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Supply-side opioid restrictions and the retail pharmacy market☆

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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.

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.

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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 (National Center for Health Statistics, 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-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 Normand, 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 Sebastian 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;

[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.

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 (Centers for Disease Control and Prevention, 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. 2015).

During our sample period, 12 states adopted a pill mill law; we report the states and adoption years in Table 1.[3] Fig. 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 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

[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 ($\widehat{\beta} = -0.022$ and SE $= 0.014$ in Table 4 column 7; $\widehat{\beta} = -0.029$ and SE $= 0.022$ in Table 5 column 7), she found a marginally significant *increase* when using 2004-2015 County Business Patterns data ($\widehat{\beta} = 0.018$ and SE $= 0.009$ in Table A2 column 7).

[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.

[5] Appendix Table 1 reports summary statistics for the covariates.

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 Public Act 257, 2013 Georgia Act 128, 2013 Indiana Senate Enrolled Act 246, 2013, and 2015 Wisconsin Act 265 (2015).

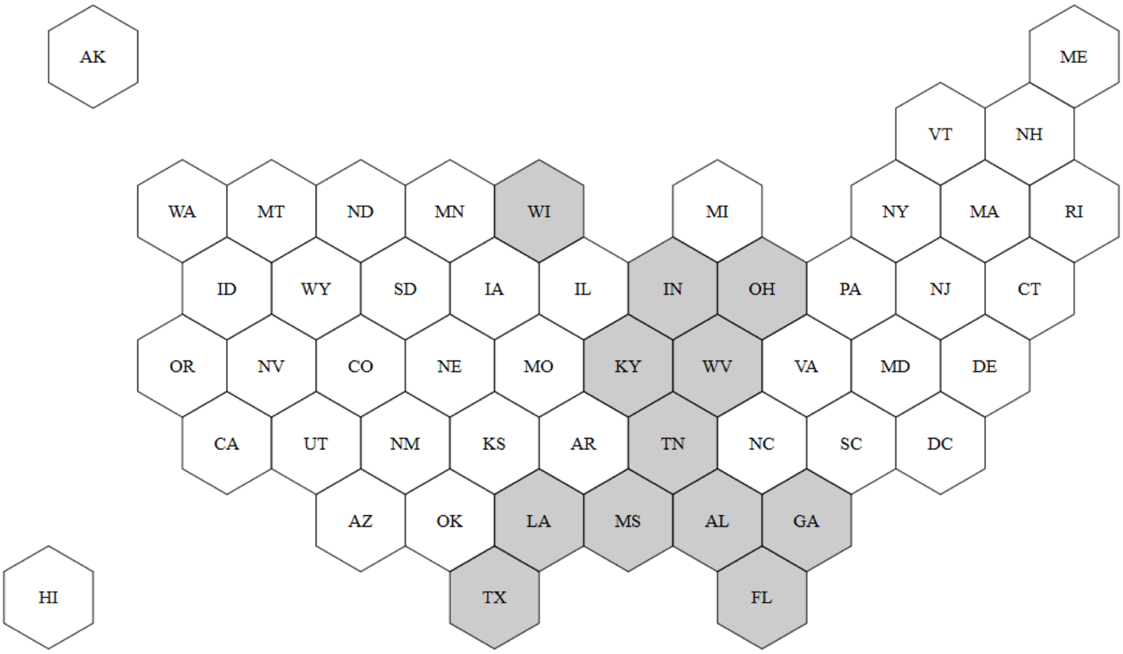

Fig. 1. Geographic Variation in State Pill Mill Laws
Note: The shaded states indicate states that adopted pill mill laws during our sample period.
Source: National Establishment Time-Series, 2000–2018

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

$$Y_{isct} = \alpha + \beta \cdot \mathrm{PILLMILLLAW}_{st} + Z_{sct}{}'\gamma + \theta_s + \tau_t + \varepsilon_{isct} \tag{1}$$

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). Meanwhile, the 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 zero in all other years. However, recent econometric

Table 2
Summary Statistics.

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

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.

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 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) 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

[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).

[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).

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 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]

Fig. 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 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

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[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.

[9] Appendix Fig. 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.

[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 Fig. 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 Rambachan and Roth (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 Fig. 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 Fig. 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.

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

Outcome →	(1) ln(Sales)	(2) ln(Employees)
Pill Mill Law	−0.053**	−0.027*
	(0.024)	(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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 (Centers for Medicare and Medicaid Services, 2006).[15] As such, for each pharmacy we tabulate the number of other pharmacies located within a 5000-meter radius (~3.1 miles), and we explore 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]

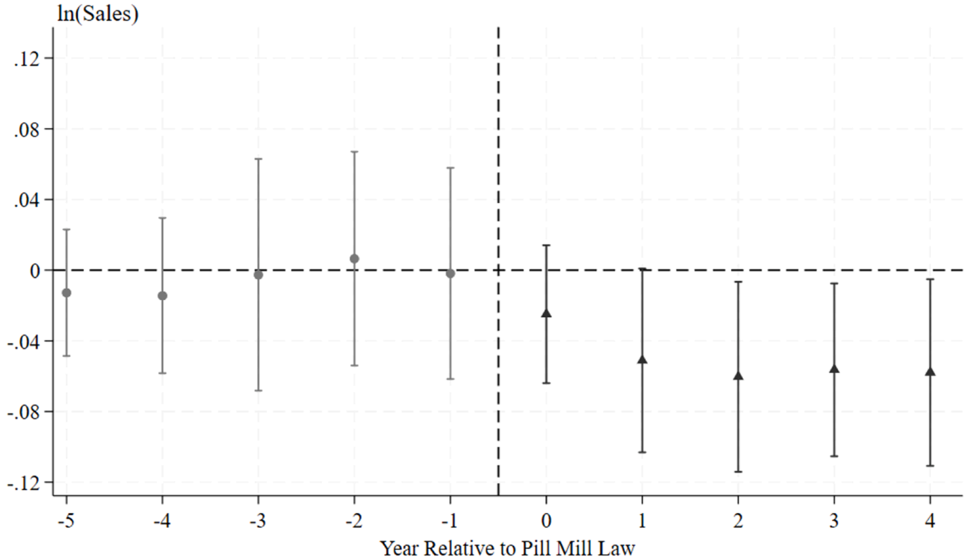
While the evidence indicates that pharmacies located within states adopting pill mill laws experienced a reduction in sales and employment, these policies may have benefited nearby pharmacies in states not adopting a pill mill law (Hodges and Shakya 2025). Indeed, prior evidence indicates that individuals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

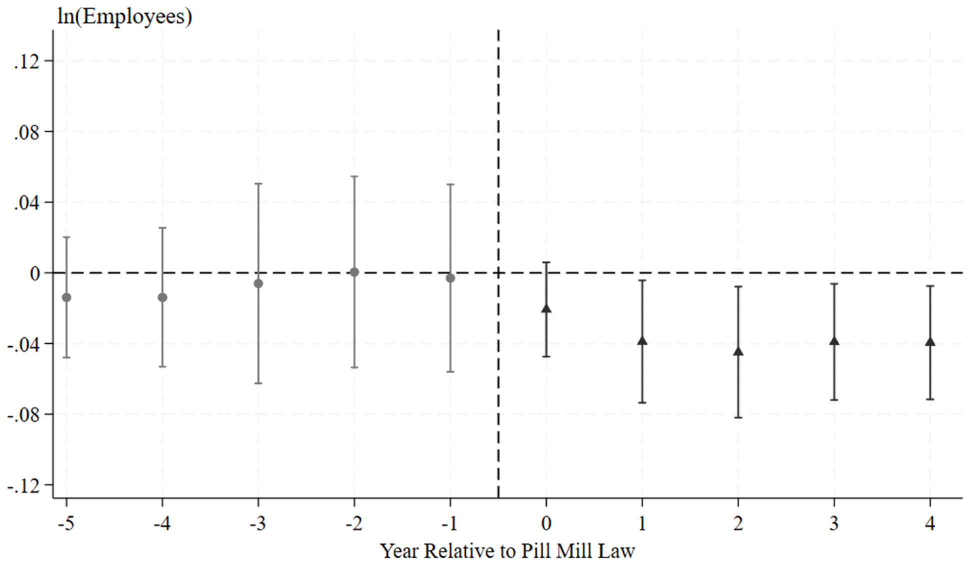
[15] Researchers have examined the importance of the number of competitors within a given radius (Janssen and Zhang 2023), the distance between a pharmacy and its five closest competitors (Chen 2019), and patients' travel times to preferred and in-network pharmacies (Starc and Swanson 2021).

[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).

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



(A) Pharmacy Sales



(B) Pharmacy Employees

Fig. 2. Pharmacy Sales and Employment Fell Following the Adoption of a State Pill Mill Law
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 Fig. 3. Standard errors are clustered at the state level.
Source: National Establishment Time-Series, 2000–2018

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

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

Sample →	(1) Full Sample	(2) Low-Competition Area	(3) Moderate-Competition Area	(4) High-Competition Area
Panel A: Dependent Variable is ln(Sales)				
Pill Mill Law	−0.053**	−0.013	−0.059**	−0.071***
	(0.024)	(0.021)	(0.024)	(0.026)
Observations	1,150,783	293,198	573,084	284,501
Panel B: Dependent Variable is ln(Employees)				
Pill Mill Law	−0.027*	0.000	−0.027	−0.035
	(0.015)	(0.016)	(0.018)	(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 5000 m 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**	0.087***
	(0.028)	(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.

Outcome →	(1) ln(Sales)	(2) ln(Employees)	(3) ln(Sales)	(4) ln(Employees)	(5) ln(Sales)	(6) ln(Employees)
Pill Mill Law	−0.053**	−0.027*	−0.055**	−0.017	0.012	0.014***
	(0.024)	(0.015)	(0.021)	(0.013)	(0.015)	(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.

Outcome →	(1) County Had a Pharmacy Opening	(2) County Had a Pharmacy Closure
Pill Mill Law	0.002	0.037***
	(0.009)	(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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 Fig. 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 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.[19,200] We show in Appendix Fig. 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. 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.

[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.

[19] Appendix Fig. 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 Fig. 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.

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]

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 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 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,

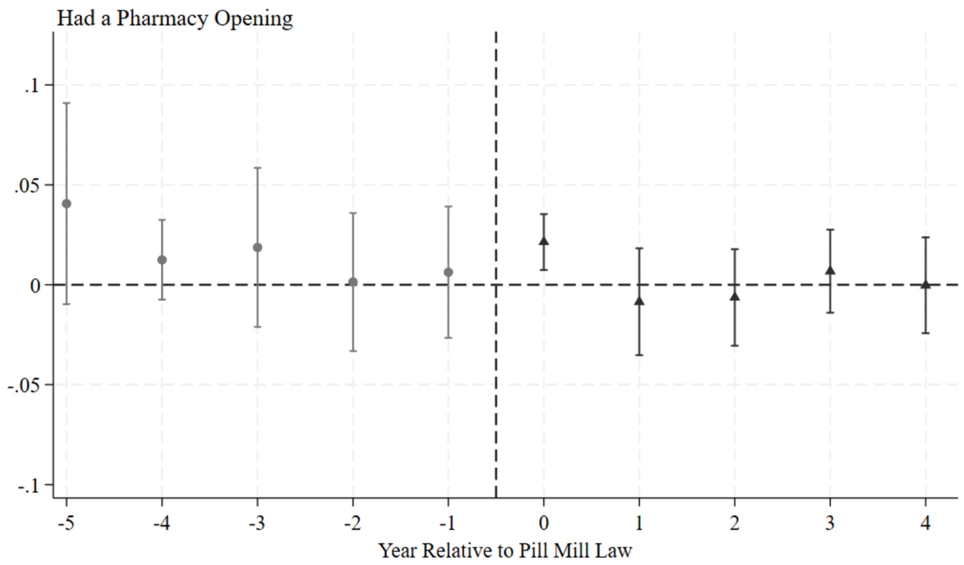
[22] We also explored heterogeneity in sales and employment by standalone status. While the results were generally inconclusive, we report them in Appendix Table 14 for completeness.

[23] Event-study estimates, shown in Appendix Fig. 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).

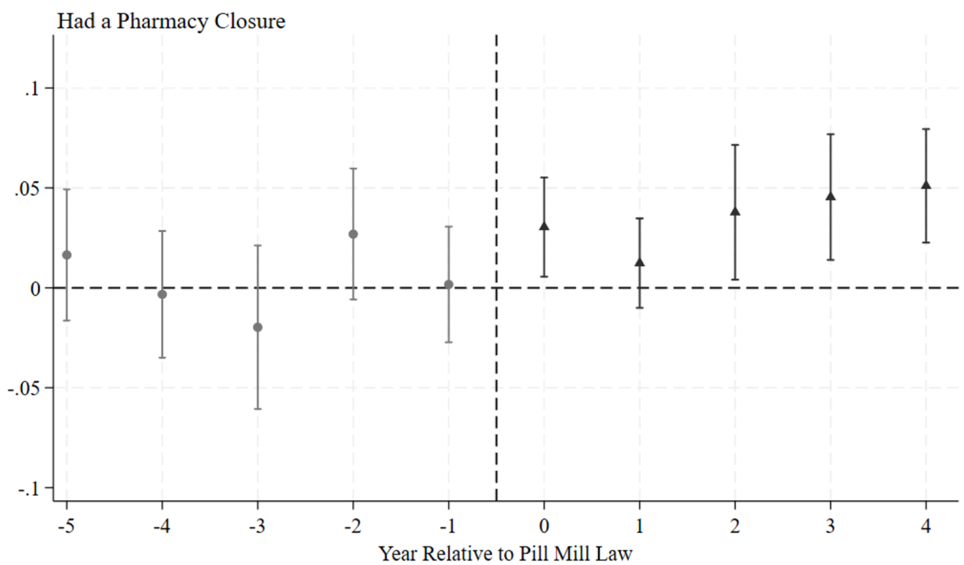
[24] Appendix Table 16 reports inconclusive results by standalone status and competition area, likely due to insufficient statistical power for such small subsamples of the data.

[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 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).

[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.



(A) Pharmacy Openings



(B) Pharmacy Closures

Fig. 3. Pharmacy Closures Increased Following the Adoption of a State Pill Mill Law
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 Fig. 3. Standard errors are clustered at the state level.
Source: National Establishment Time-Series, 2000–2018

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

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

	(1) Standalone Pharmacies	(2)	(3) Non-Standalone Pharmacies	(4)
	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	0.042***	0.002	−0.006
	(0.008)	(0.008)	(0.011)	(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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.

CRediT authorship contribution statement

Anne M. Burton: Writing – original draft, Visualization, Software, Methodology, Formal analysis, Conceptualization. **Brandyn F. Churchill:** Writing – original draft, Software, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2025.103071.

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