Capping ‘Pill Mills’: Estimating the Effect of State Pain Management Clinic Laws

by

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Abstract

This paper examines the impact of Pain Management Clinic Laws (PMCLs), state policies designed to target high-volume suppliers of prescription pain medication, on the opioid epidemic. Utilizing an extended dataset on overdose deaths, admissions to Substance Use Disorder treatment, and opioid quantities, I employ a difference-in-differences model exploiting cross-state variation in time and extent of adoption. I find that the implementation of a Pain Management Clinic Law reduces the grams of Morphine Equivalent units per person on average by 0.1, or 13%, and reduces admissions to specialty treatments for prescription opioids by 27%. Further, using an event study framework to measure effects up to four years following implementation, I find that these reductions in opioid grams and admissions persist up to four years in the post period. Additionally, I present evidence that law implementation reduces overdose deaths for prescription opioids and increases overdose deaths for heroin, indicating potential spillovers to illegal opioids. In addition to investigating the effects of PMCLs as a blanket intervention, I find that specific features of the laws, namely Physician Owner Requirements, significantly reduce opioid distribution. I also consider potential spillovers of the laws to neighboring states, finding evidence that implementation increases the quantity of opioids distributed in bordering states. These findings bolster the literature on PMCLs, open up new channels for extension, and present compelling evidence for policymakers.

I. Introduction

The misuse of prescription and illegal opioids has reached epidemic proportions in the United States. Over the course of the 21st century, deaths due to drug overdoses have increased more than four times (CDC 2019). The opioid epidemic has had heterogeneous effects within population subgroups and its drastic effects have ultimately led to an increase in the mortality rate of white non-Hispanic men (Case and Deaton 2015). In line with an increase in drug overdoses, the number of opioid prescriptions nearly tripled from 1991 to 2011 (NIDA 2015). The opioid prescribing rate at its peak in 2010 was over 80 grams per 100 people in the United States, with some counties having more prescriptions than people (Guy et al. 2017). Even with slight reductions from peak levels, the amount of opioids prescribed in 2015 was enough to medicate every person in the United States non-stop for 3 weeks (Guy et al. 2017). Initial estimates of the economic burden of this crisis were $58 billion annually (Becker et al. 2008), with more recent estimates placing the burden closer to $80 billion (Florence et al. 2016).

With these documented economic costs, policymakers at the local, state, and federal levels have implemented legislation to curb both prescription and illegal opioid abuse. These policies range from Prescription Drug Monitoring Programs (PDMPs) to Pain Management Clinic Laws (PMCLs) to Doctor Shopping Laws (DSLs), among others (Deiana and Giua 2018). Pain Management Clinic Laws, which target high-volume prescribers of opioids, attempt to reduce the supply of prescriptions opioids available for non-medical use. These laws place restrictions and requirements on pain management clinics, which are practices that primarily treat chronic pain. By focusing on these clinics, these policies intend to constrict a large supply source of opioids in the market. This paper explores the effect of these targeted laws on the opioid epidemic, including effects on prescriptions, overdose deaths, and spillover effects to

heroin and neighboring states. The literature on the effects of PMCLs is relatively light, but initial research suggests that these laws reduce prescription opioid overdose deaths (Popovici et al. 2018). Ultimately, this paper presents new evidence on the effects of PMCLs by utilizing an extended dataset over a time horizon that captures post periods for treated states, allowing for investigation of extended or lagged effects. In addition, this paper further presents new evidence on the effects of specific features within the laws and spillovers to neighboring states.

It is this paper's finding that the implementation of a PMCL reduces grams of Morphine Equivalent units per person by an average of 13%, an effect that extends for four years post law implementation. Further, the analysis finds that PMCL introduction reduces admissions to prescription opioid Substance Use Disorder treatments per 1000 people by an average of 27%. The paper additionally finds evidence of reduction effects of opioid overdose deaths as well as spillover effects to heroin and neighboring states. Given the significant economic costs associated with the epidemic, research on the effectiveness of policies aimed to reduce those costs is immediately relevant to policymakers.

This paper is organized as follows: section 2 provides background on Pain Management Clinic and other state laws; section 3 surveys the existing literature on state laws targeting opioids; section 4 discusses theory; section 5 presents the main data sources; section 6 discusses methodology; section 7 presents the results of the main analyses; section 8 presents additional analyses of spillover effects and features of laws; section 9 presents a series of robustness checks; section 10 discusses the limitations and extensions of my work; and section 11 concludes with my contribution to the literature.

II. Background

II.A Background on Pain Management Clinics

The definition of what constitutes a pain management clinic (PMC) varies by state, but typically these clinics are ones that provide chronic pain treatment to a majority of their patients (PDAPS 2018). These clinics can address pain management in a variety of ways, including therapy, controlled substances, and surgical procedures. These clinics gained notoriety during the opioid epidemic as a source of overprescribing opioids. In particular, pain clinics in Florida were found to be dispensing high volumes of non-medically justified opioids to abusers (Johnson et al. 2014; Florida 2014). In theory, these clinics exist to provide pain management services to those who struggle with chronic pain. In practice, however, these clinics, which are colloquially known as "pill mills," dispensed an outsized proportion of opioid prescriptions to both medical and non-medical users (Buchmueller and Carey 2018).

II.B Background on State Laws

States implemented a variety of laws to combat the opioid epidemic from both the supply and demand sides. Prescription Drug Monitoring Programs allow prescribers and dispensers to more easily identify a patient's opioid misuse by utilizing a centralized database of personal prescription histories. By more easily identifying misusers, physicians can take actions to curb the prevalence of opioids in the population. In addition, doctor shopping is a behavior in which patients seek out multiple medical providers for the purpose of gaining illicit access to prescription medications (Sansone and Sansone 2012). States have implemented laws designed to target doctor shopping behavior. These laws may work in tandem with PDMPs by requiring physicians to access the databases for all patients seeking opioid prescriptions or may require

patients to disclose all current prescribed controlled substances (Popovici et al. 2018). States may charge violators of these laws with felonies or misdemeanors depending on state provisions.

Pain Management Clinic Laws target pain management clinics by imposing additional requirements around ownership, operations, prescription limits, and inspection and certification guidelines (PDAPS 2018). These laws, like the definition of PMCs, vary by state but fall into the category if they increase state oversight over clinics involved in pain management. Each PMCL requires the clinic to register or seek certification from the state. Often these laws extend state oversight to inspections (CDC PMCL). Through these requirements, the laws seek to identify and punish bad actors, while maintaining proper pain management access for those for whom it is medically appropriate. It is unclear the extent to which PMCs adjusted operations due to these laws, but the laws specifically targeted those clinics engaged in inappropriate prescribing practices. The laws did result in states shutting down clinics. In particular, the implementation of a PMCL in Florida led to the closure of nearly 250 clinics (Johnson et al. 2014). It remains unclear whether specific provisions of the laws drive effects on prescriptions. Table 1 provides an overview of PMCL characteristics by state.

III. Literature Review

Much recent attention has been paid to the opioid epidemic. The literature on the subject is dense and explores a multitude of facets including causes, policies, and treatments. Recent work has looked at the causes of the opioid crisis (Ruhm 2018), the effect of health insurance on treatment (Maclean and Saloner 2017), the effect of opioids on crime (Deiana and Giua 2018) and the effect of medical marijuana (Powell et al. 2018), among other topics. In comparison to the entire literature on opioids, the literature focusing on Pain Management Clinic Laws is relatively light.

Previous work on PMCLs has either sampled a limited time frame, focused on a specific state, failed to consider the importance of specific provisions of the laws, or failed to study spillover effects. For this paper, the most relevant literature includes evaluations of state opioid policies, previous specific work on PMCLs, and literature that explores spillover effects and law characteristics.

In particular, Buchmueller and Carey (2018) investigated the effects of state prescription drug monitoring programs on Medicare claims. The authors find that a prescription drug monitoring program that includes a "must access" provision that requires physicians to access the database reduces the prevalence of doctor shopping behavior. Alternatively, those states with 'weak' PDMP laws exhibit no effects. Neither PDMP type had a statistically significant effect on poisoning incidents (Buchmueller and Carey 2018). Like Buchmueller and Carey, Patrick et al. (2016) found larger effects for PDMPs that had must access provisions and stricter criteria than for baseline PDMP implementation. Both studies highlight the potential importance of features of laws, rather than the implementation per se. Meinhofer (2018) additionally studied the effects of PDMPs on prescription drugs, extending her analysis outside of the opioid class. Like the previous papers, she found that required PDMP use reduced prescription opioid use. Her paper provides a useful framework for thinking about spillover effects of opioid-targeting state laws.

The motivation for this paper stems from Popovici et al. (2018), which explored the effect of state laws designed to prevent nonmedical use of opioids. In particular, the paper looked at the effect of state implementation of pain management clinic and doctor shopping laws, using a sample from 1999-2013. The authors utilized the National Vital Statistics Mortality Files and the Treatment Episodes Data Set to identify outcomes such as drug overdose mortality and admission into specialty treatment. Using a difference-in-differences model, the authors

exploited cross-state variation in law adoption and found that the implementation of pain management clinic laws reduced opioid overdose deaths by 9.6%. While the paper noted these effects, it has a noted limitation. The majority of Pain Management Clinic Laws were passed between 2011 and 2013, meaning the analysis likely does not capture any extended or lagged effects of the laws. The authors note the potential value in extending their paper in search of these effects (Popovici et al. 2018, p. 303).

In addition to Popovici et al. (2018), other studies have investigated the effects of PMCLs in specific states. Rutkow et al. (2015) looked at the effects of prescription drug monitoring programs and pill mill laws in Florida. Using Georgia as a control state, the authors focused on the effect of these policies on high-risk prescribers, defined as those in the top 5th percentile of opioid volume for the pre-intervention period. Their high-risk cohort represented four percent of prescribers but accounted for nearly 70% of the opioid volume. The implementation of pill mill laws and PDMPs had a statistically significant impact on high-risk providers relative to low-risk providers. The paper suggests that potential bad actors respond disproportionately to state policies targeting opioid supply (Rutkow et al. 2015). Similarly, Lyapustina et al. (2016) found that the implementation of Texas' PMCL led to a significant reduction in opioid prescribing, which could be attributed to the highest-risk patients and prescribers at the baseline. Not all studies have found significant effects of PMCLs, however. Brighthaupt et al. (2019) employed a synthetic control method to estimate the effect of PMCLs in Ohio and Tennessee. They found no effects of these laws on prescription opioid, heroin, or synthetic opioid overdose deaths in either state (Brighthaupt et al. 2019).

Much of the literature evaluating the effects of state policies on opioid outcomes employs a difference-in-differences approach exploiting cross-state variation in the timing and adoption

of laws (Popovici et al. 2018; Meinhofer 2018; Buchmueller and Carey 2018). These studies have provided evidence supporting the validity of this method, typically through the use of an event study methodology. In particular, Buchmueller and Carey (2018) and Kilby (2015) utilized an event study methodology to explore extended or lagged effects of PDMPs as well as to test for policy endogeneity.

The literature on opioids is robust. However, little of the focus has been on evaluating state Pain Management Clinic Laws. Those who have studied PMCLs utilized either a limited time frame, focused on subsamples of treated states, considered the laws as a blanket intervention, or failed to study spillover effects. Further, the current literature has detailed mixed effects of PMCLs on prescriptions and overdose deaths. This paper will extend the existing literature by considering extended or lagged effects, the relevance of characteristics of the laws, and spillovers to neighboring states.

IV. Theory

In theory, Pain Management Clinic Laws should adjust the drug environment by making it more difficult for potential opioid abusers to access prescription opioids for non-medical use. By inspecting clinics and imposing stringent requirements on opioid disbursement, states can target those high-risk clinics that disproportionately account for prescriptions. Therefore, the introduction of PMCLs should reduce the volume of opioids prescribed. Additionally, PMCLs should theoretically reduce prescription opioid overdose deaths by limiting the number of prescriptions available in the market.

The effect of these laws on admission to Substance Use Disorder (SUD) treatments and non-prescription opioid overdose rates is less clear. With regards to SUD admissions, these laws

could increase admission by identifying individuals with drug abuse problems. Individuals may not be aware of the implementation of a law and, thus, continue their same drug procurement behavior. Providers or legal entities may become aware of this behavior due to the law and push that individual to SUD treatment. Alternatively, PMCLs may reduce SUD admissions by stopping the abuse in the first place. If high-risk prescribers disproportionately account for the fulfillment of opioid prescriptions, targeting those clinics may curb initial access to highly addictive drugs. PMCLs, by limiting the supply of prescription opioids, may have spillover effects into non-prescription opioids, like heroin. If an individual is addicted to opioids in the pre-period and due to the law can no longer access these prescriptions, he or she may turn to heroin or synthetic opiates. However, in similar logic to above, PMCLs could limit the quantity of individuals ever addicted to opioids, which could have no effect or reduce heroin overdose deaths. It is possible that PMCLs could produce a dynamic effect by initially increasing heroin overdose deaths but reducing deaths over time by altering the drug environment.

V. Data

The paper utilizes a number of data sources. The research question focuses on the effect of Pain Management Clinic Laws on outcomes such as prescription opioid overdose deaths, volume of opioids prescribed, entrance into substance abuse treatments, and spillovers to neighboring states. Data on PMCLs comes from previous literature, the CDC, and PDAPS. Data on mortality will come from the National Vital Statistics System (NVSS) Mortality Files. Data on entrance into substance abuse treatments will come from the Substance Abuse and Mental Health Services Administration's (SAMHSA) Treatment Episodes Data Set (TEDS). Data on drug volume comes from the Department of Justice's Automation of Reports and Consolidated Orders System

(ARCOS). Each of these data sources is aggregated to the state level. The District of Columbia is included as well.

Pain Management Clinic Laws

Pain Management Clinic Laws currently exist in 12 states, which implemented them over a range of years from 2008 to 2018. Table 1 details the adoption and effective date of PMCLs for each state as well as effective dates for the specific features mandatory checking of the PDMP, physician owner requirements, drug testing, and inspections.

Table 1: PMCL Effective Dates

State	Law Implementation	Owner Required to be Physician	Required to Check PDMP	Inspections	Drug Testing
Alabama	2013*	-	-	-	-
Florida	2010*	2010*	-	2010*	-
Georgia	2014	2014	2014*	2014*	2014
Kentucky	2013	2013	2013	2013	2013
Louisiana	2008	2008	2014*	2008	2008
Mississippi	2012	2012	2013*	2013*	-
Ohio	2011*	2011*	2016	2013*	2011*
Tennessee	2012	-	2013*	2012	2012
Texas	2010	2010*	2017*	2010	2010
West Virginia	2013	2013	2013	2013	2015*
Wisconsin	2016*	-	2017*	-	-

Note: Arizona implemented in 2018. * marks that the law or provision was effective for a partial year

The dates included in this table match those of Popovici et al. (2018), as well as other papers that include PMCLs in their analysis (Deiana and Guia 2018). In addition, data on the specific provisions of the laws come from PDAPS, which maintains a database on a number of state laws targeting opioids. The exact composition of the laws within states varied over time, meaning that some states adjusted the features within the laws in the years following the passage of the initial law. Both the laws and facets were absorbing, meaning that once a state adopted that provision in year t, they did not repeal the law or provision in future years. In my analysis, I look at the

effects of the adoption of a PMCL, as well as the effects of specific provisions within PMCLs. In doing so, I provide evidence of channels through which these laws can adjust outcomes. For laws that were effective for partial years, I code the PMCL variable for the share of the year. For instance, if the law became effective on July 1, I would code PMCL as 0.5 for that year and 1 for the years following. Due to no post period data, Arizona will not be considered a treated state in the analysis.

Mortality

Data on mortality comes from the Centers for Disease Control and Prevention's Multiple Cause of Death files as part of the National Vital Statistics System. The data spans the years 2000-2017 and provides information on state-level national mortality. In particular, the Multiple Cause of Death files includes an individual's underlying cause of death as well as up to twenty additional multiple causes. For the purposes of this analysis, the multiple causes of death files are useful in coding for deaths due to prescription opioids, heroin, non-prescription opiates, and other illegal substances. The file classifies the causes of death in accordance with the International Classification of Disease (ICD), which provides codes corresponding to various causes including overdoses due to drugs or alcohol. The ICD-10 revision was adopted in 1999 and, thus, should be consistent across the entire sample with regards to our codes of interest. I use the following codes which relate to drug causes: X40-X44 (Accidental poisoning by and exposure to drugs and other biological substances), X60-X64 (Intentional self-poisoning by and exposure to drugs and other biological substances), and Y10-Y14 (Poisoning by and exposure to drugs and biological substances, undetermined intent).[1] Additionally, I include the following drug codes: T40.1 (Heroin), T40.2 (Natural opioid analgesics and semisynthetic opioids), and T40.3 (Methadone).

[1] These codes come from the CDC's ICD-10 Cause of Death list (https://www.cdc.gov/nchs/data/dvs/im9_2002.pdf.pdf)

For the purposes of my analyses, I split T40.1 from T40.2-T40.3. One limitation of this data set is that it suppresses any sub-national data representing 0-9 deaths. The resulting analysis tests the sensitivity of specifications to this suppressed data, including imputing values.

Figure A1 shows the quantity of prescription opioid overdose deaths over time, while Figure A2 details heroin deaths. Prescription opioid overdose deaths steadily rose from 2000 to 2011, before staying relatively stable through 2017. Heroin deaths rapidly grew following 2010.

Substance Abuse Treatment

Data on substance abuse treatment comes from SAMHSA's TEDS files. TEDS is a national dataset that represents admissions into specialty substance abuse treatment facilities. The data is collected by states as part of monitoring facilities that receive public funds. The set covers around 1.5 million admissions per year, but does not cover the entirety of SUD admissions (TEDS).

The set includes information on demographics, including age, sex, race, as well as substance abuse characteristics. I use TEDS admissions from 2000-2017 and aggregate state level admissions by year for opioids, heroin, and addictive stimulants.

Figures A3 and A4 show the growth in specialty admissions over time for opioids and heroin, respectively.

Prescription Drug Volume

For data on drug quantities, I utilize the DEA's ARCOS database. The DEA requires manufacturers and distributors of controlled substances to report their transactions in the ARCOS database. This database, which is publicly available, monitors controlled substances from the point of manufacturer to distribution. The DEA produces a yearly retail summary report, which details the distribution in grams of drugs by zip code by quarter. The system underwent changes

in 2000, making the years of interest 2001 to 2017 (ARCOS). I aggregated drug reports to the state and year levels. Specifically, this project focuses on hydromorphone, methadone, oxycodone, hydrocodone, morphine, fentanyl, and meperidine. In line with previous literature (Meinhofer 2018; Alpert et al. 2018), I converted the drug supply into grams of morphine equivalent units for parity across opioids.[2] In Figure A5, I plot the grams of morphine equivalent units per person per year for the study period. Particularly, the plot details high growth in the prevalence of opioids from 2001 to 2010, with a decline following 2012.

In Table 2, I provide summary statistics for these outcome variables.

Table 2: Opioid Outcome Summary Statistics

	Mean	Std. Dev.	Minimum	Maximum
Morphine Equivalent Units	0.750	0.337	0.1466	1.841
Oxycodone	0.155	0.082	0.0156	0.662
Fentanyl	0.105	0.055	0.0342	1.439
Hydromorphone	0.0156	0.008	0.002	0.055
Meperidine	0.001	0.001	0.00005	0.006
Morphine	0.064	0.028	0.0183	0.181
Methadone	0.300	0.223	0.0117	1.258

Opioid Overdose Deaths	Mean	Std. Dev.
Prescription Opioid	246.56	272.83
Heroin	106.60	191.47

Opioid Admissions	Mean	Std. Dev.
Prescription Opioid	3,950	5,354
Heroin	7,514	15,475

Note: Observations for drug quantities are 867, for overdose deaths are 919 (suppressed data is imputed to 0), for opioid admissions are 887.

[2] I utilized the drug multipliers from Deiana and Guia (2018) and Brady et al. (2014). The multipliers on the substances are as follows: hydromorphone by 4, methadone by 7.5, oxycodone by 1, hydrocodone by 1, morphine by 1, fentanyl by 75, and meperidine by 0.1.

VI. Methods

For the analysis of PMCLs on opioid outcomes, I employ a difference-in-differences estimation strategy, exploiting cross-state variation in time and extent of adoption. The principal regression model is

$$(1) \qquad Y_{st} = \alpha_0 + \beta PMCL_{st} + \gamma X_{st} + \upsilon_s + \phi_t + \varepsilon_{st}.$$

Y_{st} is an opioid outcome in state s in year t; $PMCL_{st}$ is a vector of state adoption of PMCLs and equals 1 if state s had an operating PMCL in year t; X_{st} is a vector of covariates that represent the presence of other opioid laws, such as a Prescription Drug Monitoring Programs or Good Samaritan Laws, and state controls for population demographics and economic factors;[3] υ_s represents state fixed effects; and ϕ_t represents time fixed effects. I include state and time fixed effects to control for state-specific, time invariant differences and national shocks, respectively.

The outcome of interest in the baseline model is β, the effect of PMCLs on an outcome such as overdose rates, controlling for other laws and potential confounders. The identifying assumption in a difference-in-differences strategy is that in the absence of Pain Management Clinic Laws, treatment and control states would continue on parallel trends in opioid-related outcomes. While the assumption is not immediately testable, I take steps to show the validity of the assumption. Admittedly, the states that implemented PMCLs are geographically clustered in the southeast United States. The variation in years of adoption, however, should aid in finding causal effects as states can be in both the treatment and control groups at various times in the study. Previous literature exploiting similar cross-state variation has provided plausible evidence of exogeneity (Popovici et al. 2018; Erfanian et al. 2019; Deiana and Giua 2018).

[3] See Table A1 for covariate summary statistics

In addition to specification 1, I employ a more flexible event study specification to present evidence on the parallel trends assumption and to test for any extended or lagged effects of the laws.[4] In the event study, I reorient the year of implementation of the law to period 0 and estimate the following equation:

$$(2) \qquad Y_{st} = \alpha_0 + \sum_{\pi} \beta_{\pi} D_{\pi,st} + \gamma X_{st} + \upsilon_s + \phi_t + \varepsilon_{st}.$$

The variables remain the same as above, with $D_{\pi,st}$ representing dummy variables for lags and leads. For the main specification, I utilize four years of lags and leads.[5] Thus, $D_{-4,st}$ represents a dummy for four years before a state s implemented a PMCL in year t. For never-treated states, $D_{\pi,st}$ takes on the value 0 for all years t. Due to the extended nature of my dataset compared to previous literature, I retain a nearly balanced sample for the initial specification. I have at least three years of post-period data for all but one treated state. In the robustness tests, I will check the sensitivity of my estimates to balanced specifications.

The regression tests for any jumps in advance of the policy implementation that might suggest endogeneity. Specifically, if the estimates on β for $\pi < 0$ are not zero, that provides evidence of policy anticipation. Alternatively, having no non-zero estimates on β for $\pi < 0$ lends credibility to the parallel trends story. For both specifications, I estimate robust standard errors clustered at the state level.

Equation 2 can also provide color on any extended or lagged effects of the policy. For instance, if an individual habitually utilizes a pain management clinic to procure non-medical opioids, the sheer implementation of the law may not curb either the behavior of the provider or

[4] The event study specification is in-line with previous literature (Buchmueller and Carey 2018; Deiana and Guia 2018; Kilby 2015).

[5] Further, I bin years 5 or more before and 5 or more after. The estimates are suppressed.

patient. There may be a time lag between a law's effective date and a crackdown on bad actors. Further, in the years following a PMCL, opioid abusers may adapt to the changing drug environment by finding other channels for prescription or illicit opioids. For instance, it is plausible to tell a story in which a PMCL has a delayed but positive effect on heroin usage, meaning that previous pill mill clientele turn to heroin due to a decrease in the supply of prescription opioids.

VII. Results

VII.A ARCOS

I begin by estimating equation 1 for the quantity of opioids using the ARCOS data. From Table 3, column 1 details the results of my full specification. The estimate shows that the introduction of a Pain Management Clinic Law reduces grams of Morphine Equivalent on average by 0.1 grams per person, or over 13%. In addition, column 2 specifies the regression without the state controls and yields a similar estimate. Both are significant at the 1% level.

Table 3: The Effect of PMCL Introduction on Opioid Quantities

	(1)	(2)
	ME per person	ME per person
PMCL	-0.1008***	-0.1121***
	(0.0331)	(0.0419)
Controls:		
State Controls	Y	N
State FE	Y	Y
Year FE	Y	Y
R^2	0.8855	0.8624
Clusters	51	51
Observations	867	867

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. *** p<.01, ** p<.05, *p<.10

The estimates in Table 3 are supported by the event study analysis detailed in Table 4. Column 2 shows the results of Equation 2 on grams of ME units per person. Firstly, the estimates on the leads, which I cannot reject from zero, lend support to the validity of the initial difference-in-difference analysis. Further, the estimates on the lags detail an extended effect of PMCL introduction, with estimates on two to four years after implementation showing a reduction of 0.12 to 0.19 grams of ME per person. Collectively, these two analyses tell a story of PMCLs

having a sustained reduction effect on prescription opioid quantities. By targeting high-volume prescribers, PMCLs successfully limit the supply of opioids.

Table 4: Event Study Analysis of PMCLs on Opioid Quantities

	(1)	(2)
	ME per person	ME per person
PMCL	-0.1008***	
	(0.0331)	
4 years before		-0.0179
		(0.0410)
3 years before		-0.0103
		(0.0379)
2 years before		0.0057
		(0.0328)
1 year before		0.0566
		(0.0406)
Year of implementation		0
		(.)
1 year after		-0.0698**
		(0.0268)
2 years after		-0.1216***
		(0.0427)
3 years after		-0.1649***
		(0.0578)
4 years after		-0.1909***
		(0.0717)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
R^2	0.8855	0.8893
Clusters	51	51
Observations	867	867

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. *** p<.01, ** p<.05, *p<.10

VII.B TEDS Admissions

By adjusting the drug environment through the supply of opioids available, PMCLs can affect admissions to Substance Use Disorder treatment. I first run a specification of Equation 1 on the ln(Opioid Admissions+1). I utilize a log specification because I found that it better satisfied the parallel trends assumption than did quantity of admissions. In my analysis, I include results for an admissions per 1000 people specification as well.

Both specifications show reductions in specialty admissions, with the log specification resulting in an average reduction in log admissions of 0.28 and the per capita specification resulting in a reduction of 0.2505 admissions per 1000 people, or 27%.

I then ran an event study specification on log admissions. I present the results in Table 6. Like the initial specification, the event study found reductions in admissions following PMCL introduction. These reductions were sustained over a four-year post-period.

Table 5: The Effect of PMCL Introduction on Admissions to Specialty Treatment

	(1) ln(admissions+1)	(2) per 1000
PMCL	-0.2826**	-0.2505**
	(0.1086)	(0.00012)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
R^2	0.9192	0.8235
Clusters	51	51
Observations	887	887
Mean	7.527	0.0009

Note: The dependent variable is admissions to substance use disorder treatment. *** p<.01, ** p<.05, *p<.10

Table 6: Event Study Analysis of PMCLs on Admissions to Specialty Treatment

	(1) log admissions	(2) log admissions
PMCL	-0.2826**	
	(0.1086)	
4 years before		-0.0417
		(0.1467)
3 years before		0.1029
		(0.1068)
2 years before		-0.0824
		(0.0992)
1 year before		-0.1543
		(0.1232)
Year of implementation		0
		(.)
1 year after		-0.1302**
		(0.0510)
2 years after		-0.2154**
		(0.0427)
3 years after		-0.2607**
		(0.1290)
4 years after		-0.2940*
		(0.1682)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
R^2	0.9192	0.9214
Clusters	51	51
Observations	887	887

Note: The dependent variable is admissions to substance use disorder treatment. *** p<.01, ** p<.05, *p<.10

VII.C NVSS Mortality

The NVSS Mortality files suppress data for fewer than 10 deaths in a year. In these main specifications, I imputed 0 for those years with suppressed data. In my robustness tests, I try alternate specifications.

In Table 7, I ran regressions following Equation 1 on the dependent variable ln(deaths+1). Column 1 represents the basic specification. In Column 2 I remove State Controls. In both models, PMCL introduction does not have a statistically significant effect on opioid overdose deaths. The estimates are statistically insignificant mainly due to imprecision as the estimates appear to be economically significant.

The results of the event study specification (see Column 1 in Table A8) show evidence of reductions in overdose deaths 2 years following the implementation of the law.

Table 7: The Effect of PMCL Introduction on Opioid Overdose Deaths

	(1) log	(2) log
PMCL	-0.1101	-0.2293
	(0.1675)	(0.1528)
Controls:		
State Controls	Y	N
State FE	Y	Y
Year FE	Y	Y
R^2	0.8855	0.8737
Clusters	51	51
Observations	918	918

Note: Dependent variable is ln(Deaths+1). Data comes from NVSS Mortality Files and includes instances with codes T40.2-T40.3. Suppressed data was imputed to 0. *** p<.01, ** p<.05, *p<.10

Additionally, I investigated the effect of PMCL implementation on overdose deaths due to heroin. In theory, restricting the supply of prescription opioids could deter users to illegal opioids. In Table A2, I present the results of my regressions, using both the difference-in-differences and event study specifications. The introduction of a PMCL increased heroin overdose deaths, suggesting spillover effects to the illegal drug market.

VII.D Discussion

My results suggest that PMCL implementation reduces average opioid volumes by over 13%, reduces admissions to specialty treatments by 27%, and provides suggestive evidence of reductions in prescription overdose deaths and increases in heroin overdose deaths. Rutkow et al. (2015) found that the implementation of Florida's PMCL resulted in a 2.5% decrease in opioid volume compared to Georgia, the control state. Further, Lyapustina et al. (2016) found an associated reduction of 24.3% in total opioid volume following Texas' PMCL implementation. Brighthaupt et al. (2019) found no effects of Ohio and Tennessee's PMCL introduction on opioid overdose deaths. Analysis by Popovici et al. (2018) of PMCL adoption using a sample through 2013 found reductions of 9.6% for prescription opioid overdose deaths across treated states but no statistically significant effects on admissions to specialty treatment. In their analysis of PDMPs using a sample from 1999-2014, Deiana and Guia (2018) found that that the implementation of PMCLs yielded an average reduction in opioid volume of 30%. Compared to the existing literature, my estimates on PMCL effects fit within the ranges of estimated effects on opioid volume, while exploiting an extended and country-wide sample. In addition, my analysis provides new evidence of PMCL reduction effects on SUD admissions and adds additional estimates on overdose deaths to the conflicting existing evidence.

VIII. Additional Analysis

In addition to the baseline analyses performed above, I also investigated the effects of specific features of PMCL laws as well as spillovers to neighboring states.

VIII.A Features

PMCLs varied across the implementing states. While the above analysis focused on PMCLs as a blanket intervention, I also want to investigate whether specific features of the laws matter. In particular, I focus on four features: mandatory checking of the PDMP, the explicit inclusion of facility inspections, the requirement that at least one owner be a physician, and mandatory drug testing of patients (see Table 1 for breakdown of features by state). Mandatory PDMP checking brings together PMCLs and PDMPs, which may produce a synergistic effect. Further, having regular inspections of facilities likely facilitates the identification of bad actors. Drug testing may help physicians identify doctor shoppers. Lastly, the ownership of clinics may impact operations. If a PMC is privately owned by a non-physician, it is plausible that the owner may have incentives beyond simply providing the best available chronic pain management care. Physicians are bound by licensing regulations that increase the economic costs of delivering illegal non-medical care. A private investor may not face similar costs, given that his or her livelihood is not dependent on maintaining a medical license.

To estimate the effects of these specific features on outcomes, I employ a difference-in-differences model following the general equation:

$$Y_{st} = \alpha_0 + \beta Feature_{st} + \gamma PMCL_{st} + \pi X_{st} + \upsilon_s + \phi_t + \varepsilon_{st}. \quad (3)$$

The coefficient of interest β captures the effect of the additional feature on our outcomes of interest. Equation 3 allows for states to implement provisions after the initial passage of the law,

by making the Feature indicator equal to 1 only if state s has the provision in year t. For instance, with mandatory PMDP checking, many states adopted that standard in second or third revisions of their PMCLs.

Table A3.1 shows the results of the analysis. Mandatory PDMP checking, Inspections, and Physician Owner requirements all yield significant reductions in ME units per person. In particular, Physician Owner requirements reduce grams of MEs by 0.11 per person, which is greater than the initial estimate on PMCL. Column 6 shows the results of a regression including indicators for all the features as well as the law implementation. Physician owner requirements drive the results, accounting for the entirety of the baseline PMCL impact. In addition, I ran specifications of equation 3 in which I exclude the PMCL variable. These results are in Table A3.2. All four features yield statistically significant estimates with magnitudes greater than the initial estimate on PMCL. In particular, Mandatory PDMP checking reduced grams of ME per person by 0.14, or nearly 19%, with the other features resulting in reductions of around 0.12. Similarly to Table A3.1, Column 6 details the inclusion of all four features. The estimate on Physician Owner Requirements is again statistically significant and accounts for nearly the entirety of the baseline PMCL impact. The point estimate on Mandatory PDMP Checking is additionally large in magnitude, although statistically insignificant due to imprecision. The results indicate that the composition of the law matters, in addition to the blanket implementation.

VIII.B Spillovers

In addition to looking at the effect of PMCLs on treated states, I also wanted to explore any spillover effects to neighboring states. The states that adopted PMCLs are fairly geographically

clustered in the southeast. To date, no literature has attempted to capture any synergistic or spillover effects due to neighboring state adoption. In theory, an opioid abuser close to the border of two states could procure prescription opioids for misuse from a neighboring state if their own state cracked down on clinics. In addition, black-market entrepreneurs could purchase opioids in non-treated states to sell in treated states, capitalizing on differing levels of access. Therefore, a geographic cluster of PMCL states could strengthen the policy for all adopting states.

In order to investigate spillover effects, I proceed in two ways. Firstly, I follow Meinhofer's (2018) analysis of PDMP spillovers and generate an absorbing dummy variable that is equal to 1 if a neighboring state implemented a PMCL in year t. For states that have multiple neighboring states that implemented PMCLs, I code the dummy as 1 in the year of the first PMCL adoption. Further, as some states move from non-treated to treated status, I turn the neighbor dummy back to 0 if a state implements its own PMCL in year t. I proceed in this way as I want to parse out the effects of implementation on non-treated adjacent states. If a state adopts its own PMCL, it no longer represents a 'lenient' drug environment, making it difficult to serve as an opioid source for opioid abusers or black-market entrepreneurs in already treated states – i.e. the neighboring state that caused the neighbor dummy to turn to 1 previously. Secondly, I investigate whether the quantity of neighbors with a PMCL matters. To investigate this question, I construct a variable $Neighbors\_share_{st}$, which measures the share of adjacent states with PMCLs in year t.

First, I regress each of these variables on opioid quantities. Then, I add PMCL to the analysis. Table A4 details the results. In Columns 2 and 3, the regressions excluding the PMCL indicator, both neighbors and neighbors_share have positive effects on grams of ME, suggesting a spillover effect to non-treated neighboring states. The effect on neighbors falls out with the

addition of PMCL, while that on neigbors_share remains marginally significant. The estimate on neighbors_share suggests added effects from additional neighboring states.

VIII.C Adjacent Treated States

In addition to spillovers to adjacent states, I wanted to investigate treatment effects for treated states with neighboring treated states. To do so, I ran regressions with specifications mirroring Equation 1, but with the samples restricted. In the first sample, I include treated states that had an adjacent treated state when it implemented its own PMCL. In the second sample, I restrict to treated states that did not have an adjacent treated state when it implemented its own PMCL. Table A5 shows the results of this analysis. Column 1 shows the results of the 'adjacent' sample, meaning treated states bordering another treated state at time of implementation. Column 2 details the 'non-adjacent' sample. The estimate on PMCL for those 'non-adjacent' states is almost double that of the adjacent states. This result further suggests spillovers to neighboring states. Residents in 'non-adjacent' treated states can cross the border to non-treated neighboring states following the implementation of a PMCL in their own state, or black-market entrepreneurs can take advantage of differing levels of restrictions on opioid access. 'Adjacent' state' residents or black-market entrepreneurs have less of an incentive to border cross, with at least one neighboring treated state.

IX. Robustness Checks

I engage in a number of robustness checks to demonstrate the validity of my findings. Firstly, I run additional event study specifications on my ARCOS drug data without state controls, and adding state trends but dropping fixed effects. The results can be seen in Table A6. In both

specifications, the results align in magnitude and direction with the baseline specification. PMCL introduction reduces the quantity of MEs per person.

Further, although my panel is nearly balanced in the baseline analysis, with 3 years of post-period data for all but one of my treated states and 4 years of post-period data for all but two, I run event study specifications on 3 and 4 year balanced panels. The estimates in Table A7 mirror those of the baseline analysis, with the four year balanced sample yielding slightly greater effects.

Additionally, as the NVSS Mortality Files suppressed data for deaths that were fewer than 10 in a year, I test the results of my findings to alternate specifications. Table A8 compares the results of imputing 0 or 5 for missing values on opioid overdoses. In Column 1, there is slight evidence of a reduction effect two years post implementation. Column 2 shows evidence of reductions beginning the year following implementation and sustaining through the third year post. Like the discussion of Table 7, these analyses reveal point estimates that seem economically significant, even if statistically imprecise. Combined, these two imputation procedures show suggestive evidence that PMCL introduction decreases overdose deaths due to opioids.

I employ a similar procedure for heroin overdose deaths. Table A9 compares the results of imputing 0 or 5. In the initial imputation, the event study did not show effects, while the difference-in-differences estimate showed positive effects. From Column 2, there is evidence of a positive effect on heroin overdose deaths two years following the implementation of the law. The reduction in the supply of prescription opioid may push users to heroin, increasing the heroin overdose deaths in the year following.

Table A10 further details the robustness of specialty admissions findings to alternate specifications.

X. Limitations and Extensions

This paper explores the effects of PMCLs on specific opioid outcomes, namely admissions to specialty treatment, drug quantities, and overdose deaths. The effects of PMCLs likely extend beyond these outcomes. In particular, I am unable to parse out if the reduction in prescription opioid quantity is solely due to restricting non-medical use or also restricts medically necessary use. An interesting exploration would be to investigate the effects of these laws on pain management care access, measures of pain management quality, and pain levels in the population. It is plausible that the implementation of these laws curbs medically inappropriate opioid use, while also impacting those for whom using opioids for chronic pain management is necessary. Another potential extension would be to look at the effect of these laws on pain control specialists. The implementation of these laws adjusts the economic environment for those practitioners. To date, the literature has yet to search for these effects.

In addition, my analysis of specific features of laws revealed that Physician Owner Requirements had significant reduction effects on opioid distribution. An extension of this paper would be to investigate the effects of private ownership on PMCLs. More specifically, there has been an uptick in recent years of private equity investment in multi-site healthcare, including Pain Management Clinics. These firms have differing incentives than medical professionals, namely focusing on profit. It would be interesting to see if private equity, or other private investment, in PMCs affects drug distribution.

My analysis focuses solely on the effects of these laws at the state level, given that they are statewide interventions. However, my analysis does not comment on potential differing within-state effects. Similar data is available at zip code levels, opening up opportunities for more granular analysis. In particular, the spillover analysis to neighboring states would benefit from investigation at the zip code or county level. Counties bordering other states likely have larger spillover effects than those within a state.

While there are avenues for extension, my paper adds to the literature by presenting new evidence on PMCLs. Additional research, particularly in the realms of care access and ownership is needed to develop a fuller picture of the effects of these laws.

XI. Conclusions

This paper contributes to existing literature around the opioid epidemic by investigating the effects of PMCLs, laws designed to target high-volume prescribers of opioids. While the literature on opioids is rich, little focus has been turned towards these laws, which have the potential to restrict the supply of opioids. Previous literature on PMCLs focused on limited time horizons that failed to capture many post-periods of the laws for many states. By utilizing an extended dataset that included four year of post-period data for all but two treated states, I provided evidence on the extended effects of the laws. In particular, I found that PMCLs reduce grams of morphine equivalent units by over 13% per person per year. Further, using an event study methodology I found that these effects extend for four years following the initial implementation of the law. My analysis presents evidence of PMCL reduction of specialty admissions for opioids. I additionally provide evidence on the effects of PMCLs on the illegal opioid market, by documenting an increase in heroin deaths following implementation.

This paper further contributes to the literature by investigating the effects of specific features of PMCLs. More specifically, I found that the inclusion of Physician Owner Requirements drives PMCL impacts on drug quantities. Lastly, I present new evidence on potential spillover effects of PMCLs to neighboring states. This paper opens up new channels for investigation of PMCL and is immediately relevant to policymakers at the state level considering possible opioid interventions.

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XII. Appendix

Figure A1: Opioid Overdose Deaths Over Time

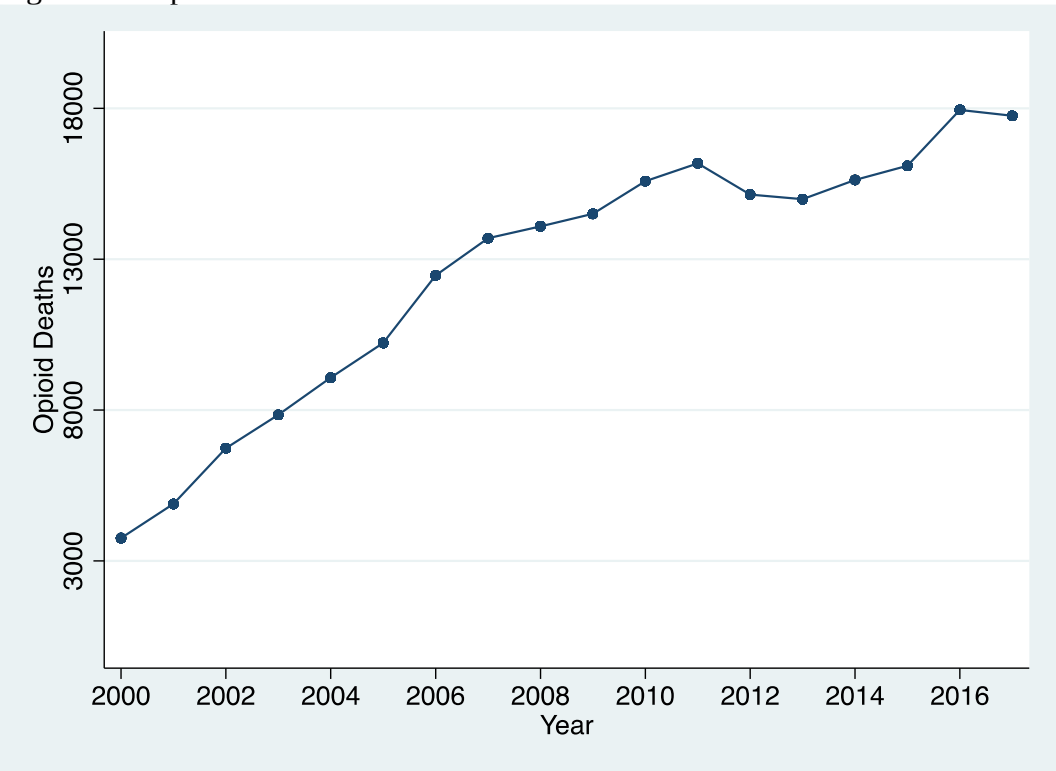

Figure A2: Heroin Overdose Deaths Over Time

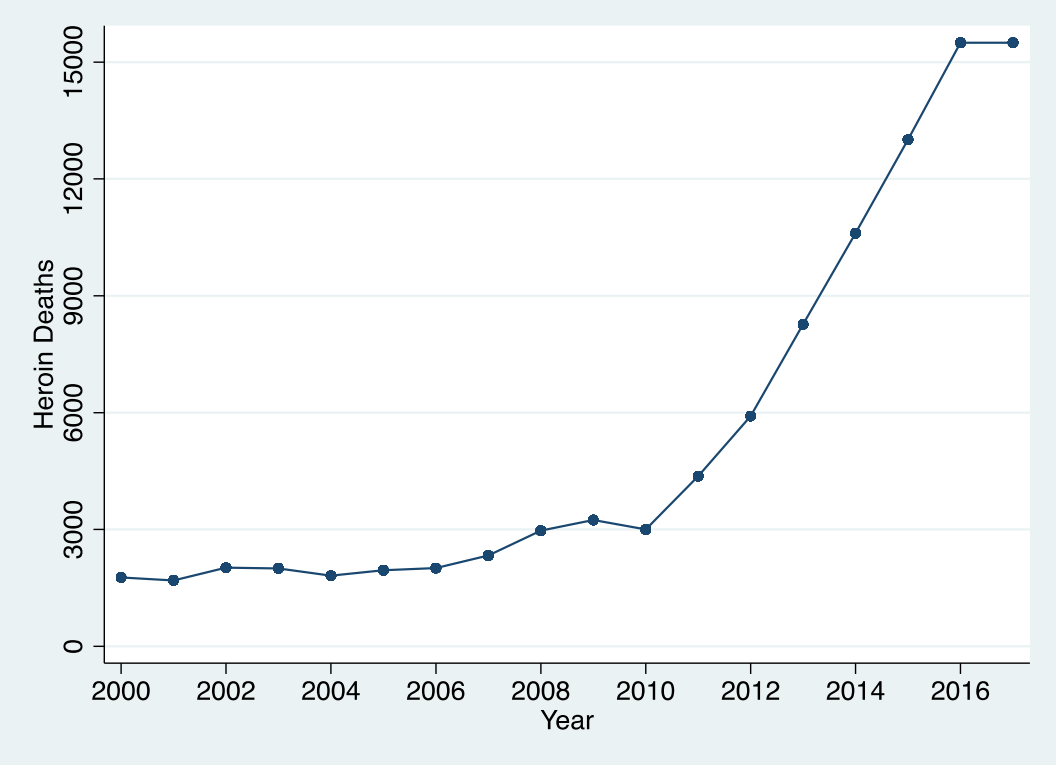

Figure A3: Treatment Admissions for Opioids Over Time

Figure A4: Treatment Admissions for Heroin Over Time

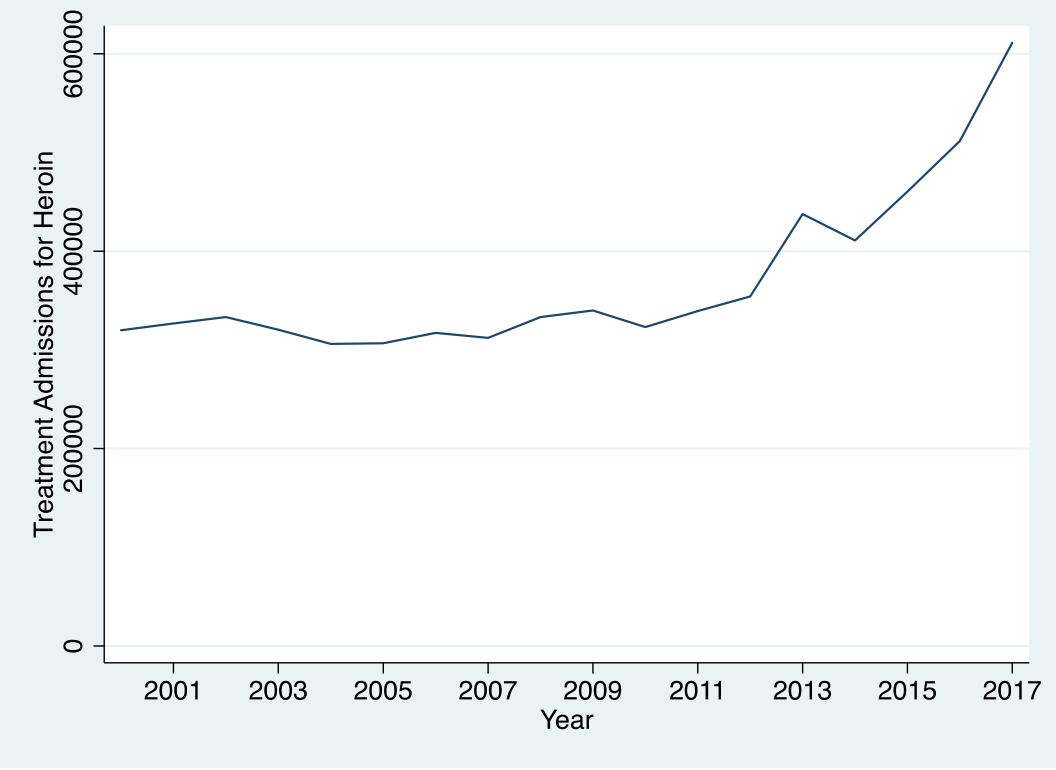

Figure A5: Prescription Opioids per Person per Year Over Time

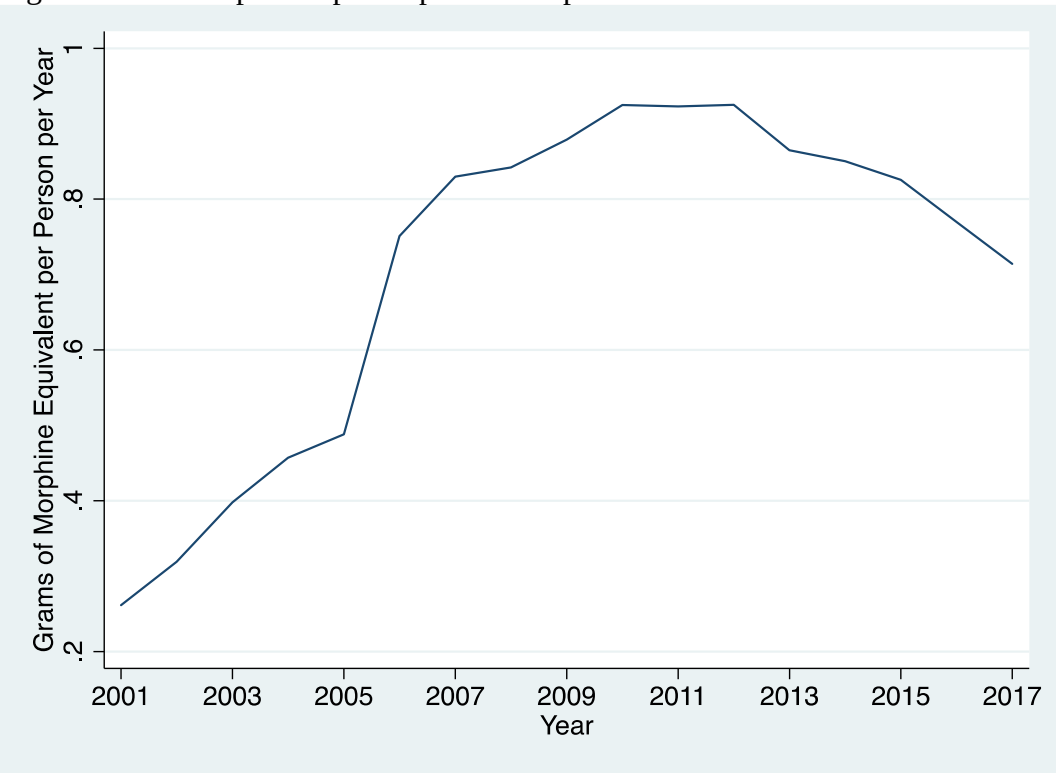

Table A1: Covariate Descriptive Statistics

Control Variables	Mean	St. Dev.
Population	5,970,838	6,693,406
Over 60 Population	1,100,646	1,195,188
Unemployment Rate	5.72	1.98
Male Proportion	0.49	0.008
White Proportion	0.81	0.132
PDMP	0.45	0.49
Good Samaritan	0.18	0.368

Note: Observations are 919 for all variables. Unemployment data came from the Bureau of Labor Statistics. Population and demographc data came from CDC Wonder Database.
Data on PDMP laws came from Mallatt (2017). Data on Good Samaritan laws came from PDAPS.

Table A2: Effect of PMCL Introduction on Heroin Deaths

	(1) log deaths	(2) log deaths
PMCL	0.9897***	
	(0.2777)	
4 years before		-0.4965
		(0.3738)
3 years before		-0.3196
		(0.2966)
2 years before		-0.0622
		(0.1705)
1 year before		-0.2473
		(0.2432)
Year of implementation		0
		(.)
1 year after		0.2118
		(0.2040)
2 years after		0.3808
		(0.2537)
3 years after		0.3189
		(0.2971)
4 years after		0.3078
		(0.2885)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
R^2	0.8866	0.8898
Clusters	51	51
Observations	918	918

Note: Dependent variable is ln(Deaths+1). Data comes from NVSS Mortality Files and includes instances with code T40.1.
Suppressed data was imputed to 0. *** p<.01, ** p<.05, *p<.10

Table A3.1: Effects of PMCL Features on Opioid Quantities

	(1) ME per person	(2) ME per person	(3) ME per person	(4) ME per person	(5) ME per person	(6) ME per person
PMCL	-0.1008***	-0.0518	-0.0309	-0.0509	-0.0143	0.0269
	(0.0331)	(0.0384)	(0.0361)	(0.0412)	(0.0275)	(0.0327)
Mandatory PDMP Check	-	-0.0959**	-	-	-	-0.0886
		(0.0428)				(0.0625)
Inspections	-	-	-0.0862**	-	-	0.04408
			(0.0423)			(0.0611)
Drug Testing	-	-	-	-0.0754	-	-0.0553
				(0.0642)		(0.0644)
Physician Owner Req.	-	-	-	-	-0.1139**	-0.1074**
					(0.0435)	(0.0463)
Controls:						
State Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
R^2	0.8855	0.8864	0.8859	0.8860	0.8864	0.8873
Clusters	51	51	51	51	51	51
Observations	867	867	867	867	867	867

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. *** p<.01, ** p<.05, *p<.10

Table A3.2: Effects of PMCL Features on Opioid Quantities

	(1) ME per person	(2) ME per person	(3) ME per person	(4) ME per person	(5) ME per person	(6) ME per person
PMCL	-0.1008***	-	-	-	-	-
	(0.0331)					
Mandatory PDMP Check	-	-0.1403***	-	-	-	-0.0869
		(0.0390)				(0.0622)
Inspections	-	-	-0.1154***	-	-	0.0559
			(0.0348)			(0.0672)
Drug Testing	-	-	-	-0.1214**	-	-0.0502
				(0.0474)		(0.0670)
Physician Owner Req.	-	-	-	-	-0.1273***	-0.0968**
					(0.0358)	(0.0395)
Controls:						
State Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
R^2	0.8855	0.8860	0.8859	0.8857	0.8863	0.8873
Clusters	51	51	51	51	51	51
Observations	867	867	867	867	867	867

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. *** p<.01, ** p<.05, *p<.10

Table A4: Effect of PMCL Introduction on Neighboring States

	(1) ME per person	(2) ME per person	(3) ME per person	(4) ME per person	(5) ME per person
PMCL	-0.1008***	-	-	-0.0827**	-0.0743*
	(0.0331)			(0.0374)	(0.0385)
Neighbor	-	0.0587**	-	0.0343	-
		(0.0246)		(0.0283)	
Neighbor Share	-	-	0.2667***	-	0.1877*
			(0.0861)		(0.101)
Controls:					
State Controls	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
R^2	0.8855	0.8843	0.8853	0.8860	0.8867
Clusters	51	51	51	51	51
Observations	867	867	867	867	867

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. *** p<.01, ** p<.05, *p<.10

Table A5: Effects on Opioid Quantities - Adjacent States

	(1) ME per person	(2) ME per person
PMCL	-0.0740*	-0.1363**
	(0.0383)	(0.0546)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
Observations	782	765
Treated States	Adjacent	Non-Adjacent

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. Adjacent treated states are defined as those that bordered an already treated state at the time of their own PMCL implementation. *** p<.01, ** p<.05, *p<.10

Table A6: ARCOS Robustness Check – Alternate Specifications

	(1)	(2)
	MEs per person	MEs per person
4 years before	-0.0125	0.0266
	(0.0386)	(0.0460)
3 years before	0.0017	-0.0076
	(0.0376)	(0.0414)
2 years before	0.0160	0.0267
	(0.0350)	(0.0358)
1 year before	0.0617	0.0717
	(.0410)	(0.0478)
Year of implementation	0	0
	(.)	(.)
1 year after	-0.0725***	-0.0935***
	(0.0268)	(0.0291)
2 years after	-0.1217***	-0.1355***
	(0.0447)	(0.0420)
3 years after	-0.1624***	-0.1913***
	(0.0583)	(0.0479)
4 years after	-0.1981***	-0.2418***
	(0.0624)	(0.0549)
Controls:		
State Controls	N	Y
State FE	Y	N
Year FE	Y	N
State Trends	N	Y
R^2	0.8678	0.7936
Clusters	51	51
Observations	867	867

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person. ME was calculated using the procedure outlined in Section V. *** p<.01, ** p<.05, *p<.10

Table A7: ARCOS Robustness Check - Balanced Panel

	(1) 4 year unbalanced	(2) 3 years balanced	(2) 4 years balanced
4 years before	-0.0179		-0.020
	(0.0410)		(0.0519)
3 years before	-0.0103	-0.0109	-0.0098
	(0.0379)	(0.0420)	(0.0476)
2 years before	0.0057	0.0065	0.0038
	(0.0328)	(0.0359)	(0.0476)
1 year before	0.0566	0.0613	0.0646
	(.0406)	(0.0442)	(0.0501)
Year of implementation	0	0	0
	(.)	(.)	(.)
1 year after	-0.0698**	-0.0762**	-0.0861***
	(0.0268)	(0.0288)	(0.0304)
2 years after	-0.1216***	-0.1279***	-0.1467***
	(0.0427)	(0.0460)	(0.0473)
3 years after	-0.1649***	-0.1714***	-0.1948***
	(0.0578)	(0.0619)	(0.0635)
4 years after	-0.1909***		-0.2055***
	(0.0717)		(0.0698)
Controls:			
State Controls	Y	Y	Y
State FE	Y	Y	Y
Year FE	Y	Y	Y
R^2	0.8893	0.8887	0.8892
Clusters	51	50	49
Observations	867	850	833

Note: The dependent variable is grams of Morphine Equivalent (ME) units per person.
ME was calculated using the procedure outlined in Section V. The 3 year panel drops Wisconsin.
The four year balanced panel drops Wisconsin and Georgia. *** p<.01, ** p<.05, *p<.10

Table A8: NVSS Prescription Opioid Sensitivity Test - Imputation

	(1) log deaths	(2) log deaths
4 years before	0.0070	0.0004
	(0.1476)	(0.1336)
3 years before	0.0908	0.0951
	(0.1337)	(0.1035)
2 years before	0.0444	0.0683
	(0.1179)	-0.0995
1 year before	0.0348	0.0331
	(0.0995)	(0.0833)
Year of implementation	0	0
	(.)	(.)
1 year after	-0.0326	-0.0614*
	(0.0359)	(0.0346)
2 years after	-0.1115*	-0.1223**
	(0.0568)	(0.0467)
3 years after	-0.1370	-0.1646*
	(0.1065)	(0.0824)
4 years after	-0.1198	-0.1143
	(0.1169)	(0.089)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
Impute	0	5
R^2	0.8856	0.940
Clusters	51	51
Observations	918	918

Note: Dependent variable is ln(Deaths+1). Data comes from NVSS Mortality Files and includes instances with codes T40.2-T40.3.
*** p<.01, ** p<.05,*p<.10

Table A9: NVSS Heroin Sensitivity Test - Imputation

	(1) log deaths	(2) log deaths
4 years before	-0.4965	-0.4060
	(0.3738)	(0.2505)
3 years before	-0.3196	-0.1828
	(0.2966)	(0.1677)
2 years before	-0.0622	-0.1833
	(0.1705)	(0.1569)
1 year before	-0.2473	-0.1582
	(0.2432)	(0.1115)
Year of implementation	0	0
	(.)	(.)
1 year after	0.2118	0.1101
	(0.2040)	(0.0864)
2 years after	0.3808	0.2081*
	(0.2537)	(0.1089)
3 years after	0.3189	0.1843
	(0.2971)	(0.1450)
4 years after	0.3078	0.1938
	(0.2885)	(0.190)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Year FE	Y	Y
Impute	0	5
R^2	0.8898	0.9143
Clusters	51	51
Observations	918	918

Note: Dependent variable is ln(Deaths+1). Data comes from NVSS Mortality Files and includes instances with code T40.1.
Suppressed data was imputed to 0. *** p<.01, ** p<.05, *p<.10

Table A10: TEDS Robustness - Alternate Specifications

	(1) log admissions	(2) log admissions
4 years before	-0.0651	0.0017
	(0.1537)	(0.1334)
3 years before	0.0826	0.1049
	(0.1013)	(0.1075)
2 years before	-0.1048	-0.0730
	(0.0910)	(0.1059)
1 year before	-0.1570	-0.1479
	(0.1210)	(0.1240)
Year of implementation	0	0
	(.)	(.)
1 year after	-0.1221**	-0.1848***
	(0.0499)	(0.0467)
2 years after	-0.2393***	-0.2908***
	(0.0856)	(0.0958)
3 years after	-0.3217***	-0.3261**
	(0.1019)	(0.1246)
4 years after	-0.3553***	-0.3589**
	(0.1309)	(0.1631)
Controls:		
State Controls	N	Y
State FE	Y	N
Year FE	Y	N
State Trends	N	Y
R^2	0.9165	0.9109
Clusters	51	51
Observations	887	887

Note: The dependent variable is admissions to substance use disorder treatment. *** p<.01, ** p<.05, *p<.10