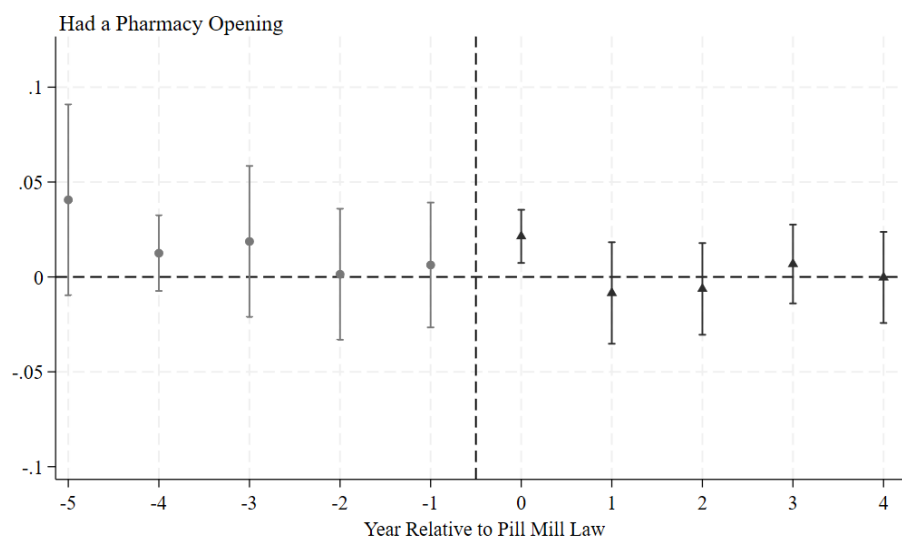


(B) Pharmacy Employees

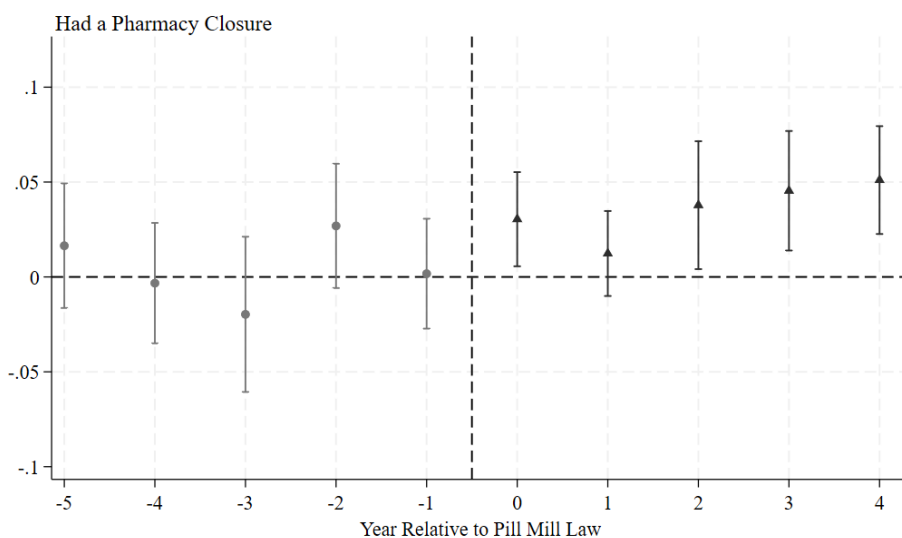
Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. The figures plot the estimates from the event-study specification obtained via Borusyak et al. (2024). The light-gray circles denote tests for parallel trends by exploring changes in the outcomes during the pre-period relative to more than five periods prior to adoption of a state pill mill law. The dark-gray triangles show how the outcomes evolved following the adoption of a state pill mill law relative to the average during the entire pre-period. The vertical bars denote the 95-percent confidence intervals. To allow for a longer post-period, the estimates exclude observations from Wisconsin. Figures reporting a shorter post-period that includes Wisconsin are shown in Appendix Figure 3. Standard errors are clustered at the state level.

Figure 3: Pharmacy Closures Increased Following the Adoption of a State Pill Mill Law



(A) Pharmacy Openings



(B) Pharmacy Closures

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is an indicator for whether the county experienced a pharmacy opening, while the dependent variable in Panel B is an indicator for whether the county experienced a pharmacy closure. The figures plot the estimates from the event-study specification obtained via Borusyak et al. (2024). The light-gray circles denote tests for parallel trends by exploring changes in the outcomes during the pre-period relative to more than five periods prior to adoption of a state pill mill law. The dark-gray triangles show how the outcomes evolved following the adoption of a state pill mill law relative to the average during the entire pre-period. The vertical bars denote the 95-percent confidence intervals. To allow for a longer post-period, the estimates exclude observations from Wisconsin. Figures reporting a shorter post-period that includes Wisconsin are shown in Appendix Figure 3. Standard errors are clustered at the state level.

Table 1: Pill Mill Law Effective Dates

State	Effective Date
Alabama	May 2013
Florida	July 2011
Georgia	July 2013
Indiana	January 2014
Kentucky	July 2011
Louisiana	July 2005
Mississippi	September 2011
Ohio	May 2011
Tennessee	January 2012
Texas	June 2009
West Virginia	September 2014
Wisconsin	March 2016

Sources: Rutkow et al. (2017), Mallatt (2017), 2013 Alabama Public Act 257, 2013 Georgia Act 128, 2013 Indiana Senate Enrolled Act 246, and 2015 Wisconsin Act 265.

Table 2: Summary Statistics

	(1)	(2)	(3)	(4)
Sample →	All States	States Adopting a Pill Mill Law 2000-2018	States Not Adopting a Pill Mill Law 2000-2018	Test Whether Column 2 = Column 3
Panel A: Establishment-Level Outcomes				
Annual Sales	\$3,597,606 (\$11,818,800)	\$3,267,947 (\$10,625,364)	\$3,774,070 (\$12,406,979)	t = 21.90 p < 0.001
Employees	13.14 (42.73)	12.23 (40.24)	13.63 (44.00)	t = 16.79 p < 0.001
Observations	1,150,783	401,231	749,552	1,150,783
Panel B: County-Level Outcomes				
Openings	1.40 (6.59)	1.32 (6.15)	1.45 (6.85)	t = 2.36 p = 0.02
Closures	1.02 (4.70)	0.985 (4.63)	1.05 (4.74)	t = 1.64 p = 0.10
Observations	59,668	23,085	36,583	59,668

Source: National Establishment Time-Series, 2000-2018

Note: Panel A reports the average value of annual sales and the number of employees at the establishment level. Panel B reports the average number of pharmacy openings and closures at the county level. Standard deviations are reported in parentheses. Column 1 reports the statistics for all states, column 2 limits the sample to states that adopted a pill mill law during the sample period, and column 3 limits the sample to states that did not adopt a pill mill law during the sample period. Finally, column 4 reports t-statistics and the corresponding p-values from testing whether the values in columns 2 and 3 are equal.

Table 3: State Pill Mill Laws Were Associated with Reductions in Pharmacy Sales and Employment

	(1)	(2)
Outcome →	ln(Sales)	ln(Employees)
Pill Mill Law	-0.053** (0.024)	-0.027* (0.015)
Observations	1,150,783	1,150,783

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the real value of annual sales, while the dependent variable in column 2 is the natural log of the number of employees. Panel A reports the estimates obtained from the difference-in-differences specification obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 4: The Relationship Between State Pill Mill Laws and Reductions in Retail Pharmacy Outcomes Was More Pronounced in More Competitive Areas

	(1)	(2)	(3)	(4)
Sample →	Full Sample	Low- Competition Area	Moderate- Competition Area	High- Competition Area
Panel A: Dependent Variable is ln(Sales)				
Pill Mill Law	-0.053** (0.024)	-0.013 (0.021)	-0.059** (0.024)	-0.071*** (0.026)
Observations	1,150,783	293,198	573,084	284,501
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.027* (0.015)	0.000 (0.016)	-0.027 (0.018)	-0.035 (0.022)
Observations	1,150,783	293,198	573,084	284,501

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. The estimates are obtained from the difference-in-differences specification obtained via Borusyak et al. (2024). Column 1 reports the baseline estimates. We determined whether an establishment likely faced competitive pressure from other pharmacies by examining the total number of other pharmacies within 5,000 meters of each establishment. Column 2 limits the sample to establishments in the bottom fourth of this distribution, column 3 to establishments in the middle half of this distribution, and column 4 to establishments in the top fourth of the distribution. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 5: State Pill Mill Laws Were Associated with Increases in Sales and Employment at Nearby Pharmacies in States Never Adopting Pill Mill Laws

	(1)	(2)
Outcome →	ln(Sales)	ln(Employees)
Bordering State Pill Mill Law	0.058** (0.028)	0.087*** (0.023)
Observations	749,552	749,552

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the real value of annual sales, while the dependent variable in column 2 is the natural log of the number of employees. The sample is limited to establishments located in states that never adopted a pill mill law. The columns report the estimates from a modified version of the difference-in-differences specification, where the independent variable of interest denotes whether the establishment was in a county on the border with a state that had adopted a pill mill law. The estimates are obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 6: Alternative Levels of Fixed Effects Indicate the Reductions Were Due to Changes at the Extensive Margin

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome →	ln(Sales)	ln(Employees)	ln(Sales)	ln(Employees)	ln(Sales)	ln(Employees)
Pill Mill Law	-0.053** (0.024)	-0.027* (0.015)	-0.055** (0.021)	-0.017 (0.013)	0.012 (0.015)	0.014*** (0.004)
Observations	1,150,783	1,150,783	1,150,783	1,150,783	1,150,783	1,150,783
Drug Policy Controls	Y	Y	Y	Y	Y	Y
Business Cycle Controls	Y	Y	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y	Y	Y
State & Year FE	Y	Y				
County & Year FE			Y	Y		
Establishment & Year FE					Y	Y

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in the odd-numbered columns is the natural log of the real value of annual sales, while the dependent variable in the even-numbered columns is the natural log of the number of employees. Columns 1 and 2 include state fixed effects, year fixed effects, and additional state- and county-level time-varying covariates. Columns 3 and 4 replace the state-level fixed effects with county-level fixed effects. Finally, columns 5 and 6 replace the county-level fixed effects with establishment-level fixed effects, meaning effects are identified from within-establishment changes over time. The estimates are obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 7: State Pill Mill Laws Were Associated with an Increase in the Likelihood Counties Experienced a Pharmacy Closure

	(1)	(2)
Outcome →	County Had a Pharmacy Opening	County Had a Pharmacy Closure
Pill Mill Law	0.002 (0.009)	0.037*** (0.010)
Mean	0.323	0.304
Observations	59,668	59,668

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is an indicator for whether the county had a pharmacy opening in that year, while the dependent variable in column 2 is an indicator for whether the county had a pharmacy closure in that year. The estimates are obtained from the difference-in-differences specification obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 8: State Pill Mill Laws Were Associated with Increases in Closures of Standalone Establishments

	(1)	(2)	(3)	(4)
	Standalone Pharmacies		Non-Standalone Pharmacies	
	County Had a Pharmacy Opening	County Had a Pharmacy Closure	County Had a Pharmacy Opening	County Had a Pharmacy Closure
Pill Mill Law	0.005 (0.008)	0.042*** (0.008)	0.002 (0.011)	-0.006 (0.009)
Observations	59,668	59,668	59,668	59,668

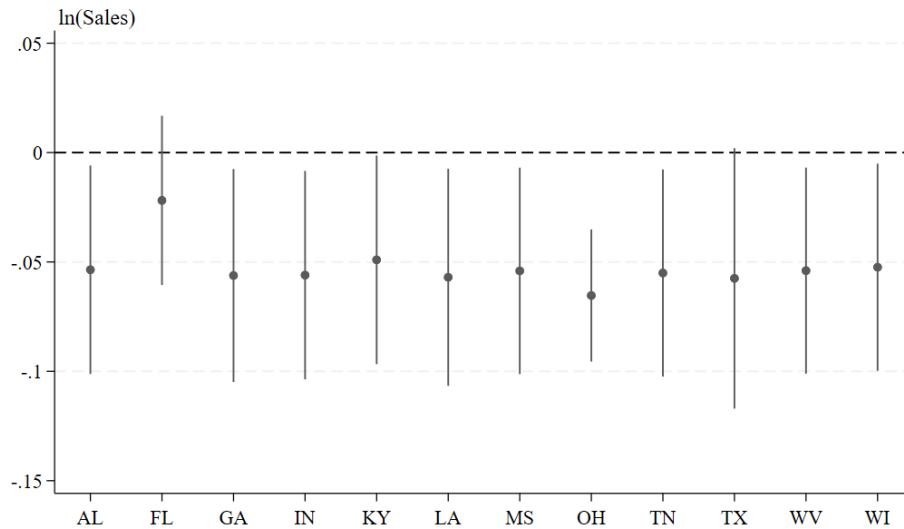
Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is an indicator for whether the county had a standalone pharmacy opening, the dependent variable in column 2 is an indicator for whether the county had a standalone pharmacy closure, the dependent variable in column 3 is an indicator for whether the county had a non-standalone pharmacy opening, and the dependent variable in column 4 is an indicator for whether the county had a non-standalone pharmacy closure. The estimates are obtained using the difference-in-differences specification obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

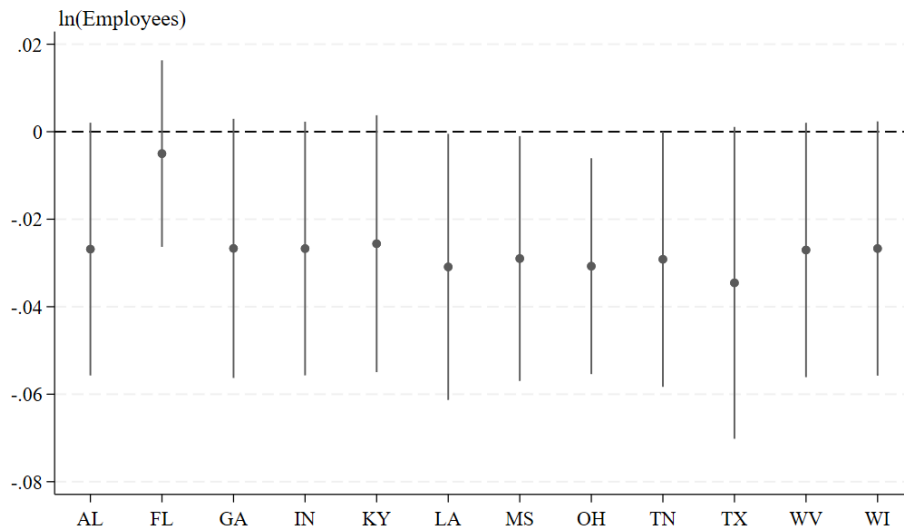
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

7. ONLINE APPENDIX

Appendix Figure 1: The Relationship Between State Pill Mill Laws and Pharmacy Sales and Employment When Iteratively Excluding Each Treated State



(A)

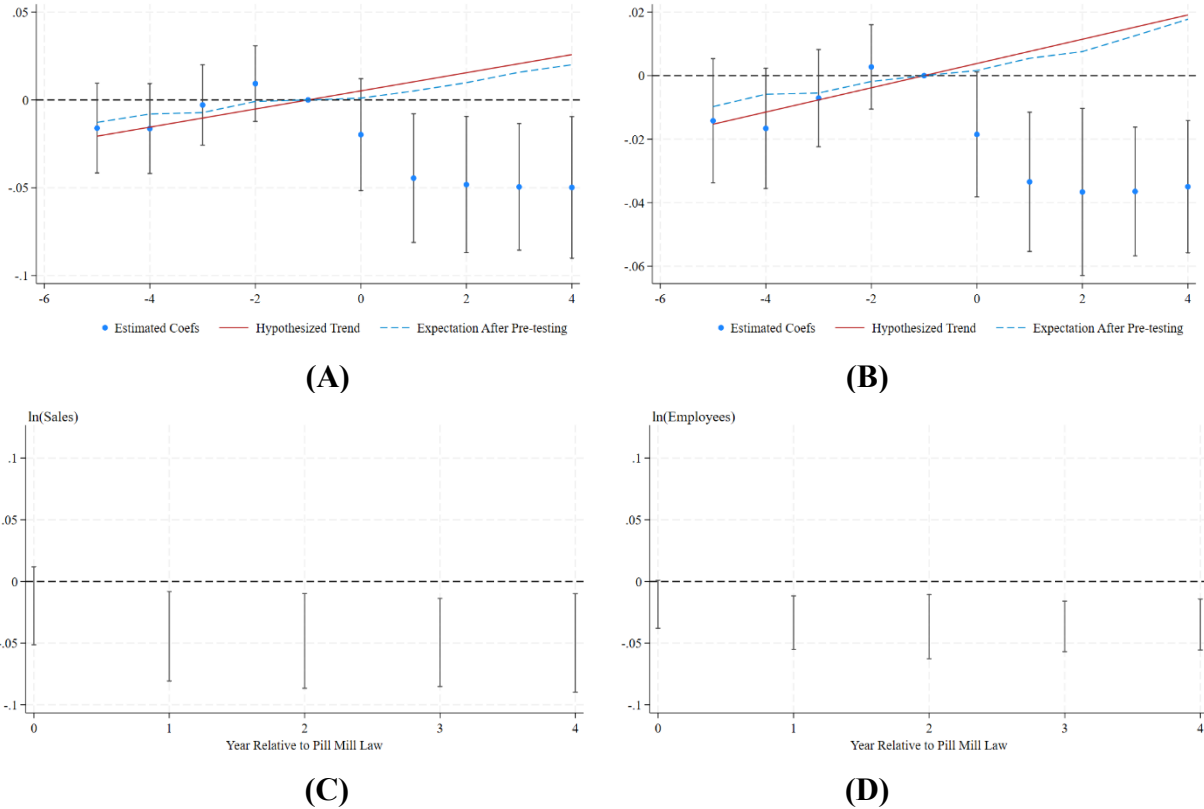


(B)

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of pharmacy sales, and the dependent variable in Panel B is the natural log of the number of employees. The figures plot the estimates from the static difference-in-differences specification obtained via Borusyak et al. (2024). The circle markers denote the point estimates and the vertical bars the 95-percent confidence intervals. Each regression is obtained by excluding one of the treated states, shown on the horizontal axis. Standard errors are clustered at the state level.

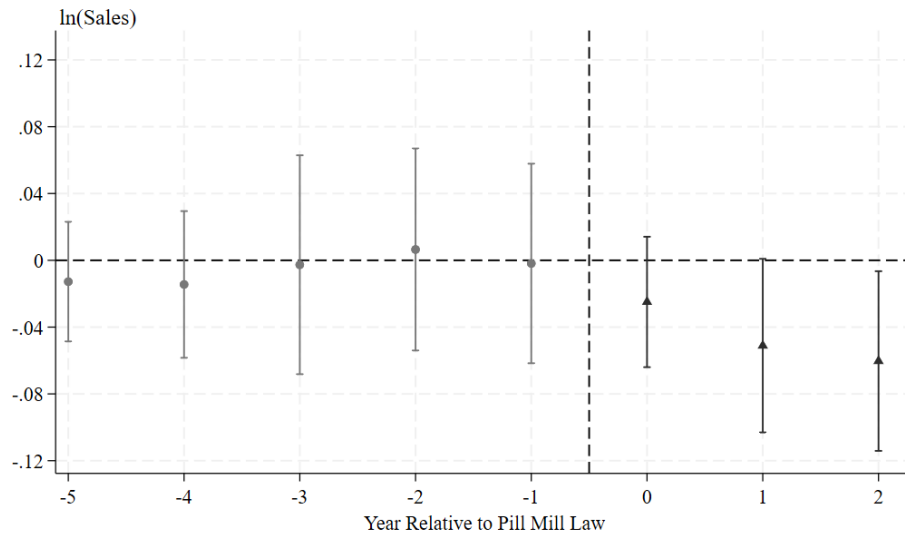
Appendix Figure 2: Tests of Power to Reject Parallel Trends for Changes in Sales and Employment



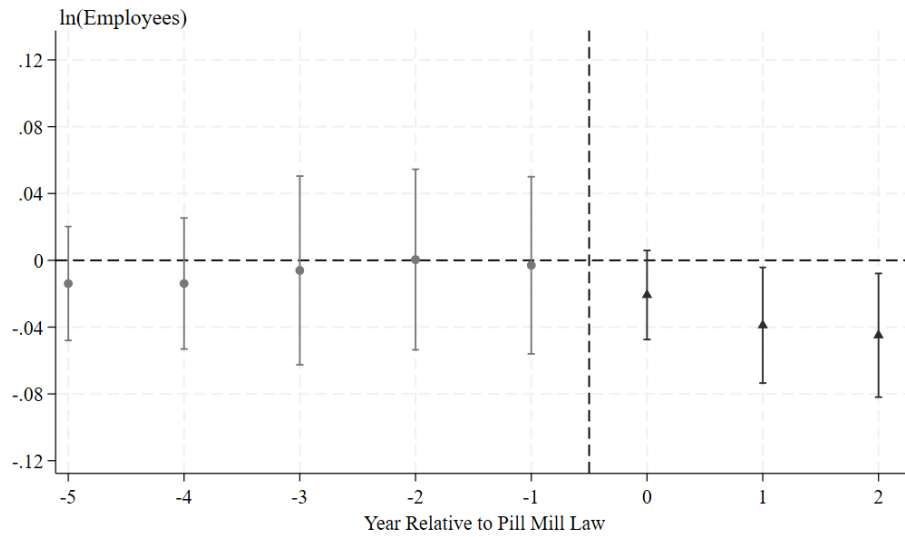
Source: National Establishment Time-Series, 2000-2018

Notes: Panels A and B report results from the Roth (2022) test of power for rejecting non-parallel pre-trends assuming 50-percent power from a two-way-fixed-effects event study where the dependent variables are the natural log of pharmacy sales and the natural log of the number of employees. The estimates use the full set of controls. The blue circles plot the event-study coefficients, and the black vertical bars plot the corresponding 95-percent confidence intervals. The solid red line plots the hypothesized worst-case-scenario trend obtained when assuming 50 percent power. The dashed light blue line plots the expected coefficients conditional on not finding a significant pre-trend. Panels C and D report the post-period event-study estimates obtained from Rambachan and Roth (2023) after imposing parallel trends in the post-period (i.e., $M = 0$).

**Appendix Figure 3: Event-Study Estimates Including Wisconsin
That Examine Pharmacy Sales and Employment**



(A) Pharmacy Sales

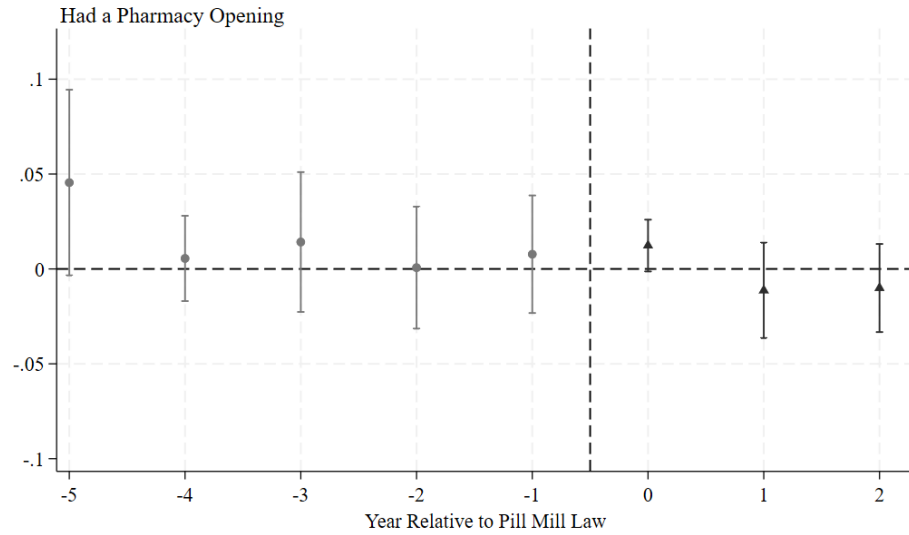


(B) Pharmacy Employees

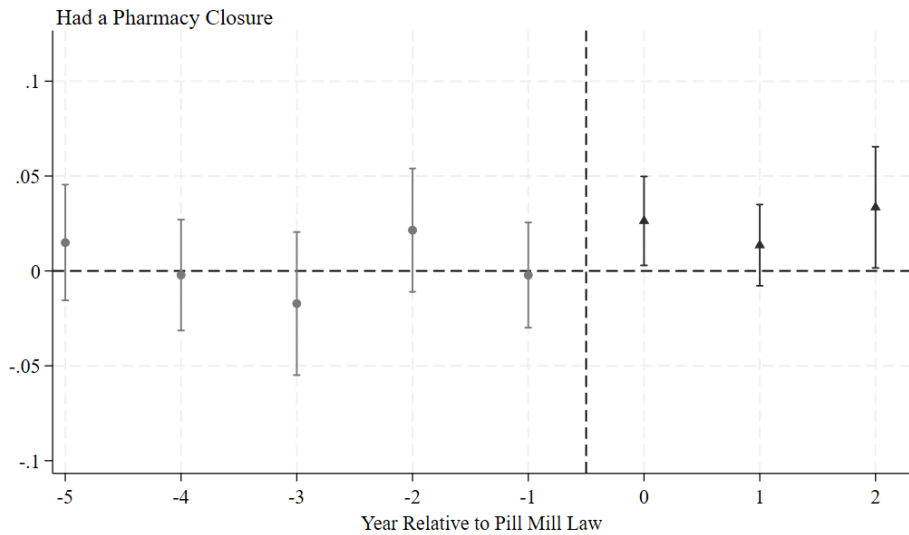
Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. The figures plot the estimates from the event-study specification obtained via Borusyak et al. (2024). The circle markers denote the point estimates and the vertical bars the 95-percent confidence intervals. Standard errors are clustered at the state level.

Appendix Figure 4: Event-Study Estimates Including Wisconsin That Examine Pharmacy Openings and Closures



(A) Pharmacy Openings

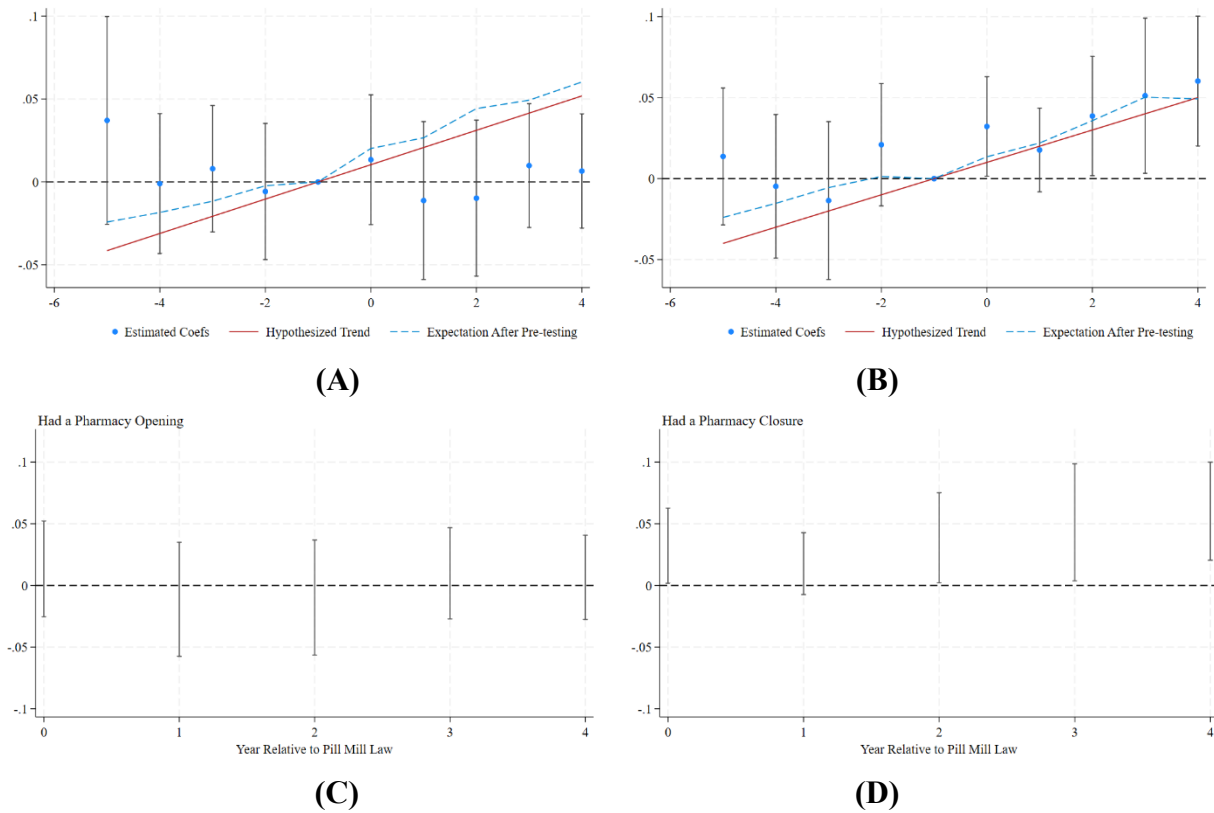


(B) Pharmacy Closures

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is an indicator for whether the county had a pharmacy opening, while the dependent variable in Panel B is an indicator for whether the county had a pharmacy closure. The figures plot the estimates from the event-study specification obtained via Borusyak et al. (2024). The circle markers denote the point estimates and the vertical bars the 95-percent confidence intervals. Standard errors are clustered at the state level.

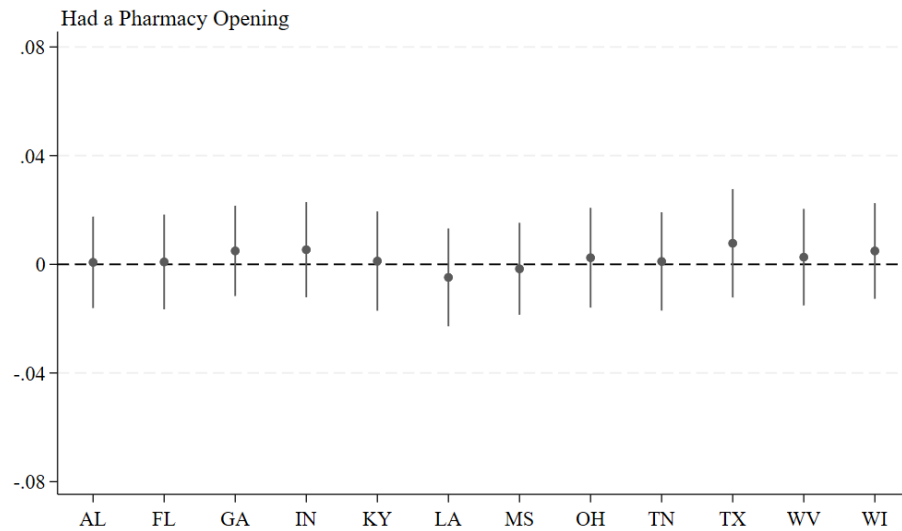
Appendix Figure 5: Tests of Power to Reject Parallel Trends for Changes in the Likelihood of Having a County-Level Pharmacy Opening or Closure



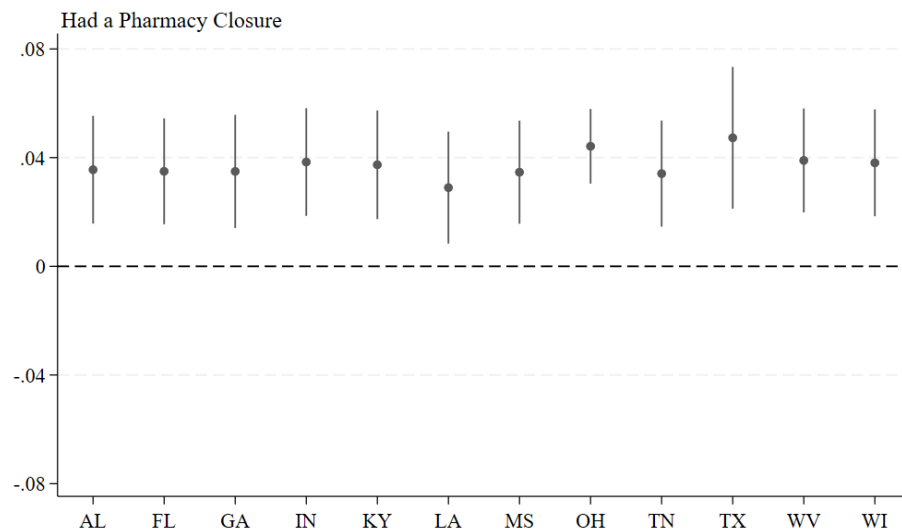
Source: National Establishment Time-Series, 2000-2018

Notes: Panels A and B report results from the Roth (2022) test of power for rejecting non-parallel pre-trends assuming 50-percent power from a two-way-fixed-effects event study where the dependent variables are indicators for whether the county had a pharmacy opening or a pharmacy closure. The estimates use the full set of controls. The blue circles plot the event-study coefficients, and the black vertical bars plot the corresponding 95-percent confidence intervals. The solid red line plots the hypothesized worst-case-scenario trend obtained when assuming 50-percent power. The dashed light-blue line plots the expected coefficients conditional on not finding a significant pre-trend. Panels C and D report the post-period event-study estimates obtained from Rambachan and Roth (2023) after imposing parallel trends in the post period (i.e., $M = 0$).

Appendix Figure 6: The Relationship Between State Pill Mill Laws and Pharmacy Openings and Closures When Iteratively Excluding Each Treated State



(A)

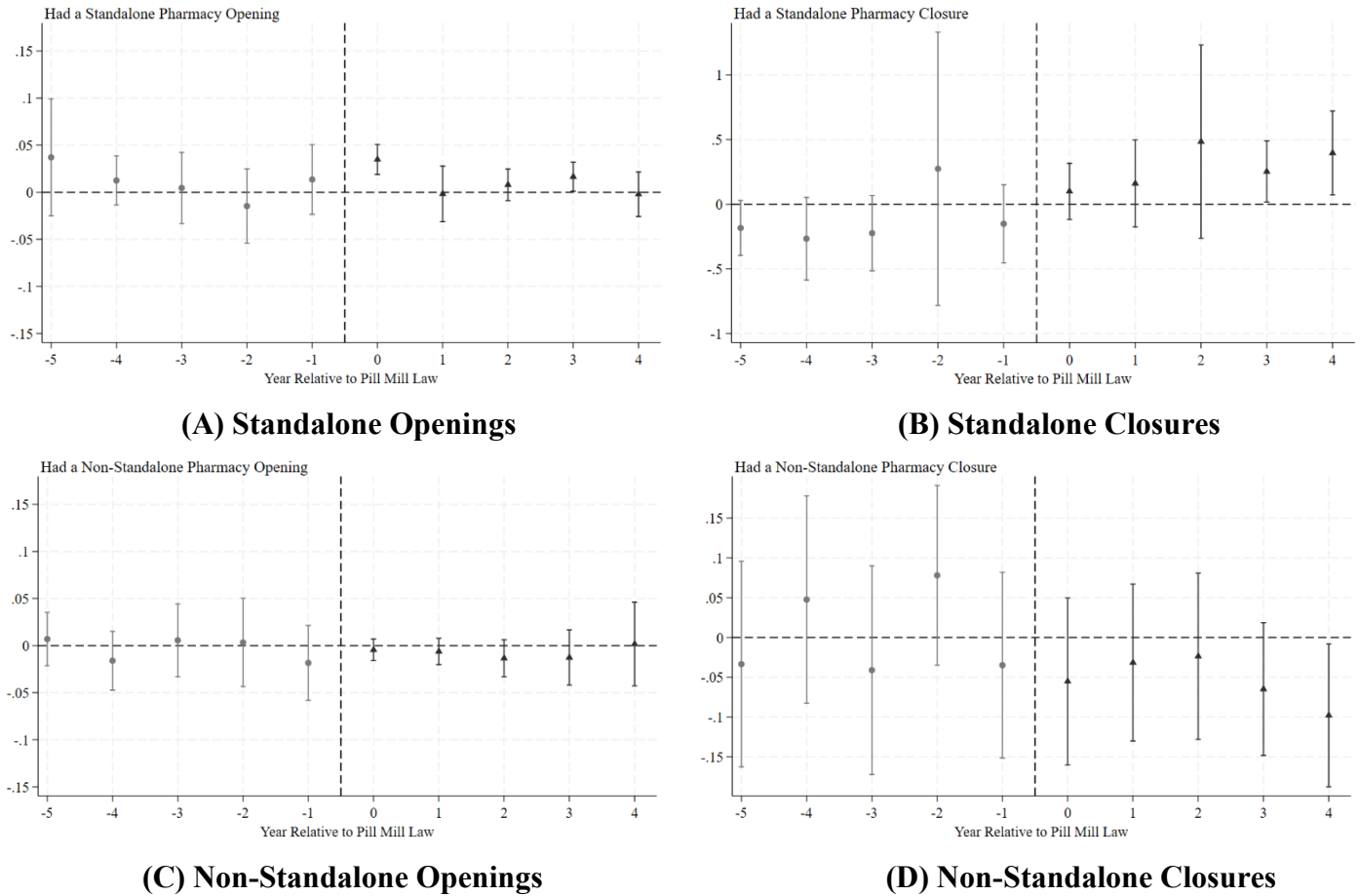


(B)

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the number of county-level pharmacy openings, and the dependent variable in Panel B is the natural log of the number of county-level pharmacy closures. The figures plot the estimates from the static difference-in-differences specification obtained via Borusyak et al. (2024). The circle markers denote the point estimates and the vertical bars the 95-percent confidence intervals. Each regression is obtained by excluding one of the treated states, shown on the horizontal axis. Standard errors are clustered at the state level.

Appendix Figure 7: Standalone Pharmacy Closures Increased After the Adoption of a State Pill Mill Law



Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is an indicator for whether the county had a standalone pharmacy opening, the dependent variable in Panel B is an indicator for whether the county had a standalone pharmacy closure, the dependent variable in Panel C is an indicator for whether the county had a non-standalone pharmacy opening, and the dependent variable in Panel D is an indicator for whether the county had a non-standalone pharmacy closure. The figures plot the estimates from the event-study specification obtained via Borusyak et al. (2024). The circle markers denote the point estimates and the vertical bars the 95-percent confidence intervals. To allow for a longer post-period, the estimates exclude observations from Wisconsin. Standard errors are clustered at the state level.

Appendix Table 1: Summary Statistics for the Covariates

	(1)	(2)	(3)
Sample →	All States	States Adopting a Pill Mill Law 2000-2018	States Not Adopting a Pill Mill Law 2000-2018
Any PDMP	0.807 (0.395)	0.769 (0.422)	0.827 (0.378)
Must-Access PDMP	0.112 (0.315)	0.112 (0.316)	0.111 (0.314)
Medical Marijuana Law	0.331 (0.471)	0.070 (0.256)	0.470 (0.499)
Recreational Marijuana Law	0.041 (0.198)	0.000 -	0.063 (0.243)
Active Medical Dispensaries	0.233 (0.423)	0.042 (0.202)	0.335 (0.472)
Active Recreational Dispensaries	0.018 (0.132)	0.000 -	0.027 (0.162)
Unemployment Rate	6.113 (2.077)	6.108 (1.950)	6.116 (2.142)
ln(Unemployment Claims)	13.157 (0.995)	13.048 (0.659)	13.214 (1.130)
ln(Residential Permits)	15.586 (1.067)	15.860 (1.130)	15.439 (1.002)
ln(State Product Per Capita)	10.955 (0.184)	10.849 (0.131)	11.011 (0.183)
ln(Minimum Wage)	2.061 (0.117)	2.009 (0.076)	2.089 (0.125)
Percent Black	0.139 (0.134)	0.173 (0.144)	0.121 (0.125)
Percent Hispanic	0.155 (0.165)	0.157 (0.194)	0.154 (0.146)
Percent Aged 18-64	0.624 (0.032)	0.619 (0.032)	0.627 (0.031)
Percent Aged 65 or Older	0.140 (0.038)	0.141 (0.046)	0.139 (0.033)
ln(County Population)	12.804 (1.689)	12.466 (1.620)	12.985 (1.697)

Appendix Table 2: The Relationships Are Robust to Additional Controls for Spatial Heterogeneity, Sample Restrictions, and Estimation Strategies

	(1)	(2)	(3)	(4)	(5)
Specification →	Baseline	(1) + Census Region-by-Year Fixed Effects	(1) + Census Division-by-Year Fixed Effects	(1) Excluding the Smallest and Largest Establishments	(1) Using a Two-Way-Fixed-Effects Estimator
Panel A: Dependent Variable is ln(Sales)					
Pill Mill Law	-0.053** (0.024)	-0.0658** (0.023)	-0.076*** (0.024)	-0.057** (0.024)	-0.042* (0.024)
Observations	1,150,783	1,150,783	1,150,783	1,016,328	1,150,783
Panel B: Dependent Variable is ln(Employees)					
Pill Mill Law	-0.027* (0.015)	-0.006 (0.014)	-0.044*** (0.016)	-0.032** (0.014)	-0.026 (0.016)
Observations	1,150,783	1,150,783	1,150,783	1,016,328	1,150,783

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. Column 1 reports the estimates from the static difference-in-differences specification obtained via Borusyak et al. (2024). Column 2 augments this specification with Census region-by-year fixed effects, and column 3 replaces the Census region-by-year fixed effects with Census division-by-year fixed effects. Column 4 excludes establishments in the bottom and top five percent based on employment. Finally, column 5 uses a two-way-fixed-effects estimator. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 3: Robustness Tests Excluding
the Largest and Smallest Establishments**

	(1)	(2)	(3)	(4)
Outcome →	ln(Sales)		ln(Employees)	
Restriction →	Excluding the Top and Bottom 5%	Excluding the Top and Bottom 10%	Excluding the Top and Bottom 5%	Excluding the Top and Bottom 10%
Pill Mill Law	-0.057** (0.024)	-0.062*** (0.024)	-0.032** (0.014)	-0.037*** (0.013)
Observations	1,016,328	909,962	1,016,328	909,962

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in columns 1 and 2 is the natural log of the real value of annual sales, while the dependent variable in columns 3 and 4 is the natural log of the number of employees. The estimates are obtained using the static difference-in-differences specification obtained via Borusyak et al. (2024). Columns 1 and 3 exclude establishments in the bottom and top five percent based on employment (i.e., restricting the sample to establishments with 3-39 employees). Columns 2 and 4 exclude establishments in the bottom and top 10 percent based on employment (i.e., restricting the sample to establishments with 4-29 employees). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 4: Robustness Tests Excluding Establishments with Fewer than 5 Employees

	(1)	(2)
Outcome →	ln(Sales)	ln(Employees)
Pill Mill Law	-0.059** (0.027)	-0.042* (0.023)
Observations	809,717	809,717

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the real value of annual sales, while the dependent variable in column 2 is the natural log of the number of employees. The estimates are obtained using the static difference-in-differences specification obtained via Borusyak et al. (2024). The sample excludes establishments with fewer than five employees. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 5: Dynamic Effects of
State Pill Mill Laws on Pharmacy Sales and Employment**

Outcome →	(1) ln(Sales)	(2) ln(Employees)
5 Years Before	-0.013 (0.018)	-0.014 (0.017)
4 Years Before	-0.014 (0.022)	-0.014 (0.020)
3 Years Before	-0.003 (0.033)	-0.006 (0.029)
2 Years Before	0.006 (0.031)	0.000 (0.028)
1 Year Before	-0.002 (0.033)	-0.003 (0.027)
Policy Change	-0.025 (0.020)	-0.021 (0.014)
1 Year After	-0.051* (0.027)	-0.039** (0.018)
2 Years After	-0.060** (0.027)	-0.045** (0.019)
3 Years After	-0.056** (0.025)	-0.039** (0.017)
4 Years After	-0.058** (0.027)	-0.040** (0.016)
Observations	1,134,241	1,134,241

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the real value of annual sales, while the dependent variable in column 2 is the natural log of the number of employees. The estimates are from the event-study specification obtained via Borusyak et al. (2024). To allow for a longer post-period, the estimates exclude observations from Wisconsin. Event studies including Wisconsin are reported in Appendix Figure 3. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 6: Robustness Test Defining Competition
Based on the Volume of Sales in an Area**

	(1)	(2)	(3)	(4)
Specification →	Full Sample	Low- Competition Area	Moderate- Competition Area	High- Competition Area
Panel A: Dependent Variable is ln(Sales)				
Pill Mill Law	-0.053** (0.024)	-0.024 (0.018)	-0.057** (0.028)	-0.085*** (0.033)
Observations	1,150,783	287,507	575,449	287,827
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.027* (0.015)	-0.012 (0.017)	-0.025 (0.018)	-0.046* (0.025)
Observations	1,150,783	287,507	575,449	287,827

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. Column 1 reports the estimates from our baseline difference-in-differences specification obtained via Borusyak et al. (2024). We determined whether an establishment likely faced competitive pressure from other pharmacies by examining the sales volume of other pharmacies within 5,000 meters of each establishment. Column 2 limits the sample to establishments in the bottom fourth of this distribution, column 3 to establishments in the middle half of this distribution, and column 4 to establishments in the top fourth of the distribution. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 7: Robustness Test Using a
Larger Radius to Define a Pharmacy Market Area**

	(1)	(2)	(3)	(4)
Specification →	Full Sample	Low- Competition Area	Moderate- Competition Area	High- Competition Area
Panel A: Dependent Variable is ln(Sales)				
Pill Mill Law	-0.053** (0.024)	-0.006 (0.020)	-0.067*** (0.022)	-0.075** (0.029)
Observations	1,150,783	305,031	561,312	284,440
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.027* (0.015)	0.001 (0.015)	-0.029* (0.017)	-0.037 (0.025)
Observations	1,150,783	305,031	561,312	284,440

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. Column 1 reports the estimates from our baseline difference-in-differences specification, obtained via Borusyak et al. (2024). We determined whether an establishment likely faced competitive pressure from other pharmacies by examining the total number of other pharmacies within 10,000 meters of each establishment. Column 2 limits the sample to establishments in the bottom fourth of this distribution, column 3 to establishments in the middle half of this distribution, and column 4 to establishments in the top fourth of the distribution. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 8: Robustness Test Using a
Smaller Radius to Define a Pharmacy Market Area**

	(1)	(2)	(3)	(4)
Specification →	Full Sample	Low- Competition Area	Moderate- Competition Area	High- Competition Area
Panel A: Dependent Variable is ln(Sales)				
Pill Mill Law	-0.053** (0.024)	-0.022 (0.023)	-0.069*** (0.025)	-0.082*** (0.027)
Observations	1,150,783	409,195	561,462,112	279,476
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.027* (0.015)	-0.002 (0.022)	-0.028* (0.016)	-0.062*** (0.015)
Observations	1,150,783	409,195	561,462,112	279,476

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. Column 1 reports the estimates from our baseline difference-in-differences specification obtained via Borusyak et al. (2024). We determined whether an establishment likely faced competitive pressure from other pharmacies by examining the total number of other pharmacies within 1,000 meters of each establishment. Column 2 limits the sample to establishments in the bottom fourth of this distribution, column 3 to establishments in the middle half of this distribution, and column 4 to establishments in the top fourth of the distribution. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 9: State Pill Mill Laws and Pharmacy Outcomes,
Excluding Border Counties from the Sample**

	(1)	(2)
Outcome →	ln(Sales)	ln(Employees)
State Pill Mill Law	-0.061*** (0.020)	-0.034** (0.014)
Observations	704,367	704,367

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the real value of annual sales, while the dependent variable in column 2 is the natural log of the number of employees. The sample is limited to establishments located in counties not on state borders. The columns report the estimates from the difference-in-differences specification obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 10: State Pill Mill Laws Were Associated with an Increase in the Likelihood That Counties Experienced a Pharmacy Closure

	(1)	(2)	(3)	(4)
Outcome →	ln(Openings) Openings > 0	ln(Closures) Closures > 0	ln(Openings)	ln(Closures)
Pill Mill Law	0.007 (0.022)	0.075** (0.030)	0.002 (0.055)	0.123*** (0.047)
Estimator	BJS	BJS	Stacked PPML	Stacked PPML
Observations	19,272	18,142	279,166	279,166

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the number of pharmacy openings that a county had in a given year among counties that experienced an opening. The dependent variable in column 2 is the natural log of the number of pharmacy closures that a county had in a given year among counties that experienced a closure. The dependent variable in column 3 is the number of pharmacy openings, while the dependent variable in column 4 is the number of pharmacy closures. Columns 1 and 2 are estimated via Borusyak et al. (2024). Columns 3 and 4 are estimated via Poisson regression by pseudo-maximum likelihood (Correia et al. 2020). Because this strategy is not compatible with the Borusyak et al. (2024) estimator, we instead use a stacked difference-in-differences approach (Cengiz et al. 2019). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 11: Robustness Tests Examining the
Effects of State Pill Mill Laws on Pharmacy Openings and Closures**

Specification →	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	(1) + Census Region-by- Year Fixed Effects	(1) + Census Division-by- Year Fixed Effects	(1) Excluding the Smallest and Largest Establishments	Excluding Border Counties	(1) Using a Two-Way- Fixed-Effects Estimator
Panel A: Outcome is an Indicator for Whether the County Had a Pharmacy Opening						
Pill Mill Law	0.002 (0.009)	0.002 (0.010)	-0.015 (0.011)	0.002 (0.010)	-0.005 (0.008)	0.002 (0.010)
Observations	59,668	59,668	59,668	59,668	37,174	59,668
Panel B: Outcome is an Indicator for Whether the County Had a Pharmacy Closure						
Pill Mill Law	0.037*** (0.010)	0.029** (0.014)	-0.003 (0.007)	0.037** (0.011)	0.037*** (0.011)	0.033** (0.013)
Observations	59,668	59,668	59,668	59,668	37,174	59,668

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the number of county-level pharmacy openings, and the dependent variable in Panel B is the natural log of the number of county-level pharmacy closures. Column 1 reports the estimates from the static difference-in-differences specification obtained via Borusyak et al. (2024). Column 2 augments this specification with Census region-by-year fixed effects, and column 3 augments the specification with Census division-by-year fixed effects. Column 4 excludes establishments in the top and bottom 5 percent of the employee distribution. Column 5 excludes establishments located in border counties. Finally, column 6 uses a two-way-fixed-effects estimator. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 12: The Relationship Between State Pill Mill Laws and Changes in Pharmacy Openings and Closures, by Competitive Area

	(1)	(2)	(3)	(4)
Specification →	Full Sample	Low- Competition Area	Moderate- Competition Area	High- Competition Area
Panel A: Outcome is an Indicator for Whether the County Had a Pharmacy Opening				
Pill Mill Law	0.002 (0.009)	0.003 (0.003)	0.002 (0.006)	0.004 (0.008)
Observations	59,668	59,668	59,668	59,668
Panel B: Outcome is an Indicator for Whether the County Had a Pharmacy Closure				
Pill Mill Law	0.037*** (0.010)	0.003 (0.003)	0.002 (0.006)	0.004 (0.008)
Observations	59,668	59,668	59,668	59,668

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the real value of annual sales, while the dependent variable in Panel B is the natural log of the number of employees. Column 1 reports the estimates from our baseline difference-in-differences specification obtained via Borusyak et al. (2024). We determined whether an establishment likely faced competitive pressure from other pharmacies by examining the total number of other pharmacies within 5,000 meters of each establishment. Column 2 limits the sample to establishments in the bottom fourth of this distribution, column 3 to establishments in the middle half of this distribution, and column 4 to establishments in the top fourth of the distribution. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 13: State Pill Mill Laws Were Associated with a Larger Increase in the Likelihood That Non-Metropolitan Counties Experienced a Pharmacy Closure Relative to Metropolitan Counties

	(1)	(2)	(3)	(4)
Group →	Below-Median Poverty Rate in the Year 2000	Above-Median Poverty Rate in the Year 2000	Metropolitan Counties	Non- Metropolitan Counties
Panel A: Outcome is an Indicator for Whether the County Had a Pharmacy Opening				
Pill Mill Law	-0.003 (0.009)	-0.002 (0.011)	0.004 (0.011)	0.001 (0.010)
Mean	0.386	0.261	0.572	0.191
Observations	29,622	29,977	20,662	38,954
Panel B: Outcome is an Indicator for Whether the County Had a Pharmacy Closure				
Pill Mill Law	0.033** (0.015)	0.038*** (0.010)	0.028** (0.014)	0.040*** (0.009)
Mean	0.363	0.264	0.529	0.185
Observations	29,622	29,977	20,662	38,954

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is an indicator for whether the county had a pharmacy opening, while the dependent variable in Panel B is an indicator for whether the county had a pharmacy closure. Column 1 limits the sample to counties that had a below-median poverty rate in the year 2000, and column 2 examines counties that had an above-median poverty rate in the year 2000. Column 3 examines metropolitan counties, while column 4 examines non-metropolitan counties (i.e., micropolitan and non-core counties). The estimates are from the difference-in-differences specification obtained via Borusyak et al. (2024). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 14: State Pill Mill Laws Were Inconclusively Related to Changes in Sales and Employment When Separately Examining Standalone and Non-Standalone Pharmacies

	(1)	(2)	(3)	(4)
	Standalone Pharmacies		Non-Standalone Pharmacies	
	ln(Sales)	ln(Employees)	ln(Sales)	ln(Employees)
Panel A: State FE, Year FE, and Additional Covariates				
Pill Mill Law	0.009 (0.012)	0.018* (0.009)	0.001 (0.024)	0.010 (0.022)
Observations	636,050	636,050	514,733	514,733
Panel B: County FE, Year FE, and Additional Covariates				
Pill Mill Law	0.011 (0.011)	0.023** (0.010)	-0.003 (0.024)	0.014 (0.019)
Observations	636,050	636,050	514,733	514,733
Panel C: Establishment FE, Year FE, and Additional Covariates				
Pill Mill Law	0.020*** (0.006)	0.026*** (0.005)	0.003 (0.022)	0.002 (0.006)
Observations	636,050	636,050	514,733	514,733

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in columns 1 and 3 is the natural log of the real value of annual sales, while the dependent variable in columns 2 and 4 is the natural log of the number of employees. Columns 1 and 2 limit the sample to standalone establishments, while columns 3 and 4 limit the sample to non-standalone establishments. The estimates in Panel A are from the difference-in-differences obtained via Borusyak et al. (2024). Panel B replaces the state fixed effects with county fixed effects, and Panel C replaces the county fixed effects with establishment fixed effects. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 15: The Relationship Between State Pill Mill Laws and Pharmacy Openings and Closures is Robust to More Granular Fixed Effects and Alternative Ways of Measuring the Dependent Variable

	(1)	(2)	(3)	(4)
	Standalone Pharmacies		Non-Standalone Pharmacies	
	County Had a Pharmacy Opening	County Had a Pharmacy Closure	County Had a Pharmacy Opening	County Had a Pharmacy Closure
Panel A: Replace State Fixed Effects with County Fixed Effects				
Pill Mill Law	0.005 (0.008)	0.040*** (0.008)	0.004 (0.010)	-0.004 (0.009)
Observations	59,668	59,668	59,668	59,668
Panel B: Replace the Natural Log with the Inverse Hyperbolic Sine				
Pill Mill Law	0.017 (0.013)	0.073*** (0.023)	0.004 (0.016)	-0.018 (0.016)
Observations	59,668	59,668	59,668	56,526
Panel C: Replace the Natural Log with the Rate per 100,000 People				
Pill Mill Law	0.052 (0.054)	0.305*** (0.044)	0.008 (0.026)	-0.018 (0.024)
Observations	59,668	59,668	59,668	59,668

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the number of county-level standalone pharmacy openings. The dependent variable in column 2 is the natural log of the number of county-level standalone pharmacy closures. The dependent variable in column 3 is the natural log of the number of county-level non-standalone pharmacy openings. The dependent variable in column 4 is the natural log of the number of county-level non-standalone pharmacy closures. The estimates are from the difference-in-differences specification obtained via Borusyak et al. (2024). Panel A replaces the state fixed effects with county fixed effects. Panel B replaces the dependent variables with the inverse hyperbolic sine of the outcomes. Panel C replaces the dependent variables with the rate of openings and closures per 100,000 people. Standard errors, shown in parentheses, are clustered at the state level.

*** p < 0.01, ** p < 0.05, * p < 0.10

Appendix Table 16: The Relationship Between State Pill Mill Laws and Retail Pharmacy Openings and Closures, by Standalone Status and Competitive Area

	(1)	(2)	(3)	(4)	(5)	(6)
	Standalone Establishments			Non-Standalone Establishments		
Specification →	Low- Competition Area	Moderate- Competition Area	High- Competition Area	Low- Competition Area	Moderate- Competition Area	High- Competition Area
Panel A: Outcome is an Indicator for Whether the County Had a Pharmacy Opening						
Pill Mill Law	0.005 (0.003)	0.005 (0.005)	0.003 (0.007)	0.001 (0.002)	-0.001 (0.009)	0.001 (0.007)
Observations	59,668	59,668	59,668	59,668	59,668	59,668
Panel B: Outcome is an Indicator for Whether the County Had a Pharmacy Closure						
Pill Mill Law	0.005 (0.003)	0.006 (0.005)	0.003 (0.007)	0.001 (0.002)	-0.001 (0.009)	0.001 (0.007)
Observations	59,668	59,668	59,668	59,668	59,668	59,668

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is an indicator for whether the county experienced a pharmacy opening in a given year, while the dependent variable in Panel B is an indicator for whether the county experienced a pharmacy closure in a given year. We determined whether an establishment likely faced competitive pressure from other pharmacies by examining the total number of other pharmacies within 5,000 meters of each establishment, and the estimates from our baseline difference-in-differences specification obtained via Borusyak et al. (2024). Columns 1 and 4 limit the sample to establishments in the bottom fourth of the competition distribution, columns 2 and 5 to establishments in the middle half of this distribution, and columns 3 and 6 to establishments in the top fourth of the distribution. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table 17: State Pill Mill Laws Were Not Associated with Statistically Significant Changes in the Number of Openings or Closures in Border Counties of States Never Adopting a Pill Mill Law

	(1)	(2)	(3)	(4)	(5)	(6)
			Standalone Pharmacies		Non-Standalone Pharmacies	
Outcome →	County Had a Pharmacy Opening	County Had a Pharmacy Closure	County Had a Pharmacy Opening	County Had a Pharmacy Closure	County Had a Pharmacy Opening	County Had a Pharmacy Closure
Border Pill Mill Law	-0.007 (0.011)	-0.002 (0.011)	0.003 (0.014)	0.003 (0.011)	-0.017 (0.014)	-0.003 (0.014)
Observations	36,583	36,583	36,583	36,583	36,583	36,583

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is an indicator for whether the county had a pharmacy opening, while the dependent variable in column 2 is an indicator for whether the county had a pharmacy closure. The dependent variable in column 3 is an indicator for whether the county had a standalone pharmacy opening, while the dependent variable in column 4 is an indicator for whether the county had a standalone pharmacy closure. The dependent variable in column 5 is an indicator for whether the county had a non-standalone pharmacy opening, while the dependent variable in column 6 is an indicator for whether the county had a non-standalone pharmacy closure. The estimates are obtained using the modified version of the difference-in-differences specification, obtained via Borusyak et al. (2024), where the independent variable of interest indicates that the county borders a state that has adopted a pill mill law. Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

**Appendix Table 18: State Pill Mill Laws Were Unrelated to
Changes in the Overall Size of the Retail Pharmacy Market**

	(1)	(2)	(3)	(4)
Outcome →	County Had No Pharmacy	Pharmacies per 100K	Total Number of Pharmacies	Total Number of Pharmacies
Pill Mill Law	0.004 (0.003)	0.295 (0.510)	0.497 (1.539)	0.348 (0.342)
% of the Mean	7.143	27.752	2.566	1.804
% of a Standard Dev.	1.729	10.344	0.731	0.514
Mean	0.056	1.063	19.287	19.287
Standard Dev.	0.230	2.852	67.745	67.745
Estimator	BJS	BJS	BJS	Stacked PPML
Observations	59,668	59,668	59,668	59,668

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is an indicator for whether the county had no pharmacies, the dependent variable in column 2 is the number of pharmacies per 100,000 people, and the dependent variable in columns 3 and 4 is the total number of pharmacies. Columns 1-3 are estimated via the Borusyak et al. (2024) specification shown in equation (1). Column 4 is estimated via Poisson regression by pseudo-maximum likelihood (Correia et al. 2020). Because this strategy is not compatible with the Borusyak et al. (2024) estimator, we instead use a stacked difference-in-differences approach (Cengiz et al. 2019). Standard errors, shown in parentheses, are clustered at the state level.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$