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Supply-Side Opioid Restrictions and the Retail Pharmacy Market

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Abstract

Policymakers routinely limit the sale of goods thought to be of risk to public health. Despite a large literature studying how these supply-side interventions affect consumer outcomes, 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 driven by an increase in the number of pharmacy closures, particularly among standalone establishments, while surviving establishments experienced modest improvements in market outcomes.

JEL Codes: I18; K23; M20
Key words: opioids; pharmacy; pill mill

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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 otherwise not internalized by the consumers (Conlon and Rao 2023). To reduce consumption, 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 (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., and over 70 percent of these deaths are attributable to opioids (NCHS 2023). To combat this 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 the 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, there is existing evidence that state pill mill laws reduced opioid prescribing (Kaestner and Ziedan 2023), and 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] 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-differences identification strategy accounting 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 2 percent reduction in the number of pharmacy employees. The reductions were limited to the post-adoption

[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, and Alpert et al. (2024) found that these policies reduced opioid prescriptions among patients presenting with diagnoses for which an opioid prescription would be inappropriate.

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 an increase in pharmacy closures, particularly among standalone (i.e., non-chain) establishments, with surviving establishments appearing to have modestly benefited from these policies. This pattern is consistent with prior evidence that independent pharmacies were more likely than chain establishments to engage in drug diversion (Janssen and Zhang 2023).

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. forthcoming). Moreover, because we show that standalone pharmacies – but not chain establishments – were more likely to close following the adoption of a state pill mill law, we build on a broad literature studying the determinants of consolidation within the healthcare industry (Harrison 2007; Town et al. 2007; Bowblis 2011; Postma and Roos 2015; Wollmann 2020; Janssen and Zhang 2023). 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; Neumark and Savych 2023; Ukert and Polsky 2023; Kaestner and Ziedan 2023).

The rest of the 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 relationships 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 & EXISTING EVIDENCE

2.1 Policy Background

Opioid overdoses caused nearly 727,000 deaths between 1999 and 2022. For the first twenty-four 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 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. 2017).

During our sample period, 12 states adopted a pill mill law, and we report the states and adoption years in Table 1.[2] Figure 1 shows that these laws were primarily enacted in southern and midwestern states – particularly in the Appalachian region. This is perhaps unsurprising, given that the majority of pill mills were located in these states (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).

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

2.2 Existing Evidence

Our paper builds on a large literature studying supply-side drug policies (Dobkin and Nicosia 2009; Dobkin et al. 2014; Ruhm 2019; Maclean et al. 2021; Alpert et al. 2022), much of which has focused on the effects of these laws on consumers. Several studies have examined "must-access" prescription drug monitoring programs (PDMPs), which require providers to access state-level databases with a patient's prescription history prior to prescribing controlled substances (Meinhofer 2018; Sacks et al. 2021; Shakya and Hodges 2022; Neumark and Savych 2023; Ukert and Polsky 2023). Using a five percent sample of Medicare Part D beneficiaries from 2007-2013, Buchmueller and Carey (2018) found that must-access PDMPs were associated with reductions in the likelihood that individuals obtained opioids from multiple prescribers and at multiple pharmacies. However, there is also evidence that PDMPs induced some individuals who would have otherwise abused prescription opioids to substitute towards cheaper alternatives, such as heroin (Balestra et al. 2021; Kim 2021).[3]

Though not studied as extensively as PDMPs, existing evidence indicates that state pill mill laws are highly effective at reducing excessive opioid prescribing (Rutkow et al. 2015; Chang et al. 2016; Lyapustina et al. 2016; Deiana and Giua 2018). 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), Kaestner and Ziedan (2023) found that state pill mill laws were associated with a 15-20 percent reduction in the volume of prescription opioids, compared to a more modest 5-10 percent reduction attributable to PDMPs. However, there is also

[3] This substitution is consistent with evidence that opioid abusers responded to the reformulation of OxyContin in August 2010 by substituting toward heroin (Alpert et al. 2018; Evans et al. 2019).

evidence that these laws induced some individuals who would have otherwise used prescription opioids to substitute towards heroin (Mallatt 2022).[4]

Several studies have linked opioid use – and the policies intended to prevent it – to changes in labor market and firm outcomes, though not necessarily among firms generating revenue through opioid sales. For example, prescription opioid use has been linked to lower rates of labor force participation (Harris et al. 2020; Aliprantis et al. 2023), business formation (Rietveld and Patel 2021), and firm performance (Kim et al. 2024; Langford and Feldman 2024). Relatedly, Beheshti (2023) found that the Drug Enforcement Agency's decision to elevate hydrocodone to a Schedule II Controlled Substance improved labor market outcomes in zip codes with higher baseline rates of hydrocodone use compared to those with lower use rates, and Kaestner and Ziedan (2023) found that state pill mill laws were associated with labor market improvements.

There is a smaller literature examining how supply-side drug interventions affect the firms producing and selling these products. Studying recreational marijuana legalization, Wang and Chan (2024) documented increases in downstream innovation and patenting (i.e., products for recreational cannabis users) without any changes in upstream innovation (i.e., chemical aspects and other factors related to medical use). For opioids, Nguyen et al. (2019) found that pharmaceutical companies responded to state must-access PDMP laws by reducing direct-to-physician advertising (i.e., physician detailing). In the study perhaps most comparable 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

[4] However, a working paper by Donahoe (2024) finds that the public health improvements attributable to reducing access to prescription opioids were not offset by any corresponding shift to alternative illicit substances.

management clinics – in the 2004-2015 Quarterly Census of Employment and Wages (QCEW) data.[5]

Building on prior well-executed work on this topic, our use of the NETS data allows us to improve on the prior literature in several important ways. First, we can examine changes in important outcomes that were unavailable to prior researchers, including retail pharmacy sales and employment. Second, the NETS data allow us to show that the increase in pharmacy closures was driven by standalone establishments while pharmacies with multiple locations were seemingly unaffected. Third, because we observe the same establishments over time, we are able to study the effects of these policies on both individual pharmacies and the industry as a whole. This proves to be an important contribution. While we show that state pill mill laws reduced the overall volume of sales and employment in the retail pharmacy industry by increasing establishment closures, we also find evidence that surviving firms experienced modest improvements in sales.

3. DATA AND METHODOLOGY

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,

[5] 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).

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. The NETS data have been used previously in studies such as 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. 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 1.3 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 the following difference-in-differences imputation estimator (Borusyak et al. 2024):

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

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). Our independent variable of interest, 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 is zero otherwise.

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). Given existing evidence linking changes in state marijuana policies to changes in opioid-related outcomes, Z also includes indicators for whether the state had a medical 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.[6]

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. Given the recent literature highlighting potential pitfalls of including earlier treated units in the comparison group for later treated units (de Chaisemartin & D'Haultfoeuille 2020; Callaway & Sant'Anna 2021; Goodman-Bacon 2021; Sun & Abraham 2021), our imputation estimator fits the state and year fixed effects using only untreated observations (Borusyak et al. 2024). These fixed effects are then used to impute the untreated potential outcomes for each observation which are then aggregated. This procedure assures that our coefficient of interest, β, is being identified from "clean" comparisons between treated and untreated units. 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 absence of the policy change – outcomes among pharmacies in states adopting pill mill laws would have evolved similarly to the outcomes among pharmacies in states not adopting pill mill laws. We explore the validity of this assumption with the following event-study specification:

$$Y_{isct} = \alpha + \sum_{j=-5,\, j\neq-1}^{4} \beta^j I^j + Z'_{isct}\gamma + \theta_s + \tau_t + \varepsilon_{isct} \quad (2)$$

where the coefficients, β^j, measure how the outcomes of interest differentially evolved in treated and never-treated states. Our first policy change occurred in 2005, so we can estimate 5 pre-periods and 14 post-periods for establishments in this state. The final state to adopt a pill mill law during

[6] 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).

our sample period, Wisconsin, did so in 2016, allowing us to estimate 16 pre-periods and 3 post-periods for this timing group. Together, this would imply that we could estimate a balanced state-year event window of 5 pre-periods and 3 post-periods to assure that our results are not being driven by changes in the states contributing to identification. To allow for a longer post-period, we drop Wisconsin from our event study analysis, allowing us to estimate a balanced state-year event window of 5 pre-periods and 5 post periods. However, we show in the appendix the robustness of the results to including Wisconsin and estimating the narrower event study window.

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 the market outcomes for retail pharmacies. 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). Panel A reports the results from our static difference-in-differences specification, while Panel B reports the results from our dynamic event-study specification. We find that state pill mill laws were associated with a 6.1 percent reduction in annual sales and a 3.2 percent reduction in the number of employees (Panel A), both of which are statistically significant at the 5 percent level.[7]

In the presence of our covariates and fixed effects, our identification assumption is that the outcomes of pharmacies in states adopting pill mill laws would have evolved similarly to the outcomes of pharmacies in states not adopting these laws. While untestable, Figure 2 assesses the validity of this parallel trends assumption by plotting estimates from the event-study specification

[7] Appendix Figure 1 shows how the estimates change when we iteratively exclude each treated state. Panel A shows larger sales reductions in specifications including Florida, which is consistent with evidence showing that there were a relatively large number of pill mill pain management clinics in Florida (Meinhofer 2018).

shown in equation (2). 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. In the years following the adoption of a pill mill law, we estimate pharmacies in adopting states experienced a 5.5-7.2 percent reduction in annual sales and a 3.9-4.6 percent reduction in the number of employees.[8,9]

In Table 4, we explore the robustness of the relationships between state pill mill laws and pharmacy market outcomes. The dependent variable in Panel A is the natural log of sales, and the dependent variable in Panel B is the natural log of the number of employees. Column 1 reprints our baseline results. Because state pill mill laws were adopted in southern and midwestern states, columns 2 and 3 further account for time-varying spatial heterogeneity. Our sales result is largely unchanged after including Census region-by-year fixed effects, though the estimate examining changes in the number of employees is no longer statistically significant. However, after including Census division-by-year fixed effects, we find that state pill mill laws were associated with a 7.9 percent reduction in sales and a 5.7 percent reduction in the number of employees, both of which are statistically significant at the 1 percent level. Prior work has shown that the NETS data may be less reliable for establishments with the smallest and largest number of employees (Neumark et al. 2007; Barnatchez et al. 2017), though column 4 shows that our results are robust to excluding establishments in the bottom and top five percent of the employee distribution.[10] Finally, column 5

[8] We detect more modest reductions in the exact year of adoption, which is consistent with the fact that most of the policies were enacted mid-year, and our outcomes are measured annually.
[9] 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 2 that the results are unchanged when including Wisconsin and estimating a shorter post-period.
[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

reports results using a traditional two-way fixed effects estimator. While the estimates are smaller in magnitude, as one would expect when comparing newly treated and previously treated states when the treatment effect grows over time, we continue to find statistically significant reductions in annual sales and the number of pharmacy employees.

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. 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).[11] 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 the robustness to using 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." Similarly, we classify establishments 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

because the NETS has higher coverage of smaller establishments. Appendix Table 2 reports results where we exclude the bottom and top 5 percent of the distribution (i.e., we retain establishments with 3-39 employees) and where we exclude the bottom and top 10 percent of the distribution (i.e., we retain establishments with 4-29 employees). We continue to find a statistically significant 6.1-6.7 percent reduction in sales and a 3.4-4.0 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. Again, Appendix Table 3 shows that the results are robust to excluding these establishments.

[11] 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).

of the distribution (i.e., those with 20 or more nearby establishments) as being in a "high-competition area."

In Table 5 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 6.1 percent reduction in sales and a 3.2 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 2.4 percent reduction in sales and a 0.7 percent reduction 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 6.4 percent reduction in sales and a 3.1 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 an 8.4 percent reduction in sales and a 4.5 percent reduction in the number of employees.

Overall, Table 5 indicates that pharmacies facing more competitive pressure experienced the largest reductions in sales and employment following the adoption of a state pill mill law. This finding is robust to alternative ways of defining competitive pressure. For example, in Appendix Table 4 we find similar results when defining competition based on the total sales volume from other pharmacies within a 5,000-meter radius, rather than basing it on the number of nearby establishments. We also document a similar pattern in Appendix Table 5 when we increase the radius to 10,000 meters (~6.2 miles). Likewise, we continue to find 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) in Appendix Table 6. However, it is worth noting that the relationship between state pill mill laws and the outcomes of pharmacies in high competition areas grows in absolute magnitude when using this smaller radius, which is consistent with Janssen and Zhang's (2023) finding that pharmacies with a competitor within one or two miles were more likely to dispense OxyContin and other prescription opioids, seemingly for drug diversion.[12]

One benefit of the NETS data is that we observe the same establishments over time, so in Table 6 we test how our estimates change when including increasingly more granular levels of geographic fixed effects. All columns include location-invariant year fixed effects and our time-varying policy, economic, and demographic controls. Columns 1 and 2 include state fixed effects, columns 3 and 4 include county fixed effects, and columns 5 and 6 include establishment fixed effects. Our results remain largely unchanged after including county fixed effects. Interestingly, though, the direction of the effect changes sign after including establishment fixed effects. 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 1.3 percent increase in the number of employees. What might explain this change? Estimates including establishment fixed effects are identified off of within-establishment changes over time among those establishments that remained 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 benefitted from these laws.

[12] Janssen and Zhang (2023) estimate the effect competition on opioid dispensing using nine different radii (see Figure 6 on page 26). While the authors find large increases when pharmacies face an additional competitor within one or two miles, the estimates largely converge when the radius is increased beyond four miles.

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 the number of establishment openings and closures. The dependent variables in Table 7 are the natural log of the number of county-level pharmacy openings + 1 (column 1) and the natural log of the number of county-level pharmacy closures + 1 (column 2). Panel A reports the results from our static difference-in-differences specification, while Panel B reports the results from our dynamic event-study specification. We also plot these event-study estimates in Figure 3.

We do not find any evidence that state pill mill laws were associated with changes in pharmacy openings in either the pre-period or the post-period. Nor is there strong evidence of a differential pre-trend in pharmacy closures, though we do estimate a statistically insignificant increase in closures two years prior to adoption. Overall, the pre-period estimates are smaller in magnitude, inconsistently signed, and not statistically significant in all years, including the year prior to adoption. However, following the adoption of a state pill mill law, we find an increase in the number of pharmacy closures.[13] Summarizing these changes using the static difference-in-differences specification from equation (1), we find that state pill mill laws were associated with a 5.1 percent increase in the number of pharmacy closures. We show in Appendix Figure 4 that these patterns are robust to iteratively excluding each of the treated states. Likewise, we show in

[13] The event studies exclude observations from Wisconsin to allow for a long post-period with a balanced state-year event window. We show in Appendix Figure 3 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.

Appendix Table 7 that the results are robust to alternative controls for spatial heterogeneity, sample restrictions, and difference-in-differences estimators.[14]

There is evidence that independent pharmacies were more likely than chain pharmacies to dispense excessive quantities of prescription opioids. For example, a bipartisan Congression 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. Drug Enforcement Agency, Janssen and Zhang (2023) showed that (i) independent pharmacies dispensed approximately 39 percent more opioids and 61 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).[15] We leverage this in Table 8 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 6.2 percent increase in the number of standalone pharmacy closures. In contrast, column 4 indicates that the relationship for non-standalone pharmacies was over 70 percent smaller in magnitude, opposite signed, and statistically

[14] 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 8 for completeness.

[15] We also explored heterogeneity in sales and employment by standalone status. While the results were generally inconclusive, we report them in Appendix Table 9 for completeness.

insignificant.[16] Collectively, these results suggest that state pill mill laws influenced the retail pharmacy market by increasing the number of standalone pharmacy closures.[17]

5. CONCLUSION

This paper provides new evidence on how public policies limiting the sale of goods posing 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 6 percent reduction in pharmacy sales and a 3 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

[16] Event study estimates, shown in Appendix Figure 5, confirm that the increase in pharmacy closures was limited to standalone pharmacies in the post-period. Meanwhile, Appendix Table 10 shows that the 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).

[17] Appendix Table 11 offers suggestive evidence that state pill mill laws were associated with increased closures among standalone pharmacies in more competitive areas.

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 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).

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 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 replaced with revenue increases for those supplying prescription opioid alternatives, such as heroin and fentanyl. However, prior evidence has connected changes in prescription opioid access to changes in the illicit drug market (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.

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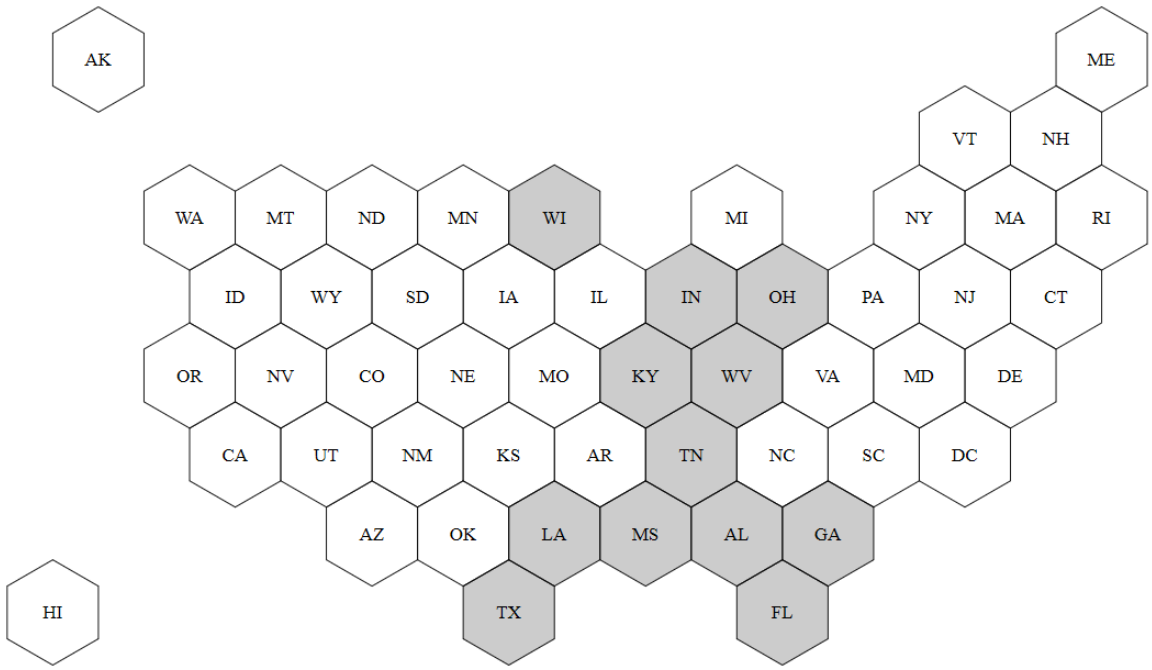
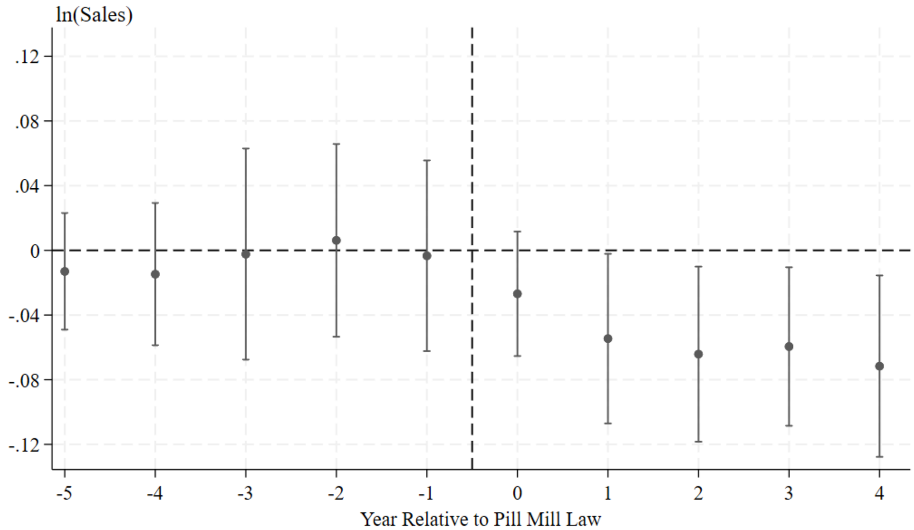
Figure 1: Geographic Variation in State Pill Mill Laws

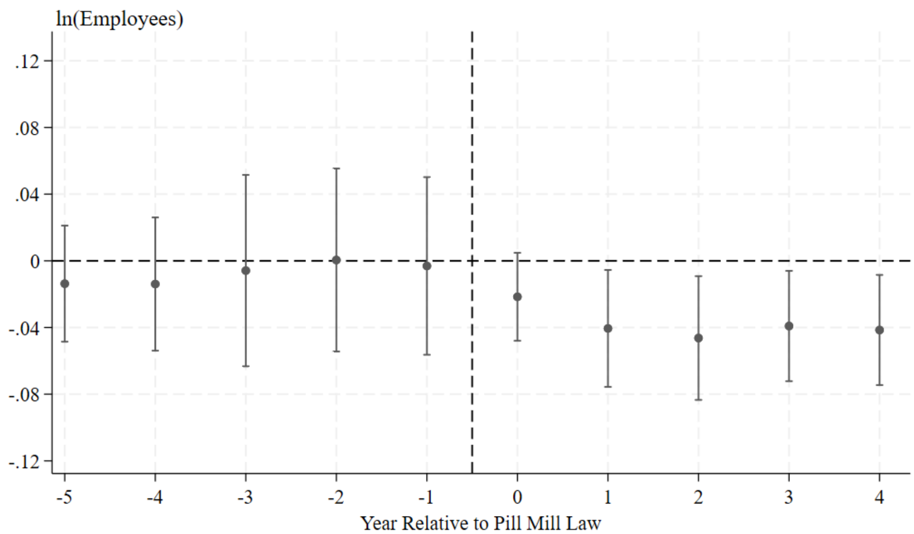


Source: National Establishment Time-Series, 2000-2018
Note: The shaded states indicate states that adopted pill mill laws during our sample period.

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



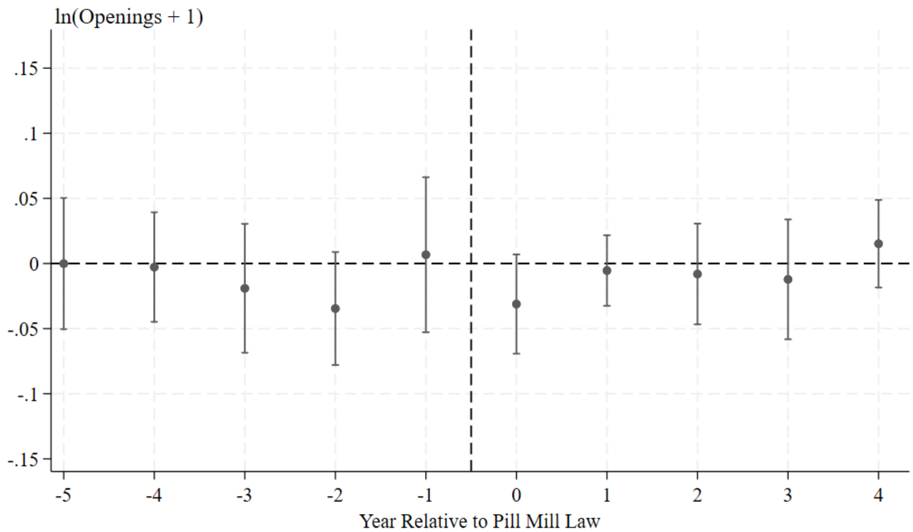
(A) Pharmacy Sales



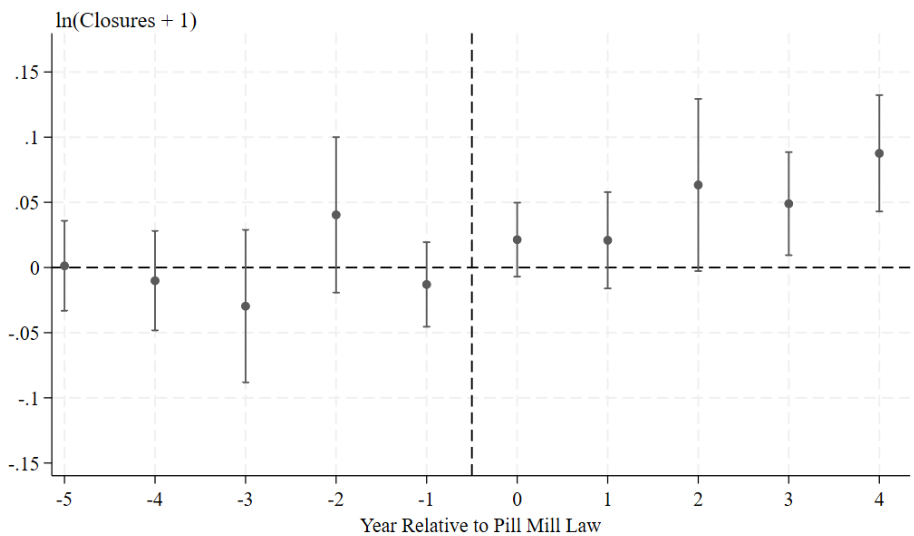
(B) Pharmacy Employees

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, shown in equation (2). 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. Figures reporting a shorter post-period that include Wisconsin are shown in Appendix Figure 2. 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 the natural log of the number of county-level pharmacy openings + 1, while the dependent variable in Panel B is the natural log of the number of county-level pharmacy closures + 1. The figures plot the estimates from the event-study specification, shown in equation (2). 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. Figures reporting a shorter post-period that include 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 (2020), 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	\$3,597,599	\$3,283,651	\$3,765,428	t = 20.84
	(\$11,818,771)	(\$10,777,904)	(\$12,335,962)	p < 0.001
Employees	13.14	12.28	13.60	t = 15.81
	(42.73)	(40.76)	(43.74)	p < 0.001
Observations	1,150,789	400,882	749,907	1,150,789
Panel B: County-Level Outcomes				
Openings	1.44	1.36	1.49	t = 2.32
	(6.74)	(6.29)	(7.00)	p = 0.02
Closures	1.02	1.05	0.99	t = 1.64
	(4.70)	(4.74)	(4.63)	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 which adopted a pill mill law during the sample period, and column 3 limits the sample to states which 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)
Panel A: Static Difference-in-Differences		
Pill Mill Law	-0.061**	-0.032**
	(0.024)	(0.015)
Observations	1,150,789	1,150,789
Panel B: Event-Study Estimates		
5 Years Before	-0.013	-0.014
	(0.018)	(0.018)
4 Years Before	-0.015	-0.014
	(0.022)	(0.020)
3 Years Before	-0.002	-0.006
	(0.033)	(0.029)
2 Years Before	0.006	0.000
	(0.030)	(0.028)
1 Year Before	-0.003	-0.003
	(0.030)	(0.027)
Policy Change	-0.027	-0.022
	(0.020)	(0.013)
1 Year After	-0.055**	-0.041**
	(0.027)	(0.018)
2 Years After	-0.064**	-0.046**
	(0.028)	(0.019)
3 Years After	-0.059**	-0.039**
	(0.025)	(0.017)
4 Years After	-0.072**	-0.042**
	(0.029)	(0.017)
Observations	1,134,281	1,134,281

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, shown in equation (1), while Panel B reports estimates obtained from the event-study specification, shown in equation (2). To allow for a longer post-period, Panel B excludes observations from Wisconsin. Event studies including Wisconsin are reported in Appendix Figure 2. Standard errors, shown in parentheses, are clustered at the state level.

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

Table 4: 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	-0.061**	-0.062***	-0.079***	-0.061**	-0.047*
	(0.024)	(0.022)	(0.022)	(0.024)	(0.024)
Observations	1,150,789	1,150,789	1,150,789	1,016,333	1,150,789
Panel B: Dependent Variable is ln(Employees)					
Pill Mill Law	-0.032**	-0.024	-0.057***	-0.034**	-0.029*
	(0.015)	(0.015)	(0.017)	(0.013)	(0.015)
Observations	1,150,789	1,150,789	1,150,789	1,016,333	1,150,789

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, shown in equation (1). 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 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.

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

Table 5: The Relationship Between State Pill Mill Laws and Reductions in Retail Pharmacy Outcomes Was More Pronounced in More Competitive Areas

	(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	-0.061**	-0.024	-0.064***	-0.084***
	(0.024)	(0.021)	(0.024)	(0.026)
Observations	1,150,789	293,198	573,090	284,501
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.032**	-0.007	-0.031*	-0.045**
	(0.015)	(0.015)	(0.017)	(0.022)
Observations	1,150,789	293,198	573,090	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. Column 1 reports the estimates from our baseline difference-in-differences specification, shown in equation (1). 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 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	-0.061**	-0.032**	-0.063***	-0.025*	0.012	0.013***
	(0.024)	(0.015)	(0.022)	(0.013)	(0.014)	(0.004)
Observations	1,150,789	1,150,789	1,150,789	1,150,789	1,150,789	1,150,789
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 use the difference-in-differences specification from equation (1) that includes state fixed effects, year fixed effects, and additional state- and county-level time-varying covariates. Columns 3 and 4 replace the state fixed effects with county fixed effects. Finally, columns 5 and 6 replace the county fixed effects with establishment-level fixed effects, such that the relationships are identified off within establishment changes over time. 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 Increases in Pharmacy Closures

	(1)	(2)
Outcome →	ln(Openings + 1)	ln(Closures + 1)
Panel A: Static Difference-in-Differences		
Pill Mill Law	-0.004	0.051***
	(0.014)	(0.018)
Observations	59,668	59,668
Panel B: Event-Study Estimates		
5 Years Before	-0.000	0.001
	(0.026)	(0.018)
4 Years Before	-0.003	-0.010
	(0.021)	(0.019)
3 Years Before	-0.019	-0.030
	(0.025)	(0.030)
2 Years Before	-0.035	0.040
	(0.022)	(0.030)
1 Year Before	0.007	-0.013
	(0.030)	(0.017)
Policy Change	-0.031	0.021
	(0.019)	(0.014)
1 Year After	-0.005	0.021
	(0.013)	(0.019)
2 Years After	-0.008	0.063*
	(0.020)	(0.034)
3 Years After	-0.012	0.049**
	(0.023)	(0.020)
4 Years After	0.015	0.088***
	(0.017)	(0.023)
Observations	58,300	58,300

Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the number of county-level pharmacy openings + 1, while the dependent variable in column 2 is the natural log of the number of county-level pharmacy closures + 1. Panel A reports the estimates from the static difference-in-differences specification, shown in equation (1), while Panel B reports the estimates from the event-study specification, shown in equation (2). To allow for a longer post-period, Panel B excludes observations from Wisconsin. Event studies including Wisconsin are reported in Appendix Figure 3. Standard errors, shown in parentheses, are clustered at the state level.

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

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

	(1)	(2)	(3)	(4)
	Standalone Pharmacies		Non-Standalone Pharmacies	
	ln(Openings + 1)	ln(Closures + 1)	ln(Openings + 1)	ln(Closures + 1)
Pill Mill Law	-0.001	0.062***	-0.001	-0.014
	(0.010)	(0.019)	(0.014)	(0.012)
Observations	59,668	59,668	59,668	59,668

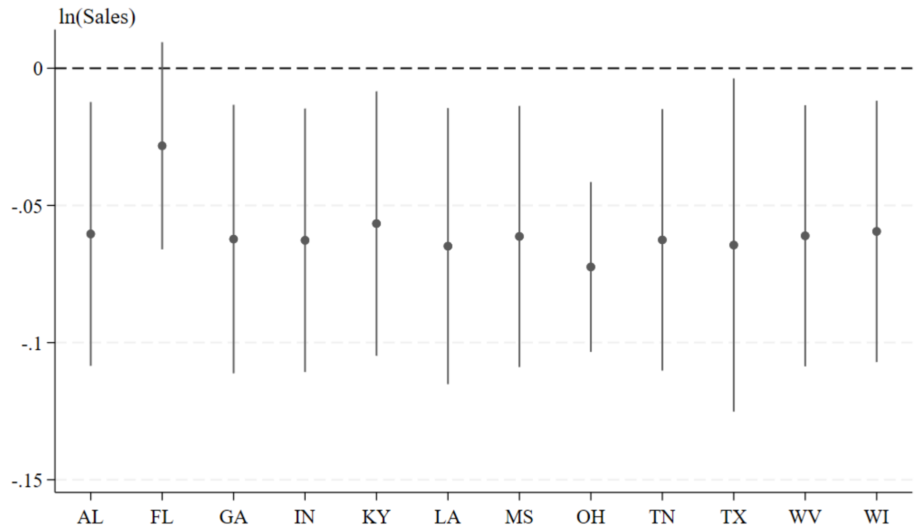
Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in column 1 is the natural log of the number of county-level standalone pharmacy openings + 1, the dependent variable in column 2 is the natural log of the number of county-level standalone pharmacy closures + 1, the dependent variable in column 3 is the natural log of the number of county-level non-standalone pharmacy openings + 1, and the dependent variable in column 4 is the natural log of the number of county-level non-standalone pharmacy closures + 1. The estimates are obtained using the difference-in-differences specification, shown in equation (1). Standard errors, shown in parentheses, are clustered at the state level.

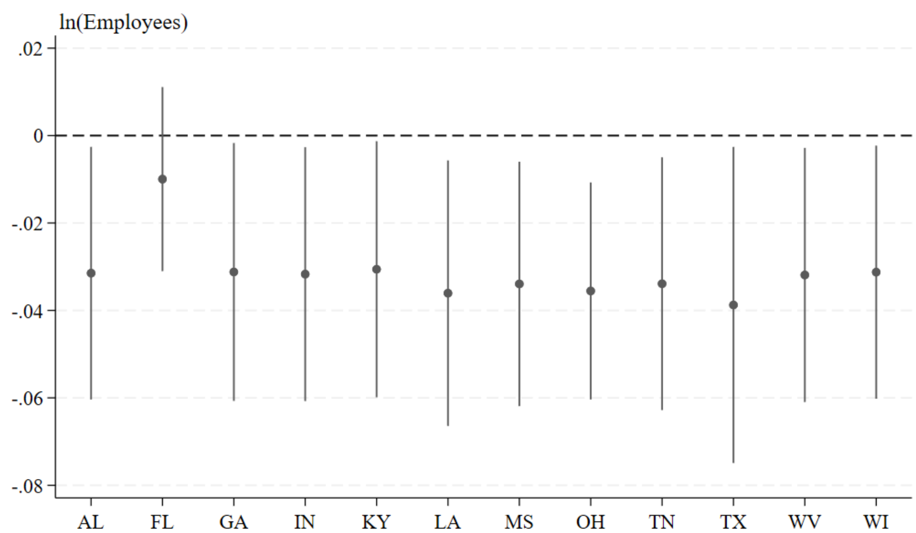
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

7. APPENDIX

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



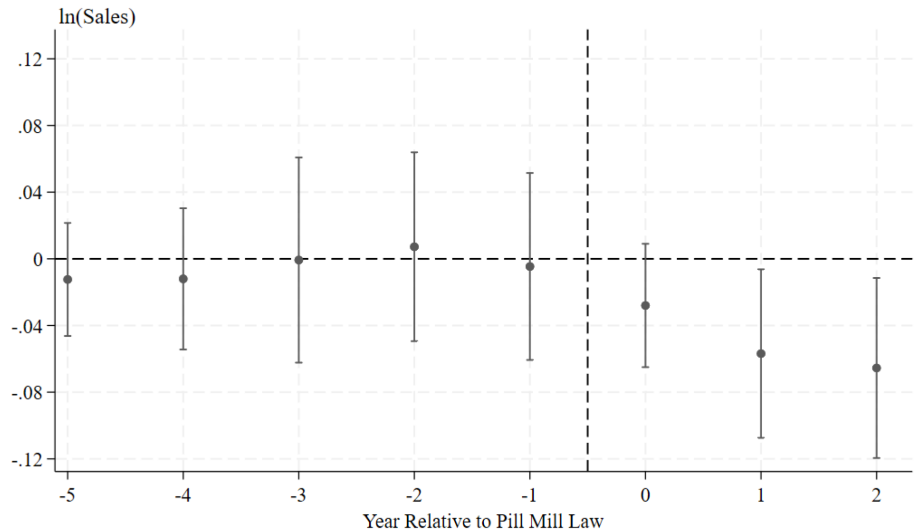
(A)



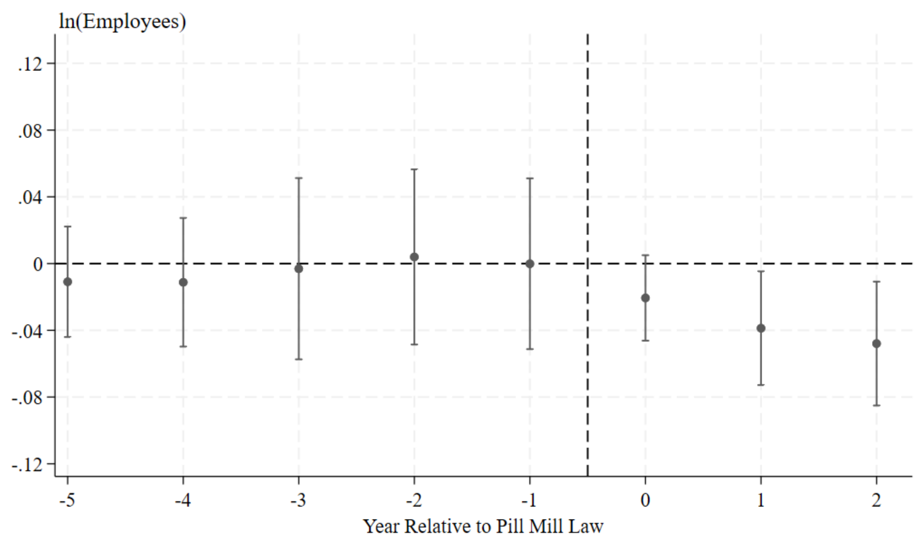
(B)

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, shown in equation (1). 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: Event Study Estimates Including Wisconsin That Examine Pharmacy Sales and Employment



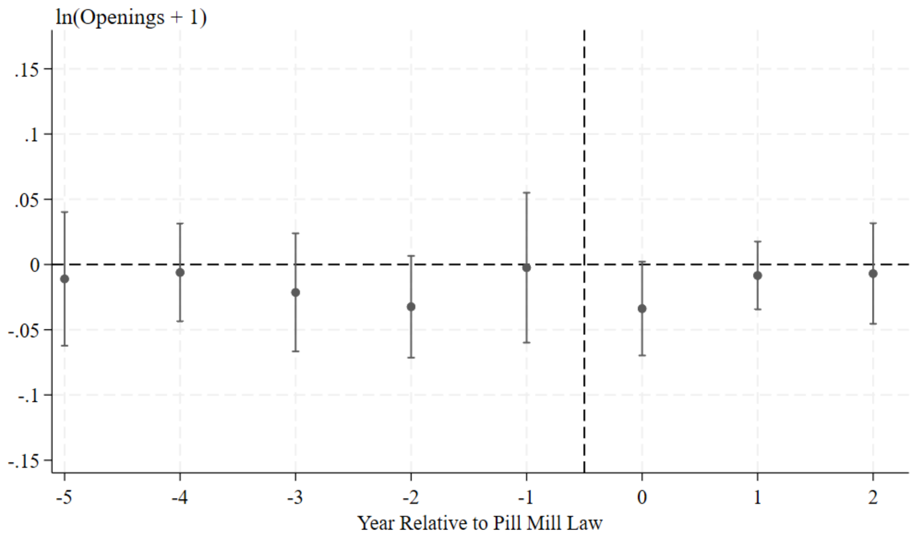
(A) Pharmacy Sales



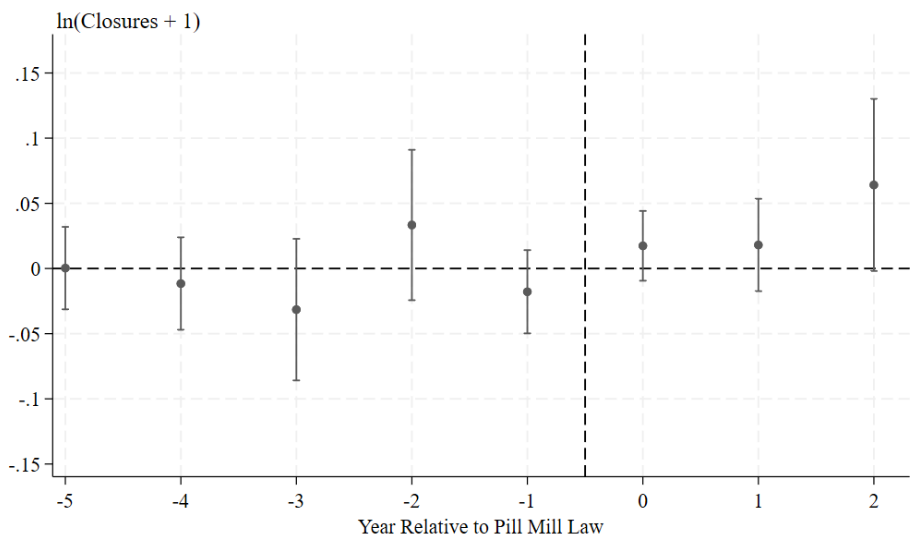
(B) Pharmacy Employees

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, shown in equation (2). 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 3: Event Study Estimates Including Wisconsin That Examine Pharmacy Openings and Closures



(A) Pharmacy Openings

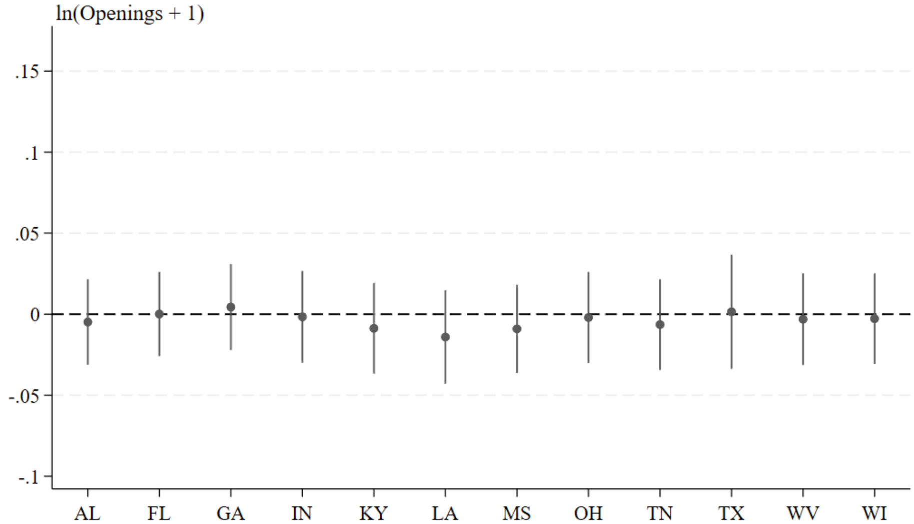


(B) Pharmacy Closures

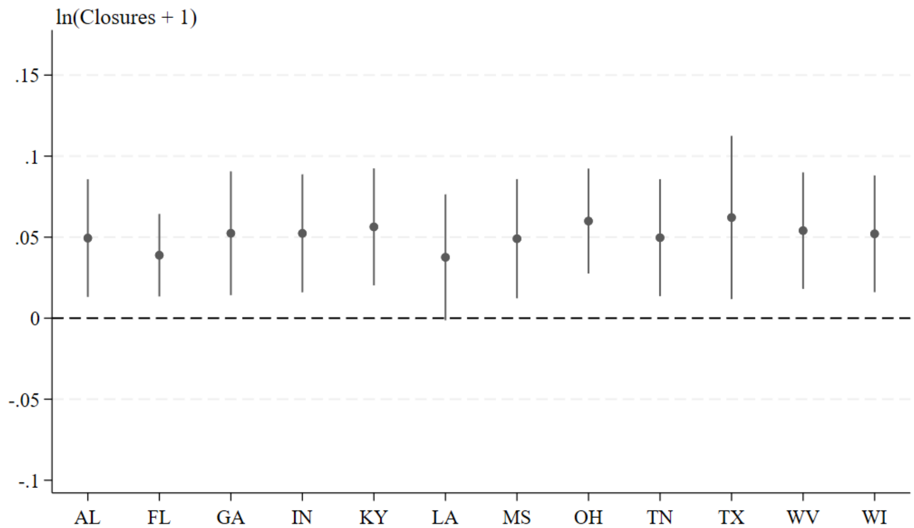
Source: National Establishment Time-Series, 2000-2018

Note: The dependent variable in Panel A is the natural log of the number of county-level pharmacy openings + 1, while the dependent variable in Panel B is the natural log of the number of county-level pharmacy closures + 1. The figures plot the estimates from the event-study specification, shown in equation (2). 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: The Relationship Between State Pill Mill Laws and Pharmacy Openings and Closures When Iteratively Excluding Each Treated State



(A)

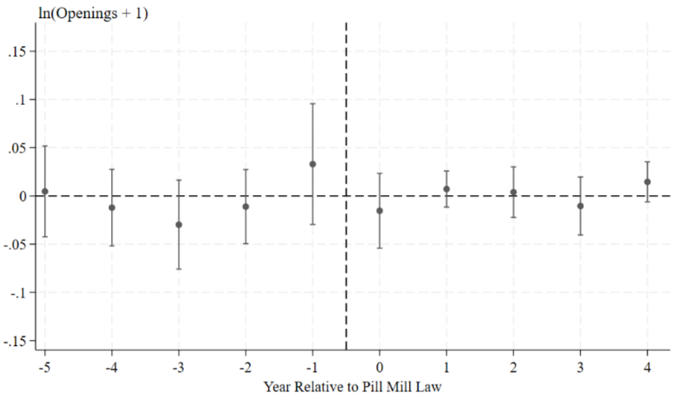


(B)

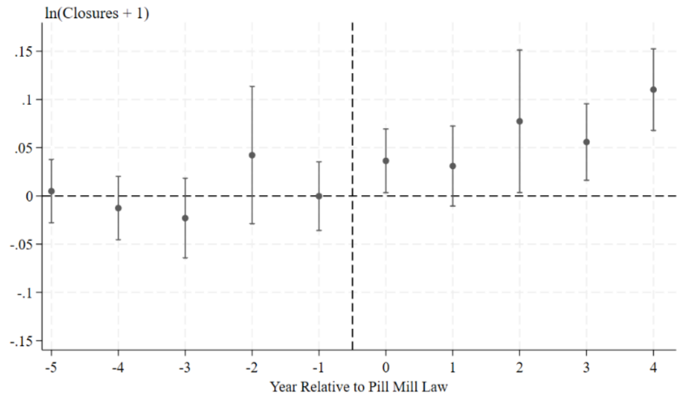
Source: National Establishment Time-Series, 2000-2018

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, shown in equation (1). 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.

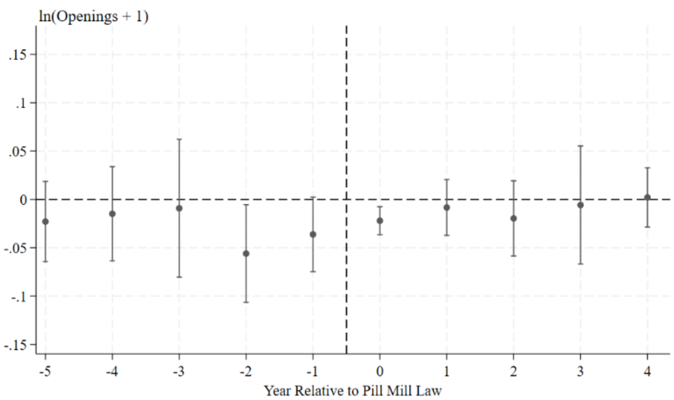
Appendix Figure 5: Standalone Pharmacy Closures Increased After the Adoption of a State Pill Mill Law



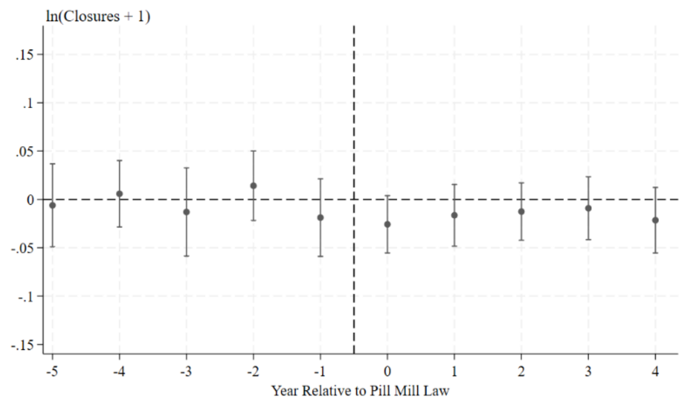
(A) Standalone Openings



(B) Standalone Closures



(C) Non-Standalone Openings



(D) Non-Standalone Closures

Source: National Establishment Time-Series, 2000-2018
Note: The dependent variable in Panel A is the natural log of the number of county-level standalone pharmacy openings + 1, while the dependent variable in Panel B is the natural log of the number of county-level standalone pharmacy closures + 1. The dependent variable in Panel C is the natural log of the number of county-level non-standalone pharmacy openings + 1, while the dependent variable in Panel D is the natural log of the number of county-level non-standalone pharmacy closures + 1. The figures plot the estimates from the event-study specification, shown in equation (2). 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	0.808	0.769	0.828
	(0.394)	(0.422)	(0.377)
Must Access PDMP	0.112	0.112	0.111
	(0.315)	(0.316)	(0.315)
Medical Marijuana Law	0.331	0.070	0.471
	(0.471)	(0.256)	(0.499)
Recreational Marijuana Law	0.041	0.000	0.027
	(0.198)	-	(0.162)
Active Medical Dispensaries	0.233	0.042	0.117
	(0.423)	(0.201)	(0.072)
Active Recreational Dispensaries	0.018	0.000	0.027
	(0.132)	-	(0.162)
Unemployment Rate	6.114	6.108	6.117
	(2.077)	(1.950)	(2.142)
ln(Unemployment Claims)	13.157	13.047	13.215
	(0.996)	(0.660)	(1.131)
ln(Residential Permits)	15.574	15.839	15.432
	(1.066)	(1.126)	(1.004)
ln(State Product Per Capita)	10.955	10.849	11.012
	(0.184)	(0.131)	(0.183)
ln(Minimum Wage)	2.061	2.009	2.089
	(0.117)	(0.076)	(0.125)
Percent Black	0.136	0.171	0.117
	(0.081)	(0.086)	(0.072)
Percent Hispanic	0.151	0.151	0.152
	(0.122)	(0.135)	(0.115)
ln(County Population)	12.804	12.465	12.985
	(1.689)	(1.619)	(1.697)

Appendix Table 2: 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	-0.061**	-0.067***	-0.034**	-0.040***
	(0.024)	(0.024)	(0.013)	(0.013)
Observations	1,016,333	909,967	1,016,333	909,967

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, shown in equation (1). Columns 1 and 3 exclude establishments in the bottom and top five percent based on employment (i.e., evaluating only establishments with 3-39 employees). Columns 2 and 4 exclude establishments in the bottom and top 10 percent based on employment (i.e., evaluating only 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 3: Robustness Tests Excluding Establishments with Fewer than 5 Employees

	(1)	(2)
Outcome →	ln(Sales)	ln(Employees)
Pill Mill Law	-0.051**	-0.036*
	(0.024)	(0.020)
Observations	809,721	809,721

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, shown in equation (1). 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 4: 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	-0.061**	-0.029	-0.064**	-0.010***
	(0.024)	(0.018)	(0.027)	(0.032)
Observations	1,150,789	287,698	575,394	287,697
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.032**	-0.014	-0.031*	-0.056**
	(0.015)	(0.016)	(0.017)	(0.026)
Observations	1,150,789	287,698	575,394	287,697

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, shown in equation (1). 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 5: 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	-0.061**	-0.014	-0.072***	-0.087***
	(0.024)	(0.020)	(0.023)	(0.028)
Observations	1,150,789	305,031	561,318	284,440
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.032**	-0.002	-0.034**	-0.047*
	(0.015)	(0.014)	(0.016)	(0.025)
Observations	1,150,789	305,031	561,318	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, shown in equation (1). 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 6: 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	-0.061**	-0.032	-0.077***	-0.095***
	(0.024)	(0.024)	(0.026)	(0.028)
Observations	1,150,789	305,031	561,318	284,440
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	-0.032**	-0.008	-0.034**	-0.070***
	(0.015)	(0.021)	(0.016)	(0.018)
Observations	1,150,789	305,031	561,318	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, shown in equation (1). 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 7: Robustness Tests Examining the Effects of State Pill Mill Laws on Pharmacy Openings and Closures

	(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(Openings + 1)					
Pill Mill Law	-0.004	-0.004	-0.019*	-0.004	-0.005
	(0.014)	(0.018)	(0.011)	(0.013)	(0.013)
Observations	59,668	59,668	59,668	59,668	59,668
Panel B: Dependent Variable is ln(Closures + 1)					
Pill Mill Law	0.051***	0.041**	0.021	0.048**	0.045**
	(0.018)	(0.020)	(0.015)	(0.019)	(0.019)
Observations	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 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, shown in equation (1). 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. 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 8: 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: Dependent Variable is ln(Openings + 1)				
Pill Mill Law	-0.004	0.000	-0.006	0.002
	(0.014)	(0.011)	(0.014)	(0.006)
Observations	59,668	59,668	59,668	59,668
Panel B: Dependent Variable is ln(Closures + 1)				
Pill Mill Law	0.051***	0.007	0.014	0.007
	(0.018)	(0.008)	(0.009)	(0.007)
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, shown in equation (1). 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 9: 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	0.004	0.014	0.003	0.010
	(0.012)	(0.009)	(0.022)	(0.021)
Observations	636,056	636,056	514,733	514,733
Panel B: County FE, Year FE, and Additional Covariates				
Pill Mill Law	0.009	0.018	-0.001	0.012
	(0.011)	(0.010)	(0.023)	(0.018)
Observations	636,056	636,056	514,733	514,733
Panel C: Establishment FE, Year FE, and Additional Covariates				
Pill Mill Law	0.017***	0.024***	0.006	0.002
	(0.006)	(0.005)	(0.019)	(0.007)
Observations	636,056	636,056	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 obtained using the difference-in-differences specification in equation (1). Panel B augments this specification with county fixed effects, and Panel C further includes 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 10: 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	
	ln(Openings)	ln(Closures)	ln(Openings)	ln(Closures)
Panel A: Replace State Fixed Effects with County Fixed Effects				
Pill Mill Law	-0.002	0.057***	-0.001	-0.012
	(0.009)	(0.017)	(0.013)	(0.012)
Observations	59,668	59,668	59,668	59,668
Panel B: Replace the Natural Log with the Inverse Hyperbolic Sine				
Pill Mill Law	-0.001	0.078***	-0.002	-0.018
	(0.012)	(0.023)	(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	0.021	0.325***	-0.003	-0.021
	(0.050)	(0.046)	(0.027)	(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 obtained using the difference-in-differences specification from equation (1). 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 11: 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: Dependent Variable is ln(Openings + 1)						
Pill Mill Law	-0.002	-0.002	0.003	0.002	-0.004	0.003
	(0.007)	(0.011)	(0.006)	(0.007)	(0.011)	(0.003)
Observations	59,668	59,668	59,668	59,668	59,668	59,668
Panel B: Dependent Variable is ln(Closures + 1)						
Pill Mill Law	0.006	0.017**	0.011	0.001	-0.000	0.006
	(0.006)	(0.007)	(0.007)	(0.006)	(0.009)	(0.003)
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 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, shown in equation (1). 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.

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