



Research Article

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The Intended and Unintended Effects of Opioid Policies on Prescription Opioids and Crime

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Abstract: In response to the opioid crisis, US states have implemented policies to reduce the dispensing of opioids and curb drug mortality. Exploiting a long panel of county-level data, we analyse the combination of demand- and supply-side state opioid policies and evaluate their effect on opioids per capita dispensed and their unintended fallouts on drug-related crime. We demonstrate that only laws targeting the supply for opioids reduce the volume of prescribed drugs, while demand-side policies are less effective. We also emphasize that within supply-side state regulations, Pain Management Clinics Laws are the most successful in reducing the dispensation of prescription opioids. Remarkably, the drop in opioids distributed due to supply-side regulations is accompanied by negative externalities in the local market for illicit drugs.

Keywords: crime, drugs, opioid laws, prescription opioids

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1 Introduction

Since the late 1990s, the rapid escalation in the use of prescription and non-prescription opioid-based drugs in the US has originated the so-called opioid epidemic, the deadliest drug overdose crisis in American history. According to the Centres for Disease Control and Prevention (CDC), yearly deaths from a drug overdose in the US have increased five-fold since 1999, reaching 63,632 victims in 2016 only, i.e. more than those caused by car crashes and gun violence in the same year (Hedegaard, Warner, and Miniño 2017). In response to this dramatic crisis, many US states have progressively enacted several laws that restrict the prescribing and the dispensing of controlled substances and promote access to emergency services in case of opioid overdose.

In this paper, we assess the impact of a wide set of opioid state laws on the quantity of prescription opioids dispensed and on drug-related arrest rates. Understanding the relative importance of each type of law can provide useful insights to policy makers, the more comprehensive the analysis. Yet, research on their effectiveness has produced conflicting results.¹

We draw on a number of opioid-related policies adopted in the US over the past decades, namely *Prescription Drug Monitoring Programs* (PDMP), *Pain Management Clinics Laws* (PMCL) and *Doctor Shopping Laws* (DSL). Some of these regulations aim at reducing the amount of prescription opioids dispensed either on the supply side (PMCL and PDMP) or the demand side (DSL) of the market for drugs, depending on whether they impose restrictions on the prescribers or the patients.²

In order to offer an extensive view of the dynamics occurring in this context, we first consider the combination of demand- and supply-side opioid state laws simultaneously and evaluate the effect of each type of regulation taking into account the impact of the other regulations. Using a difference-in-differences set-up and linking various sources of county-level panel data, we exploit the staggered timing in the implementation of these laws across US states to identify their causal effect on the number of opioids dispensed over the period 2001–2016. While the intention of the policy makers is aimed at reducing the abuse of prescription drugs,

¹ This is possibly due to the heterogeneity in the set of regulations considered. There is some controversy on how implementation dates are chosen by researchers, especially in the case of Prescription Drug Monitoring Programs (Davis 2017; Horwitz et al. 2018).

² As Ruhm (2019) notes, the terms “supply side” and “demand side” must be interpreted with caution because, in the case of addictive products such as opioids, supply-driven increases in dispensation will possibly raise contemporaneous demand and vice versa.

we examine the overall amount of opioid-based active ingredients distributed at the local level under the assumption that the higher their dispensation, the higher the potential rate of abuse.

We demonstrate that the implementation of supply-side state laws reduces the quantity of prescription opioids per capita dispensed at the county level. In terms of magnitude, PDMP and PMCL yield, on average, a 4 and 15% reduction in the per capita drug units dispensed, respectively. The former provide for the implementation of databases that monitor the prescription and dispensation of controlled substances. The latter set minimum requirements for pain management clinics to operate. On the contrary, interventions regulating the demand-side of the market, by obliging patients to disclose information on their prescription history to health care professionals (DSL), do not produce an overall appreciable statistical impact on prescription rates. Such weak response casts doubts on the real efficacy of this type of intervention.³

We also provide some first evidence on the unintended spillovers occurring between changes in opioid legal dispensation and criminal activities. Other studies point out that drug misuse correlates with adverse fallouts of various nature (Hansen et al. 2011), and opioid state laws have been shown to have indirect effects on suicides (Borgschulte, Corredor-Waldron, and Marshall 2018), neonatal abstinence syndrome births (Gihleb, Giuntella, and Zhang 2020a) and foster care admissions (Gihleb, Giuntella, and Zhang 2020b). Yet, although lawmakers have designed these regulations to limit the misuse of legally prescribed opioids, it is still unclear whether they might generate spillovers on the illicit market of drugs, given the links between drug misuse and crime (Dave, Deza, and Horn 2018; Dobkin, Nicosia, and Weinberg 2014; Doleac and Mukherjee 2018; Mallatt 2017; Meinhofer 2017).

Despite the legal deterrents against selling controlled substances without authorisation or possessing controlled substances without a prescription, the illegal market remains a relevant source of prescription opioids for many users. In fact, the inappropriate and unnecessary quantity of prescription drugs dispensed often translates in a large amount of pills that are diverted to family members or friends of patients or to the black market.⁴ While restricting the availability of

³ Alpert, Powell, and Pacula (2018) acknowledge that only a few funds have been directed to demand-side regulations and that only recently the specific budget has been expanded to \$181 million to increase prevention and addiction services (Comprehensive Addiction and Recovery Act, 2016).

⁴ Non-medical opioid users typically find their habitual dose of drug: from dealers/strangers (4.3%); from the internet (0.1%); by other means (4.4%); from doctors (23.8%); buying/taking it from friends or relatives (14.6%); and free from friends or relatives (53%). Those in the latter category report that their friends or relatives obtained the drugs from doctors themselves 87% of the times (Meinhofer 2017).

excess opioid drugs can reduce misuse and, in turn, lead to better health and a potential decrease in crime, it may also be that users who face obstacles in obtaining prescription opioids turn to the black market for substitutes.⁵ Indeed, previous studies document an increase in consumption of similar or even more harmful opiates (e.g. heroin) following shocks to the supply of legally available opioid drugs (Alpert, Powell, and Pacula 2018; Evans, Lieber, and Power 2019).⁶

Along these lines, existing works find evidence compatible with substitution for other illegal drugs (Mallatt 2017; Meinhofer 2017) and, more generally, higher propensity to commit crime (Dave, Deza, and Horn 2018; Doleac and Mukherjee 2018), as opioid state laws lower the availability of legally prescribed drugs. Our results suggest that reducing the legal availability of these drugs may have unintentional negative externalities on the illegal market for drugs. In particular, we observe a significant response to the enforcement of *Pain Management Clinics Laws* on arrest rates for the sale of opium, cocaine, their derivatives and for synthetic narcotics.

With this paper, we contribute to the recently economic-oriented literature on the effects of opioid state laws on the quantity of legally dispensed prescription opioids and drug-related crime. Compared to the existing analyses, we examine the relative effectiveness of both supply- and demand-side opioid laws in reducing the amount of drugs dispensed. Our analysis includes the assessment of the effects of two largely under-studied sets of regulations (i.e. PMCL and DSL) and sheds light on the potential unintended effect of health policies on a broader domain, namely the market for illicit substances.⁷ With respect to the existing works, we deliver results on drug arrests covering the entire US population for a 16-year long period, a larger set of supply and demand-side opioid state laws.

⁵ That is because opioid laws yield an increase in the price of legally accessible opioids and a decrease in the quantity demanded. However, the demand for drugs typically is very inelastic. Responses to policy interventions might arise either via new producers taking advantage of high prices and entering the market (or increasing production) or via consumers obtaining alternative drugs (Alpert, Powell, and Pacula 2018).

⁶ Similar substitution mechanisms also occurs in the context of other illicit drug markets (Dobkin and Nicosia 2009). Also, Powell, Pacula, and Jacobson (2018) show that expanding the legal availability of marijuana decreases abuse of opioid because of substitution.

⁷ The number of empirical contributions assessing the effects of opioid state laws is rapidly growing and most works focus on health-related issues or belong to the medical literature (Ali et al. 2017; Brady et al. 2014; Grecu, Dave, and Saffer 2019; Haegerich et al. 2014; Meara et al. 2016; Paulozzi, Kilbourne, and Desai 2011; Yang et al. 2015). Early contributions to the economic literature do not find consistent evidence on the impact of opioid-related regulations on mortality rate and hospital admissions (Buchmueller and Carey 2018; Kilby 2015; Popovici et al. 2017; Rees et al. 2019). With some exceptions, for instance Meara et al. (2016) and Popovici et al. (2017), previous works that analyse the efficacy of opioids state laws typically focus only on one or two regulations at a time.

The remainder of the paper is organised as follows. Section 2 describes the policies implemented in the past decades to curb prescription rates and the effects of the opioid crisis. Data and empirical strategy are presented in Section 3. In Section 4 we present our results on the quantity of opioids dispensed and on the spillovers on criminal activities. Section 5 concludes.

2 Opioid State Laws and Their Potential Effects

The reaction of the policy makers to the opioid crisis has come mainly at the state level, with the implementation of several laws in different states at different times. The target of these policies varies in terms of the individuals involved (patients, prescribers, pharmacists, physicians) and of the types of limitations or incentives. Specifically, these regulations can be grouped into two main categories, supply and demand laws, which are described in Table 1.

Following this classification, we construct a dataset that summarises the date of adoption of the opioid state laws in the years 2001–2016 (Table A.1).⁸ The timing

Table 1: State laws on prescription opioids.

Law	Name	Description
Supply-side laws		
PDMP	Prescription drug monitoring programs	Implementation of systems that collect information on prescriptions of controlled substances and that allow physicians and pharmacists to view a patient's prescribing history.
PMCL	Pain management clinics laws	Sets of regulations concerning the minimum requirements for a pain management clinic to be allowed to dispense prescription drugs.
Demand-side law		
DSL	Doctor shopping laws	Obligation for patients to reveal to a health care practitioner about previous prescriptions received from other doctors and prohibition to obtain drugs through fraud, deceit, misrepresentation, etc.

⁸ We are aware of possible dates misspecification, especially in light of Horwitz et al. (2018). Thus, we perform a battery of robustness checks using the dates they propose, as discussed in Section 4. We also employ similar variables as computed by Popovici et al. (2017, Table 1, p. 4 and Table 5, Appendix), Meara et al. (2016, Online Appendix), Rees et al. (2019, Tables 2, p. 8) and Buchmueller and Carey (2018, Table 1, p. 85) for robustness. Despite referring to different (or smaller) samples, when we use indicators from these other sources we obtain comparable results.

of their implementation across US states is summarised in Figure A.1, while Figure A.2 shows the geographical distribution of the laws enacted by 2016. In what follows, we explain the details of each set of laws and we briefly outline their predicted impact on the outcomes.

2.1 Supply-Side Laws

Prescription Drug Monitoring Programs (PDMP) represent the most common and well-studied supply-side policy.⁹ Since the early 2000s, PDMP have been increasingly implemented across US states. Full national coverage has been reached in 2017 with Missouri, the last state to adopt this type of regulation. It consists of state-level databases that monitor the prescription and the dispensing of controlled substances. The information contained in the system is available to all authorised health-care providers including physicians and pharmacists to prevent improper drug prescription or dispensation.¹⁰ In some states, under certain circumstances, prescribers and dispensers are required to access PDMP by law (hence, called “must-access” or “mandate”), while in others the use of this system is “non mandated”.

Pain Management Clinics Laws (PMCL) embody all regulations aimed at preventing inappropriate prescribing and dispensing of controlled substances within clinics specialised in pain management. These clinics have been such a great source of prescription drugs that they are sometimes called “Pill Mills”. They have become such a serious issue in the context of the opioid crisis that PMCL have been implemented in one every five states since the mid-2000s. Although there is some heterogeneity across states, regulations associated with PMCL typically provide for requirements concerning the ownership, the licensing procedures, the operational standards and the personnel qualification of pain management clinics, facilities or practice locations. These interventions have resulted in a massive shutdown of pain management clinics that did not meet the new standards (Mallatt 2017).

Both PDMP and PMCL induce a shock on the supply side of the market for prescription opioids because they provide for restrictions to the agents supplying

⁹ See Haegerich et al. (2014) and Horwitz et al. (2018) for a review of the evaluation literature on PDMP state interventions.

¹⁰ Access is also granted to law enforcement warrantless in many states. In some cases, it might be that PDMP also affects the demand for prescription opioids, especially when users want to avoid physicians and pharmacists that operate using the database.

drugs (physicians and pharmacists).¹¹ As a consequence, we expect the two policies to have a negative effect on the volume of legally-dispensed drugs. Moreover, this drop might be accompanied by an increase in drug-related crimes due to users turning to the black market in search of drugs. However, the reduced availability of drugs from the legal channel may yield a shortage in the illegal market, hence contributing to a decline in arrests for drug sale or possession.

2.2 Demand-Side Laws

Doctor Shopping Laws (DSL) are also directed at limiting the amount of opioids dispensed but they involve the demand side of the market for drugs, as they impose restrictions on patients rather than on suppliers. They refer to any regulation that prohibits doctor shopping, i.e. the practice of obtaining controlled substances from multiple healthcare practitioners. The number of states that have adopted these laws has doubled since the year 2000 and is currently around a third of the total. DSL limit a patient's ability to seek medications from multiple providers and prohibit withholding of any information that may be relevant to the physician or the pharmacist.

Theoretically, curtailing access to prescription drugs for non-medical use in this manner is potentially effective as health care providers are the most common source of opioids used non-medically (Substance Abuse and Mental Health Services Administration, 2014). Moreover, the previous medical literature has found a positive relationship between doctor shopping practices and overdose mortality risk (Peirce et al. 2012). Thus, this set of regulations is expected to negatively affect the amount of opioids available on the market from the demand side, especially when prescriptions are unnecessary or excessive. Nevertheless, in the absence of systematic surveillance and large heterogeneity in the legal implementation across US States, the regulation could turn out to be weakly effective, since heavy users especially might have a strong incentive not to disclose the relevant information to health care professionals to obtain more painkillers than necessary.

¹¹ Tamper-resistant forms regulations, which typically require prescribers to write their prescriptions on tamper-resistant pads, would also fall into the supply-side regulations. Yet, the application of PDMP across US states in recent years has made the provision of tamper-resistant forms less binding, given that with the implementation of PDMP doctors and pharmacists are able to monitor the prescribing and dispensing histories of patients. We account for their enforcement in the robustness checks in Section 4.

3 Data and Empirical Strategy

In this section we describe how we combine various sources to build our main dataset. Then, we illustrate the empirical strategy and provide some descriptive statistics.

3.1 Data Sources

The data on prescription opioids comes from the Automation of Reports and Consolidated Orders System (ARCOS), which is run by the Office of Diversion Control of the US Drug Enforcement Administration. Since the Controlled Substance Act of 1970, manufacturers of controlled substances are required to provide information on the amount of drugs produced and dispensed in the US. The yearly ARCOS reports provide a record of the quantities (in grams) of each controlled active ingredient dispensed in the US. This information is disaggregated at the three-digit zip code level across all US states.

We consider a set of opium-based active ingredients available in ARCOS, namely morphine, oxycodone, hydrocodone, hydromorphone, methadone, meperidine, and fentanyl classified as Schedule II or Schedule III drugs.¹² We build an overall indicator that accounts for the relative potency of these drugs so that each drug is converted into Morphine Gram Equivalent units (MGEs).¹³ Since it considers the overall amount of opium-based active ingredients, this represents our main indicator to quantify the dispensation of prescription opioids.¹⁴

Then, we link our dataset to the Uniform Crime Reporting (UCR) Program Data provided by the Federal Bureau of Investigation, which contains the number of arrests disaggregated by county and by type of crime. We take into account

¹² The list of drugs come from Brady et al. (2014, Table 1, p. 142). The order of the Schedule decreases with the abuse potential of the drug. For instance, heroin is classified as a Schedule I substance, while Schedule V drugs include coughing preparations with less than 200 mg of codeine per 100 ml. Schedule II and Schedule III substances are considered to have a high to moderate potential for abuse, respectively, and to lead to psychological or physical dependence.

¹³ Unfortunately, ARCOS data do not distinguish between the route of administration of the substances, which in some cases can change the relative potency of the drugs. In choosing the multipliers to convert into MGE units we follow Gammaitoni et al. (2003), Paulozzi, Kilbourne, and Desai (2011), and Brady et al. (2014). Thus, we rescale substances according to the following: morphine by 1, oxycodone by 1, hydrocodone by 1, hydromorphone by 4, methadone by 7.5, meperidine by 0.1 and fentanyl by 75. In addition, we are aware of the data limitation of ARCOS, in that it may overstate the amount of drugs eventually consumed because not all dispensed drugs are used by patients.

¹⁴ The use of an overall indicator also allows mitigating multiple hypothesis testing concerns.

drug-related crimes that involve the possession and selling of different substances such as opium, cocaine, marijuana and other synthetic drugs.

Finally, we match the information from ARCOS to the official population intercensal estimates at the county level, which include counts of the overall population and by sex, age band and race/ethnicity group.¹⁵ The US Census is also the source of all the data used in the heterogeneity analysis about education, health care insurance coverage and employment in health services (County Business Patterns), while income comes from the Bureau of Economic Analysis. Drug and alcohol mortality data are drawn from the Global Health Data Exchange of the Institute for Health Metrics and Evaluation (University of Washington).

Our final sample comprises 3127 counties across the US that we follow during the period 2001–2016. To our knowledge, this is the first paper evaluating the effects of supply- and demand-side opioid state laws on the volume of prescribed opioids and on drug-related crime rates that exploit such an extensive dataset, both in terms of time span and of geographical coverage at the county level.

3.2 Empirical Model

We employ a typical regression difference-in-differences setting such that:

$$Y_{cst} = \alpha + \beta L_{st} + \mu M_{st} + \delta_t + \gamma_c + \theta_{rt} + \epsilon_{cst}, \quad (1)$$

where Y_{cst} is the outcome of interest measured in county c , in state s and in year t . The set L_{st} includes dummy variables for each law as from Table 1, which take value 1 when the regulation is in force in a given state and 0 otherwise. Hence, the coefficient β corresponds to the treatment effect of interest, as it captures the effect of regulation on different outcomes while controlling for the other laws. We analyse such effect on the quantity of drugs distributed and on drug-related crime.¹⁶

We acknowledge the contemporaneous implementation of other state regulations, specifically aimed at reducing opioid overdose mortality (namely, Naloxone Access Laws and Good Samaritan Laws) by adding a set of two dummy variables (M_{st}). Their role in this context is discussed in Appendix B. Moreover, we include county (γ_c), year (δ_t) and region-year (θ_{rt}) fixed effects to control for fixed

¹⁵ We use the 2000 and the 2010 zip-to-county crosswalks produced by the MABLE/Geocorr Application of the Missouri Census Data Center.

¹⁶ All variables are transformed into logarithms, as AIC and BIC yield to the smallest values. Outcome variables for drugs are expressed in grams per capita. We add one to the count of criminal activities to circumvent sample selection issues that would emerge from deleting observations with no reported crimes. Crime rates are expressed as per 100,000 residents.

heterogeneity at local level, at time and region-by-year fluctuations, respectively. Errors are clustered at the state level.¹⁷

A critical assumption for our identification strategy is that states that enact opioid laws and those that do not adopt them behave similarly in the pre-implementation period, to ensure that the enactment of the laws is not endogenously related to trends in opioid prescriptions. We already control for time, county and region-year heterogeneity, but the event-study analysis approach helps to check for pre-existing trends, i.e. we verify the existence of *parallel trends*. This posits that the average change in the comparison group represents the counterfactual change in the treatment group if there were no treatment. If the leads in the event-study analysis are not statistically different from zero, this implies that the treated counties are trending similarly to the untreated counties prior to the policy, and this constant heterogeneity vanishes in difference. Thus, the identifying assumption of the differences-in-differences model would be supported. Hence, we also estimate the following equation:

$$Y_{cst} = \alpha + \sum_{\pi=-5}^{-1} \beta_{l,\pi} L_{l,st+\pi} + \sum_{\tau=1}^5 \beta_{l,\tau} L_{l,st+\tau} + \beta_{-l} L_{-l,st} + \mu M_{st} + \delta_t + \gamma_c + \theta_{rt} + \epsilon_{cst}, \quad (2)$$

which allows for five pre- and five post-treatment effects for each law l , while still controlling for the enforcement of all the other laws ($-l$). According to this specification, the baseline year is the one before the implementation of law l , while leads and lags are identified by the coefficients $\beta_{l,\pi}$ and $\beta_{l,\tau}$, respectively. Here, the β associated to $\pi = -5$ and $\tau = 5$ include all periods prior to $t - 5$ and after $t + 5$, respectively. If the leads $\beta_{l,\pi}$ are not statistically different from zero we can assume that the parallel trends assumption holds. The $\beta_{l,\tau}$ coefficients, instead, allow us to examine whether the treatment effect of law l fades, increases or stays constant over time. Additionally, a battery of robustness checks in support of our identification strategy is presented in Section 4.1 where we discuss potential confounding effects.

3.3 Descriptive Analysis

Figure 1 shows the raw average of MGE units per capita dispensed by year since the introduction of each policy. The portion to the left of the dashed vertical line corresponds to the years prior to the onset of each law. The graph depicts a constant increase in the average amount of drugs dispensed per capita, which is only

¹⁷ This derives from the treatment being at state level. Nonetheless, clustering the errors at county level provides even better results in terms of statistical power of our estimates.

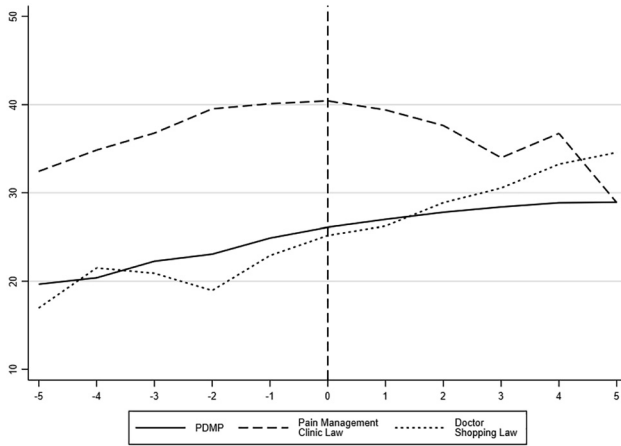


Figure 1: Average MGEs dispensed by year since/to the introduction of the policies.
 Note: The labels in the x-axis are such that the zero, the negative and the positive values correspond to the year of, the years before and the years after the introduction of each policy, respectively. Treated states only (49 for PDMP, 10 for PMCL, 20 for DSL).

slowed down after the introduction of the policies (i.e. to the right of the dashed line). The only exception to this inversion in trend seems to be associated with DSL, for which we do not observe any change in slope.

Table A.2 reports the descriptive statistics of the main outcomes and control variables used in the analysis. Drug quantities are expressed in MGE units per capita to take into account the relative potency of each drug component. Overall, a total of 704 kg of prescription opioids (i.e. 30 g per capita) are dispensed in each county every year. Between 2001 and 2016 the total county-level average of MGE units has increased almost three-fold from 336 to 746 kg. The most commonly dispensed substances are morphine, methadone and hydrocodone, with around 13, 5 and 4 g per capita, respectively.

Figure A.3 in the Appendix describes the geographical distribution of the average MGE units in the years 2001 and 2016 (top and bottom panels, respectively). It is worth noting that had we considered the 2001 quartile distribution, we would have obtained an almost entirely red map for the year 2016. As a matter of fact, the median of the MGE units per capita distribution in 2016 is more than double the one in 2001 (14.51 and 7.08 MGEs per capita, respectively). For this reason, we construct the percentile thresholds based on the average distribution of MGE units per capita in the period 2001–2016. The colder (darker blue) areas, the lower the levels of POs per capita. Vice versa, the warmer (darker red) the area, the higher the dispensation of MGE units. Nevertheless, we observe a remarkable

variation both across years and counties. The map for 2016 is much warmer compared to the one for 2001, which indicates that the dispensation of opioid analgesics per capita rises during the period analysed. Besides, the maps display a clear heterogeneity both within and across states.

4 Results

In this section we present our results. First, we assess whether, and to what extent, the policies under analysis yield a reduction in the amount of MGE units per capita dispensed in each county. Then, we estimate their unintended impact on drug-related crimes.

4.1 The Effect of State Laws on Prescription Opioids

Table 2 shows the main results on the amount of MGE prescription drugs dispensed in each county. In columns 1 to 4 we consider one set of state laws at a time: PDMP, PCML and DSL. Column 1 presents the effect of PDMP alone, while in column 2 we include an interaction term that accounts for the adoption of Mandate PDMP. In line with the existing literature, we find that the effect of Mandate PDMP is

Table 2: Effect on drug quantities.

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
	MGEpc	MGEpc	MGEpc	MGEpc	MGEpc	MGEpc
PDMP	-0.040 (0.025)	-0.045* (0.025)			-0.038* (0.020)	-0.041* (0.021)
Mandate PDMP		-0.085** (0.037) [-0.130**]				-0.035 (0.024) [-0.075**]
Pain management clinic law			-0.150*** (0.028)		-0.152*** (0.026)	-0.141*** (0.026)
Doctor shopping law				0.010 (0.043)	0.037 (0.036)	0.038 (0.035)
Observations	50,032	50,032	50,032	50,032	50,032	50,032
R-squared	0.987	0.987	0.987	0.987	0.987	0.987

The dependent variable is the natural log of MGE per capita. Population-weighted OLS estimates, where the weight is computed as the share of the population in the county relative to the national population. All regressions include two dummy indicators capturing the enforcement of Naloxone Access Laws and Good Samaritan Laws, plus year, county and region-year fixed effects. Errors are clustered at the state level. Coefficient in square brackets is associated to $\beta_{PDMP} + \beta_{MandatePDMP}$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

substantially higher than that of the non-compulsory PDMP. Coefficients suggest that ordinary PDMP reduces the quantity of MGEs by 4.5% while Mandate PDMP yields a further drop by 8.5%. Conversely, the overall impact of Mandate PDMP on MGE units per capita consists of a decrease by 13%. The coefficient associated to PMCL suggests that imposing more stringent operational restrictions to pain management clinics determines a decrease in the amount of MGE units dispensed by 15% (column 3). Conversely, in column 4, DSL, which compel patients to reveal to health care professionals whether they had already be prescribed or administered prescription drugs, does not appear to have any meaningful impact on the quantity of per capita MGE units.

When we consider all sets of laws in the same model (columns 5 and 6), coefficients maintain the same sign and significance, with the exception of the one capturing the differential between PDMP and Mandate PDMP. Given that the different types of PDMP do not differ in their effect on the quantity of MGEs distributed, column 5 is our main specification.

Our estimates suggest a reduction in prescription opioids per capita following the enforcement of the state laws that aim at reducing abusive behaviour on the supply side, namely PDMP and PMCL. The negative impact in column 5 corresponds to an average reduction in the amount of MGE units per capita by almost 4% following the introduction of PDMP and by more than 15% after the enactment of PMCL.¹⁸ Given that the average amount of MGEs per capita in the sample is 29.73, these effects translate in a drop by around 1.14 and 4.52 g per capita, respectively. Our results are in line with those of Mallatt (2017) and Meinhofer (2017), who estimate the negative effects of PDMP and PMCL on the total amount of oxycodone dispensed to be around 8 and 17%, respectively. The absence of effects predicted by DSL might be due to its weak implementation as well as to the fact that demand-side interventions generally receive less funding and attention compared to supply-side policies (Alpert, Powell, and Pacula 2018).

We provide evidence on the validity of our estimates with an event-study approach, which allows estimating lagged effects while testing for the absence of pre-existing trends. This is shown in Figure 2. The plots suggest the existence of parallel trends between treated and control units, as the coefficients in the pre-treatment period are never statistically different from zero. This points to the absence of a plausible systematic pattern in the distribution of MGE units per capita before the introduction of any opioid state law, which also allows us to exclude potential anticipation or announcement effects.

¹⁸ Double clustering at state and year levels yields to identical results. Using wild bootstrap procedure with 100 replications, our conclusions do not change. We find that the standard errors associated to PDMP and PMCL are 0.025 and 0.034, respectively.

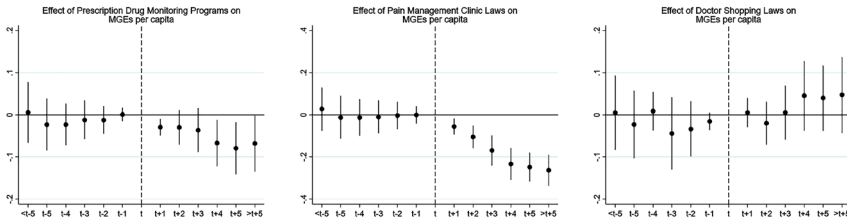


Figure 2: Event-study analysis: effect on drug quantities.

Note: Coefficients estimated as in Eq. (2). The coefficient associated to $t - 5$ pertains to all periods prior to the fifth year before the implementation of the law. The coefficient associated to $t + 5$ refers to all years from the fifth after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

Figure 2 also highlights a small but persistent negative impact of PDMP on the outcome, while PMCL yield a sharp and increasingly large decrease in the quantity of MGE units per capita.¹⁹ The introduction of DSL does not have any impact on dispensation rates. If anything, DSL might bring about a mild increase in the amount of MGE units per capita dispensed, although coefficients are never statistically significant.²⁰

As we are in the presence of staggered time law implementation, we apply the decomposition of our estimates in the spirit of Goodman-Bacon (2018), based on comparisons across different treatment groups (namely, always treated, never treated and units that switch from being untreated to treated). Here, weights are based on the size of each treatment sub-group and on the variance of the treatment, which in turn depends on how distant is the onset of the treatment from the start and the end of the observational window. We apply the procedure to our three main estimates of the impact of opioids laws on the quantity of Morphine Gram Equivalent units. The decomposition exercise in Table 3 shows that the estimated effect for the PMCL coefficient is almost entirely driven by the comparison between never-treated units to those that enact this type of regulation starting from 2009. The comparison across treated units is much less relevant, although the sign of the coefficient is in line with the main estimate. For what concerns PDMP, the coefficient of -0.038 is derived by the comparison across treated units over time (timing groups and always-treated versus timing groups). While this is expected, given

¹⁹ Figure D.1 (left plot) also presents the event-study for the total effect of Mandate PDMP on MGE units per capita.

²⁰ In the Online Appendix we provide identical evidence on quarterly level data (Table D.1 and Figure D.2). We also test whether the effects are driven by outliers so we run the model excluding one state at a time (Figure D.3). Additionally, we exclude the states that implement multiple opioid laws in the same year (39.90% of the sample). In both cases, we find consistent results.

Table 3: Effect on drug quantities: Goodman-Bacon decomposition.

	PDMP		Pain management clinic law		Doctor shopping law	
	β	Weight	β	Weight	β	Weight
Timing groups	-0.026	0.254	-0.051	0.080	-0.007	0.085
Always versus timing	-0.080	0.682			0.007	0.249
Never versus timing	0.062	0.028	-0.177	0.866	-0.047	0.623
Always versus never	-0.374	0.000			1.807	0.001

Decomposition of the estimated effects performed as proposed by Goodman-Bacon (2018) and using the Stata command `bacondecomp` (Goodman-Bacon, Goldring, and Nichols 2019). Groups are defined as follows: *Timing* are units that become treated during our period of observation; *Always* are always treated; *Never* are never treated counties.

that 49 states out of 50 are eventually treated in our sample, it is reassuring that the estimated effect is not driven by the comparison with the sole state that is never treated (Missouri, which implemented PDMP in 2017). Finally, the results on Doctor Shopping Laws appear less precise, although the comparison between treated (timing groups) and never-treated suggests that, if anything, the effect should be slightly negative.

4.2 Robustness Checks

Table A.3 shows a set of robustness checks. In column 1 we account for additional changes in the institutional framework which might potentially interfere with the dispensation of prescription opioids and bias our estimated effects: the introduction of Medicare Part D in 2006 and the reformulation of OxyContin in 2010. Medicare Part D is a federal program that subsidizes the costs of prescription drugs and of prescription drug insurance premiums for Medicare beneficiaries. Thus, it might disproportionately compensate for the incidence of the policies on the amount of prescription opioids dispensed in areas with a larger number of beneficiaries. We proxy the exposure to the program with the share of people aged 65 or over at county level, interacted with a dummy variable that takes value 1 in the years after 2006 and 0 otherwise as in Powell, Pacula, and Taylor (2015).

OxyContin was reformulated in 2010 with the intent of making it more difficult to abuse this drug.²¹ Alpert, Powell, and Pacula (2018) and Evans, Lieber, and

²¹ OxyContin was one of the most popular oxycodone-based prescription drugs in the US since its release in 1996. Its new abuse-deterrent version was specifically designed to avoid crushing or dissolving of the pill and reduce abusive behaviour.

Power (2019) show that its reformulation has been followed by a significant drop in the prescribing rates of this drug. At the same time, however, they find evidence of substitution towards other opioid-based substances, especially fentanyl and heroin. Hence, we include an interaction between the amount of oxycodone dispensed in each county in 2000 and a dummy that takes value 1 in the years after 2010 (Alpert, Powell, and Pacula 2018; Evans, Lieber, and Power 2019; Mallatt 2017). We obtain comparable results with the inclusion of such indicators, which suggests that these changes to the institutional framework do not influence the estimated impact of the laws on the amount of dispensed prescription opioids per capita.²²

In column 2 of Table A.3 we include a set of time-varying demographic indicators to rule out, or at least alleviate, the possibility that trends in the composition of the population may be correlated with the propensity of a state to institute specific opioid regulations. The absence of confounding effects is confirmed by the fact that the coefficients of interest are not statistically different from the main specification. In column 3 we estimate a model in which state-specific linear time trends are estimated only based on the pre-treatment periods and groups. We partial out pre-treatment trends after de-meaning all the dependent and independent variables following the procedure of Goodman-Bacon (2018), and, reassuringly, we obtain estimates that are almost identical to our main effects.²³ In the next columns, we include linear and quadratic trends based on the initial level of MGE units per capita and state-specific linear and quadratic trends.²⁴

22 In a further check, we include an indicator for the enactment of state blood alcohol content laws, because alcohol and prescription drugs are often co-abused, especially by young adults (McCabe, Cranford, and Boyd 2006). We obtain identical results. Also, adding a control for the provision of tamper-resistant forms does not change our results on the quantity of drugs distributed (years 2000–2012). Moreover, the dummy associated to tamper-resistant forms is never statistically different from zero (0.032 with s.e. 0.020). Finally, in Appendix B we show results concerning the effect of Naloxone Access Laws and Good Samaritan Laws, which are intended to offer incentives to seek medical assistance in case of overdose emergencies.

23 We can only apply the test for the indicator on PMCL because this is the only case in which we do not observe always-treated units.

24 The initial level of MGE units per capita is coded as low, medium or high based on terciles. We also include county-specific linear and quadratic trends to capture specific trends at the local level (such as alcohol consumption) that might confound the main effects. Additionally, we substitute the dummy variables for the opioids state laws with four categorical indicators that take value 0.25, 0.5, 0.75 and 1 depending on whether each law is enacted in the first, second, third or fourth quarter of a given year. Moreover, we check that our results are robust to the use of non-weighted estimation. In all cases, the estimated coefficients are similar to the main specification (the coefficients associated with PDMP lose significance but magnitudes are unchanged). Last, comparable results are obtained when the dependent variable is expressed in levels.

In column 8 we exclude methadone from the dependent variable. Methadone is considered clinically different from other prescription opioids and often used in the treatment of opioid and heroin addiction in replacement therapies (Paulozzi 2012). Yet, in some states methadone is also one of the most widely diverted and abused drugs (Cicero and Inciardi 2005; Jones 2016). Coherently, the magnitude and significance of the coefficients imply that the enactment of state laws limiting the dispensing of opioids on the supply-side (PDMP especially) disproportionately impacts on the amount of methadone dispensed. Unfortunately, whether this is due to abuse rates falling or to a change in prescribing practices by suppliers cannot be tested here. DSL, if anything, are associated with an increase in the volume of opioids other than methadone, possibly because the detection of “ordinary” patients performing doctor shopping is more difficult for health care professionals in good faith, while methadone users are subject to stricter control.

We also investigate the existence of potential spillovers from the local labour market performance. In the spirit of Pei, Pischke, and Schwandt (2019), we test whether employment and unemployment rates are correlated with the implementation of opioid state laws, finding that the coefficients associated with opioid regulations are not different from zero (Table D.2). Moreover, if we add employment and unemployment rates as controls to the main specification, we obtain identical results.²⁵

Finally, the recent paper by Horwitz et al. (2018) highlights issues deriving from recurring differences in measuring the correct starting dates of PDMP in the literature. Although, as stated by the authors, the definition of the implementation dates is not always clear-cut, a similar point of estimate would support the reliability of our choice of dates. In Table D.3 we compare our measure of PDMP with the list provided by Horwitz et al. (2018). Columns 1–4 display our estimates using four different definitions of PDMP assembled by Horwitz et al. (2018, Table 2, pp. 31–32), namely enactment, contingent on funding, electronic and user access. The

²⁵ In Appendix C we exploit the cross-sectional variation at the beginning of the period to investigate whether the estimated results mask relevant heterogeneities based on cross-counties differences at the start of the period. We consider a number of indicators that proxy for several aspects of socio-economic conditions and of “drug environment” factors, which have been pinpointed as potential drivers of the epidemic (Case and Deaton 2015, 2017; Krueger 2017; Ruhm 2018, 2019). We observe that opioid state laws, especially the ones targeting the supply of the market for opioid prescriptions, display a larger impact in areas with better socio-economic status. Additionally, other initial drug environment factors curtail the effectiveness of supply-side laws in reducing the dispensation of prescription opioids.

last six columns refer to dates taken from publicly available databases, as selected by Horwitz et al. (2018, Table 3, pp. 33–34). The estimated coefficients are similar to our baseline.

4.3 The Effect of State Laws on Drug-Related Crime

Next, because of the predicted disruption to the legal market for opioid painkillers in combination with their high potential for addiction, we estimate whether the opioid state laws have any indirect fallout on the illegal market for drugs. While a few recent studies look at the effects of some opioid state laws on crime (Dave, Deza, and Horn 2018; Doleac and Mukherjee 2018; Mallatt 2017; Meinhofer 2017), the empirical evidence on the unintended impact of these laws on the market for illicit drugs is still scarce.

Table 4 shows the estimated effects of opioid state laws on crime related to the possession and sale of opium and derivatives and of synthetic opioids (column 1). We consider this as our main indicator for drug-related crime as it contains the two categories of drugs that are attributable to the diversion of opioids. We then investigate the effect of the laws on each category of drug-related crimes separately as grouped by UCR (2000): cocaine, opium and their derivatives such as morphine, codeine and heroin (column 2), synthetic

Table 4: Effect on drug-related crime.

Dependent variable	(1)	(2)	(3)	(4)	(5)
	Possession & sale	Possession & sale			
	Opium & synthetics	Opium	Synthetics	Marijuana	Non-narcotics
PDMP	-0.038 (0.108)	0.011 (0.102)	-0.068 (0.131)	0.022 (0.102)	0.011 (0.098)
Pain management clinic law	0.214** (0.106)	0.221* (0.111)	-0.002 (0.116)	0.066 (0.074)	0.068 (0.138)
Doctor shopping law	-0.034 (0.167)	-0.085 (0.134)	0.007 (0.185)	0.006 (0.193)	-0.185 (0.122)
Observations	50,032	50,032	50,032	50,032	50,032
R-squared	0.940	0.938	0.939	0.950	0.927

Dependent variables expressed in natural logs and in per capita terms. Population-weighted OLS estimates. All regressions include two dummy indicators capturing the enforcement of Naloxone Access Laws and Good Samaritan Laws, plus year, county and region-year fixed effects. Errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

narcotics including semi-synthetic and synthetic opioids like oxycodone, methadone and fentanyl (column 3), marijuana and hashish (column 4) and other non-narcotic drugs such as benzedrine (column 5).²⁶

PDMP do not seem to be associated with any changes in drug-related crime, while PMCL have a positive impact on the possession and the sale of drugs, for which arrest rates rise by 21%, i.e. 20 people every 100,000 inhabitants. Conversely, we do not find any statistical significance for DSL. The event-study analyses corresponding to the coefficients from column 1 are shown in Figure A.4, where point estimates indicate an increase in arrests for the possession or sale of opium and synthetic drugs, although the significance is weak. The plots also suggest that the common trend assumption holds in all cases, as the coefficients in the pre-implementation period are never statistically different from zero.

The comparison between coefficients in columns 2 and 3 of Table 4 might suggest that the effect associated to the introduction of minimum requirements to pain management clinics (PMCL) might be driven by the diversion of illicit drugs such as heroin (or cocaine), rather than synthetic drugs (including opioids). However, a more in-depth observation of the phenomenon suggests that the enactment of PCML has significantly increased crimes related to the sale of both types of drugs. This is clearly evident from the plots reported in Figure 3, which demonstrate that *Pain Management Clinics Laws* produce a large unintended increase in arrests for the sale of these drugs.²⁷ As shown in the previous subsection, PMCL constitute the set of policies under analysis that substantially curb the dispensation of prescription opioids. The differential increase in arrest rates following their enactment is coherent with prior works uncovering the existence of a substitution across different opioid drugs when the legal alternative becomes less viable (Alpert, Powell, and Pacula 2018; Evans, Lieber, and Power 2019; Mallatt 2017). Moreover, columns 4 and 5 in Table 4 show that there is no effect on arrests for possession or sale of

26 We are aware of the limitations with the UCR data, such as agency non-response or very low reporting rate as in the case of Illinois. To alleviate these concerns, we weight the crimes by the coverage indicator provided by UCR that adjusts for incomplete reporting and exclude problematic states like Illinois. Both results are similar to Table 4, if anything more precise.

27 Figure D.4 shows the full set of results by law and by type of crime. Figure D.1 (right plot) also reports the event-study for the total effect of Mandate PDMP on crime. In Appendix C we demonstrate the existence of relevant heterogeneities based on different county-level indicators that proxy for several aspects of socio-economic conditions and of “drug environment” factors, which have been pinpointed as potential drivers of the epidemic (Case and Deaton 2015, 2017; Krueger 2017; Ruhm 2018, 2019). Table D.4 shows that the increase in drug-related crime after the adoption of PMCL is driven by people aged 25–44 and 45–64.

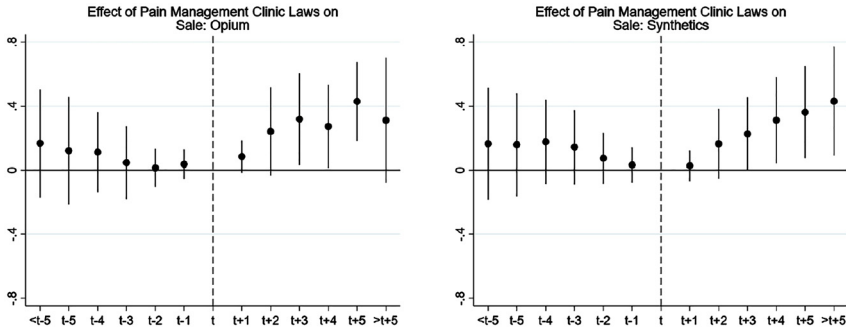


Figure 3: Event-study analysis: Effect on drug-related crime of PMCL.

Note: Coefficients estimated as in Eq. (2). The coefficient associated to $t - 5$ pertains to all periods prior to the fifth year before the implementation of the law. The coefficient associated to $t + 5$ refers to all years from the fifth after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

marijuana or other non-narcotic drugs. This is an expected result, given that the state laws under study are specifically aimed at tackling the overdispensation of opioid-based drugs.²⁸

As additional evidence, we investigate whether the introduction of opioid state laws is associated with changes in police forces. We do so to test whether arrest rates related to the dispensation of illegal substitutes of opium are affected by unobserved differences in law enforcement and policing public expenditure across counties. We proxy this by using the number of police officers per 1000 inhabitants as dependent variable and find that this is not correlated to any of the laws under consideration (column 9, Table A.3).²⁹

5 Conclusion

The United States are currently struck by an unprecedented epidemic of drug overdoses that has begun at the end of the 1990s with the rise in prescribing rates of opioid medications and is still causing tens of thousands of deaths across the country every year. In total, over 350,000 US Americans have died of opioid-

²⁸ We also do not find any impact of opioid state laws on the property (burglaries, larcenies, and motor vehicle theft) or the violent (murders, rapes, robberies and aggravated assaults) crime rates.

²⁹ Also, controlling for police enforcement in our main regressions does not change our results. We acknowledge the fact that this might not be a fully comprehensive measure, but it alleviates some concerns related to changes in policing strategies in response to the opioid crisis.

related overdose since 1999. According to the Centers for Disease Control and Prevention (2017), in 2006 doctors wrote 72.4 opioid prescriptions per 100 persons. The prescription rate has been increasing annually by 4.1% until 2008 and by 1.1% in 2008–2012 and has finally started to decrease since 2012, reaching a rate of 66.5 per 100 persons in 2016. That year, 19.1 per 100 persons received one or more opioid prescriptions, with 3.5 prescriptions per patient on average.

Our analysis suggests that the recent declining trends in the dispensation of prescription opioids might have been supported by the sets of opioid state laws implemented in recent years. These laws aim at limiting the quantity of opioids prescribed by physicians or dispensed by pharmacists, tackling the supply, *Prescription Drug Monitoring Programs* and *Pain Management Clinics Laws*, or the demand, *Doctor Shopping Laws*, for opioids. We assess the effects of these policies on per capita grams of opioids dispensed and on drug-related crime rates.

We find that state laws targeting the supply for opioids yield an overall reduction in the quantity of MGE units per capita, particularly in the case of PCMLs, which have brought to the closure of a considerable number of the so-called “pill mills”. Per contra, regulating the demand for opioids through DSL appears to be less adequate, as they do not yield significant effects on any outcome. Our results also reveal that the effectiveness of PMCL in reducing the quantity of legally dispensed opioids is somewhat counter-balanced by an increase in arrest rates for the possession and the sale of opium-based drugs.

Developing effective tools to regulate and alleviate the costs of opioid crisis and its unintended effects should be a high priority on the agenda of policy makers and researchers, not only with reference to the US context but also to other countries which have recently seen an upward trend in prescription rates and in drug-related deaths, namely the UK, Germany, France, Spain and the Netherlands (Helmerhorst et al. 2017).

Our results suggest important policy implications. First, state laws targeting the supply and the demand for legal prescription opioids do not have the same effectiveness in reducing the overall volume of drugs dispensed. Second, policies that restrict the availability of legally-dispensed prescription opioids have important indirect effects on drug-related crime rates, which are driven by the sale and the possession of opium and synthetic drugs. This unveils the existence of a close relationship between the legal and the illegal markets for drugs, which should not be neglected.

Appendix A: Tables and Figures

Table A.1: Dates of opioid state laws.

State	Supply			Demand	Mortality	
	PDMP	PDMP	Pain management	Doctor	Naloxone	Good
	Non mandatory	Mandatory	Clinic law	Shopping law	Access law	Samaritan law
AL	04/01/2006		01/01/2014		10/05/2016	08/10/2014
AK	01/08/2011				15/03/2016	80/09/2008
AZ	10/01/2008				06/08/2016	
AR	01/03/2013				22/07/2015	22/07/2015
CA	1939				01/01/2008	01/01/2013
CO	01/07/2007				10/05/2013	29/05/2012
CT	01/07/2008			1967	01/10/2003	01/10/2011
DE	01/03/2012	01/03/2012			04/08/2014	31/08/2013
DC	15/08/2016				19/03/2013	19/03/2013
FL	01/09/2011		01/07/2011	2003	22/01/2016	01/10/2012
GA	01/07/2013		01/07/2013	01/07/1990	24/04/2014	24/04/2014
HI	1943			1991	16/06/2016	07/07/2015
ID	1967				01/07/2015	
IL	1961			09/09/2015	01/01/2010	01/06/2012
IN	1998				01/07/2016	01/07/2016
IA	01/01/2009				06/04/2016	
KS	01/02/2011				07/04/2017	
KY	1999	01/07/2012	12/07/2012		25/06/2013	25/03/2013
LA	01/11/2008	01/08/2014	11/07/2005	2007	06/06/2016	01/08/2014
ME	01/07/2004			31/01/2003	29/04/2014	
MD	20/08/2013				01/10/2013	01/10/2014
MA	1994	01/06/2013			02/08/2012	02/08/2012
MI	1989				14/10/2014	04/01/2017
MN	04/01/2010				10/05/2014	01/07/2014
MS	04/05/2006		24/03/2011		15/03/2017	01/07/2016
MO	07/01/2017				28/08/2016	
MT	12/03/2012			2007	04/05/2017	04/05/2017
NE	14/04/2011				28/05/2015	02/05/2017
NV	1997	01/10/2007		1971	01/10/2015	01/10/2015
NH	02/09/2014			1990	02/06/2015	06/08/2015
NJ	01/09/2011				01/07/2013	02/05/2013

Table A.1: (continued)

State	Supply			Demand	Mortality	
	PDMP	PDMP	Pain management	Doctor	Naloxone	Good
	Non mandatory	Mandatory	Clinic law	Shopping law	Access law	Samaritan law
NM	01/01/2005	01/09/2012			03/04/2011	15/06/2007
NY	1973	01/08/2013		1973	24/06/2014	18/09/2011
NC	01/07/2007			2005	09/04/2013	09/04/2013
ND	09/01/2007				01/08/2015	21/04/2017
OH	01/06/2006	01/11/2011	20/05/2011		11/03/2014	14/09/2016
OK	1991				01/11/2013	
OR	01/06/2011				06/06/2013	03/03/2016
PA	1973				29/11/2014	01/12/2014
RI	1979				18/06/2012	18/06/2012
SC	01/02/2008			1978	05/06/2016	
SD	05/12/2011			1990	01/07/2016	27/03/2017
TN	01/12/2006	01/01/2013	01/04/2013	2007	01/07/2014	01/07/2015
TX	1982		02/05/2010	01/09/1989	01/09/2015	
UT	1996			1990	13/05/2013	20/05/2014
VT	01/01/2009	01/11/2013		23/03/1968	01/07/2013	05/06/2013
VA	09/01/2003				01/07/2013	01/07/2015
WA	07/10/2011				10/06/2010	10/06/2010
WV	01/07/1995	01/06/2012	10/03/2012	2002	10/06/2016	12/06/2016
WI	01/04/2013		17/03/2016		09/04/2014	09/04/2014
WY	01/07/2004			2008	01/07/2017	

Table A.2: Descriptive statistics.

Variable name	Mean	St. dev.	Min	Max
MGE per capita	29.7317	98.1625	0.1713	5729.6470
MGE per capita (no methadone)	24.2789	74.7191	0.1539	4004.4160
Possession and sale of opium/synthetics	0.0010	0.0028	0.0000	0.5534
Possession and sale of opium	0.0006	0.0027	0.0000	0.5533
Possession and sale of synthetics	0.0004	0.0009	0.0000	0.0519
Possession and sale of marijuana	0.0021	0.0160	0.0000	2.9826
Possession and sale of other non-narcotics	0.0008	0.0067	0.0000	1.3830
Population	97,049	312,470	55	10,200,000
Share of females	0.5008	0.0218	0.2785	0.5737
Share of people aged 65+	0.1601	0.0438	0.0168	0.5583
Share of blacks	0.0901	0.1452	0.0000	0.8626
Share of hispanics	0.0800	0.1304	0.0000	0.9729

Table A.3: Effect on drug quantities: robustness checks I.

Dependent variable	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)	
	MGEpc	All	MGEpc	All	MGEpc	All	MGEpc	All	MGEpc	All	MGEpc	All	MGEpc	All	MGEpc	All	MGEpc	Police
PDMP	-0.038*		-0.037*		-0.038*		-0.039*		-0.039*		-0.035		-0.035		-0.006		-0.019	
	(0.019)		(0.021)		(0.020)		(0.020)		(0.020)		(0.027)		(0.027)		(0.023)		(0.013)	
Pain management	-0.158***		-0.140***		-0.152***		-0.150***		-0.150***		-0.072*		-0.072*		-0.109***		-0.017	
clinic law	(0.025)		(0.029)		(0.026)		(0.026)		(0.026)		(0.038)		(0.038)		(0.025)		(0.022)	
Doctor shopping	0.021		0.035		0.037		0.038		0.038		0.056		0.057		0.064**		0.007	
law	(0.033)		(0.039)		(0.036)		(0.035)		(0.035)		(0.034)		(0.034)		(0.027)		(0.013)	
Observations	50,032		50,032		50,032		50,032		50,032		50,032		50,032		50,032		49,874	
R-squared	0.988		0.988		0.841		0.987		0.987		0.989		0.989		0.990		0.950	
Other laws	✓																	
Controls			✓															
Bacon-Goodman procedure					✓													
Initial POS exposure										Linear								
State-linear trend											Squared							
State-quadratic trend																		

The dependent variable is expressed as natural log. Population-weighted OLS estimates. All regressions include two indicators for the enforcement of Naloxone Access Laws and Good Samaritan Laws, plus year, county and region-year fixed effects. Institutional changes: indicators for the introduction of Medicare (2006) and the reformulation of OxyContin (2010). Other controls: population, share of females, share of over65, share of blacks, share of hispanics. Initial POS Exposure indicate trends based on the initial level of MGEs in the county. Police rate is the rate of officials. Errors are clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

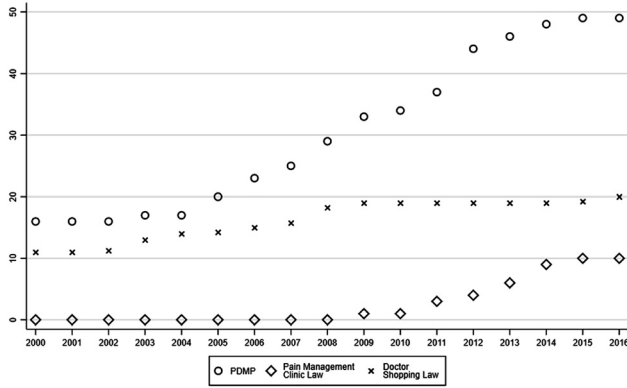


Figure A.1: Onset of opioid-related policies by year.

Note: Each marker corresponds to the total number of states in which a given policy is in effect in a given year.

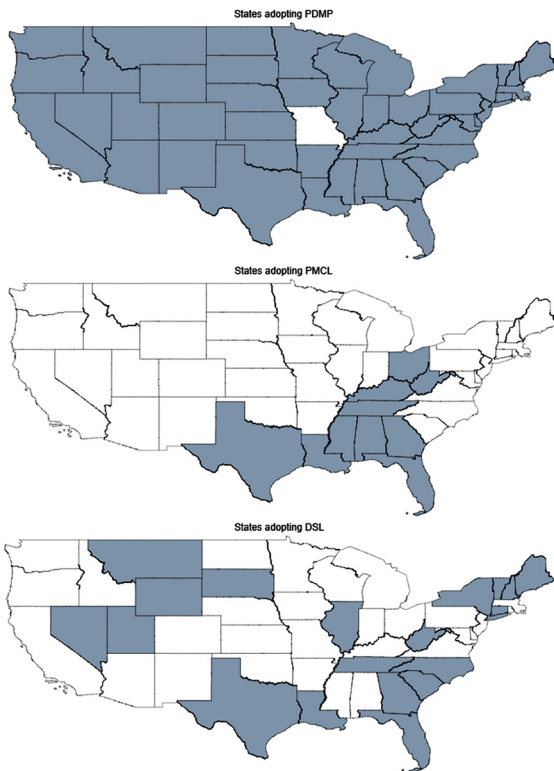


Figure A.2: State laws (2001–2016).

Note: Blue states are those where a given law is in place by the end of the period.

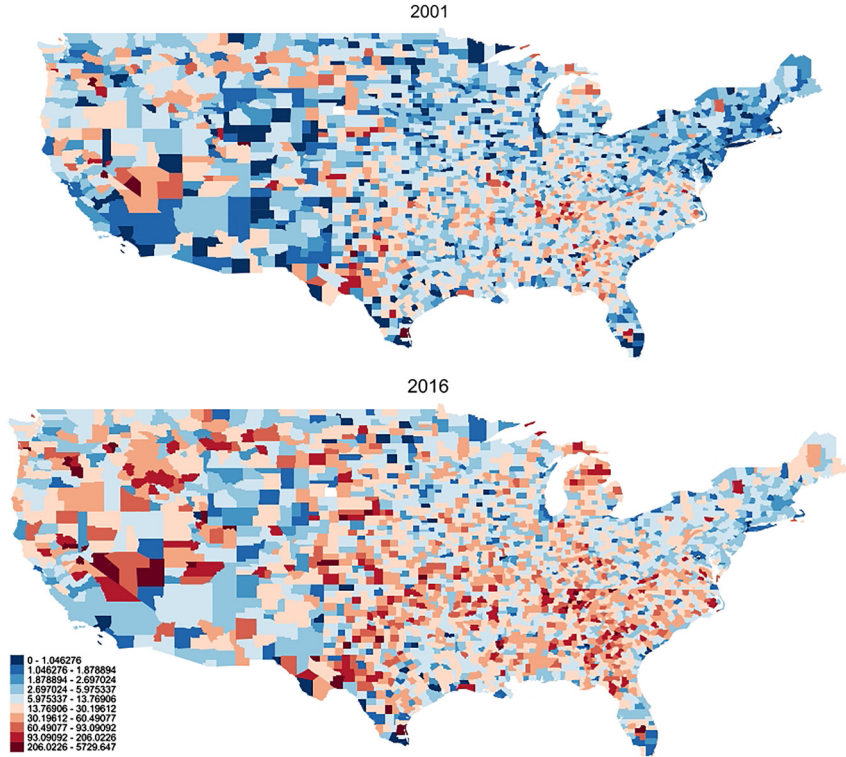


Figure A.3: Geographical distribution of MGE in 2001 and 2016.

Note: Geographical distribution of the MGE units per capita in 2001 and 2016. Thresholds are set at the 1st, 5th, 10th, 25th, 50th, 75th, 90th, 99th percentiles of the 2001–2016 average distribution.

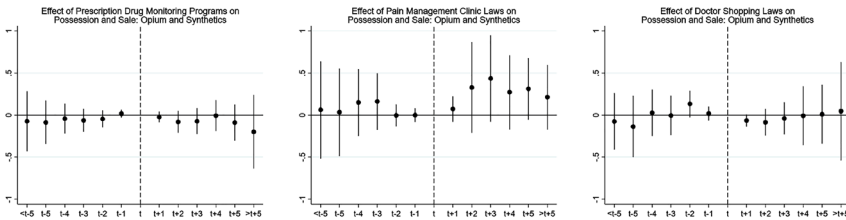


Figure A.4: Event-study analysis: effect on drug-related crime.

Note: Coefficients estimated as in Eq. (2). The coefficient associated to $t - 5$ pertains to all periods prior to the fifth year before the implementation of the law. The coefficient associated to $t + 5$ refers to all years from the fifth after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

We retrieve the dates of adoption from the inventory reports published by the Centers for Disease Control and Prevention (CDC) under the Public Health Law Program (<https://www.cdc.gov/phlp/publications/topic/prescription.html>) and Brandeis University's Prescription Drug Monitoring Program Training and Technical Assistance Center (<https://www.pdmpassist.org/content/pdmp-legislation-operational-dates>), state legislative laws and bills, government newsletters. When we cannot find information from official sources we rely on the previous literature (Mallatt 2017; NAL and GSL; PMCL; Popovici et al. 2017; Rees et al. 2019). Whenever we cannot recover the exact month of adoption, we assign July 1st as starting date.

Appendix B: State Laws Contrasting Opioid Mortality

In addition to PDMP, PMCL and DSL, several states have enacted *Naloxone Access Laws* (NAL) and *Good Samaritan Laws* (GSL). These laws have been designed and implemented with the intention to reduce the number of fatal overdoses due to the abuse of opioids by providing incentives and support to those seeking medical assistance in the case of an overdose emergency. NAL allow administering naloxone, a lifesaving medication that blocks or reverses the effects of an opioid overdose, to individuals experiencing an overdose due to opioids without incurring in any civil, criminal or disciplinary prosecution (Davis and Carr 2015). GSL grant some form of immunity or mitigation in the prosecution or at sentencing for people who call emergency medical assistance in the case of an overdose. The aim of GSL is specifically to encourage people who otherwise would not reach for help for the fear of being charged for possession of drugs. These laws have been enforced fairly recently (since 2010) in most of the states that currently have such regulations.

Their predicted effects are potentially ambiguous. On the one hand, these laws reduce the opportunity costs associated with drug abuse. In fact, they might reduce the risk of death per use, thereby making riskier opioid use more appealing, and they might save the lives of active drug users, who survive to continue abusing opioids (Doleac and Mukherjee 2018).³⁰ Hence, we might observe an increase in the quantity of opioids dispensed. On the other hand, increased access to medical assistance and counseling, both in the case of NAL and of GSL, might improve the health and psychological conditions of users and persuade them to quit drugs. In this case, we would expect a reduction in drug-related crimes. In particular, GSL are expected to

³⁰ Recent work by Freeman et al. (2018) finds that naloxone dispensing nationwide has increased dramatically since 2015.

determine a reduction in the arrest rate for drug possession because of the immunity and mitigation in court granted to the person that seeks medical assistance.

We employ two dummy indicators to control for the implementation of NAL and GSL in all our specification, given their relatedness to the analysis herein presented. In the main specification we find no statistical impact of the two laws on the amount of MGE per capita dispensed. The coefficients are reported in Table B.1, column 1. The event study analysis is presented in Figure B.1, where no tangible patterns arise. The weak impact of NAL and GSL might be a consequence of the lower opportunity cost of doing drugs that these laws generate (Doleac and Mukherjee 2018).

The exclusion of methadone results in negative coefficients (and significant in the case of NAL). These are displayed in column 2. On the one hand, medical assistance and counseling can bring about a reduction in the overall amount of opioids dispensed, but not in the quantity of methadone distributed, which is

Table B.1: Effect on drug quantities.

Dependent variables	(1)	(2)	(3)
	MGEpc	MGEpc	Possession & sale Opium & synthetics
	All	No methadone	
Naloxone access law	0.003 (0.019)	-0.027* (0.016)	0.062 (0.060)
Good Samaritan law	0.004 (0.021)	-0.018 (0.020)	-0.013 (0.116)
Observations	50,032	50,032	50,032
R-squared	0.987	0.990	0.940

See note to Table 2.

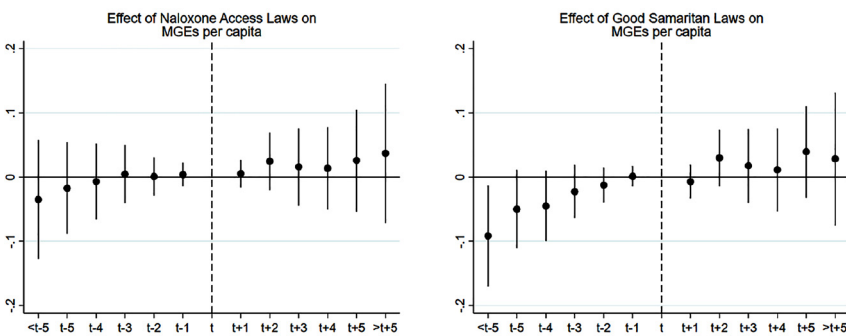


Figure B.1: Event-study analysis: effect on drug quantities.

Note: Coefficients estimated as in Eq. (2). The coefficient associated to $t - 5$ pertains to all periods prior to the fifth year before the implementation of the law. The coefficient associated to $t + 5$ refers to all years from the fifth after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

attributable to higher take-up of rehabilitation programs.³¹ On the other hand, the lower opportunity costs of drug abuse generated by these laws determine an increase in the amount of drugs that are typically consumed by heavy users (namely, methadone). This is coherent with the findings by Rees et al. (2019), who claim that the relationship between NAL and opioid-related deaths that do not involve heroin is stronger than the relationship between NAL and heroin-related deaths.³²

Finally, the two laws addressing drug-related mortality do not appear to have a significant impact on the arrest rates for the possession of opioid-based drugs.

Appendix C: Heterogeneity on Initial Local Features

Case and Deaton (2015) trace out the origin of the recent surge in drug, but also alcohol and suicide, mortality, i.e. *deaths of despair*, to the prolonged deterioration of socio-economic conditions in the US. Other studies highlight the importance of drug supply factors and of medical practices and norms, which have contributed to the rise in the amount of drugs prescribed and mortality (Harris et al. 2020; Krueger 2017; Ruhm 2019).³³

Here, we investigate whether the enforcement of opioid state laws has different effects depending on the initial levels of relevant socio-economic and drug environment indicators at the county level. The former category includes income per capita in 1990, the share of people with a degree in 1990 and the share of people with medical insurance coverage in 1998. These are all proxies for socio-economic status and living conditions, which have recently been associated with the increase in mortality due to drugs, alcohol and suicides and the abuse of prescription drugs (Case and Deaton 2015, 2017).

31 As a matter of fact, when we take drugs one by one, NAL and GSL are both associated to higher levels of methadone dispensed, while for most drugs coefficients they are negative and significant.

32 Rees et al. (2019) oppose that NAL might encourage opioid abuse due to lower opportunity costs of using drugs, as they estimate a reduction in opioid-related deaths by around 10% following the adoption of NAL.

33 See also Joyce and Xu (2019), who discuss the case of the UK. Recent contributions analyse how the demand for drug prescription varies with health insurance coverage (Clayton 2019) and Medicaid expansion (Ghosh, Simon, and Sommers 2019). Hollingsworth, Ruhm, and Simon (2017) argue that mortality and emergency department visits attributable to opioid abuse rise when economic conditions worsen. Similarly, Carpenter, McClellan, and Rees (2017) provides evidence that economic downturns are associated with an increase in drug and alcohol abuse disorders, while Krueger (2017) shows that labour participation is lower (and declines at a faster rate) in counties where more opioids are prescribed. Finally, Charles, Hurst, and Schwartz (2018) finds that the decline in the manufacturing sector partly explains the increase in the abuse of opioids during the same period. See also Stiglitz (2015), Meara and Skinner (2015), and Pierce and Schott (2020).

The second group of indicators encompasses the share of workers in the health sector, the number of opioids dispensed in 2000 and mortality rates due to alcohol and drug abuse disorders in 1990. While, in a broad sense, the share of workers in the health sector might identify access to health services (similarly to health insurance coverage), it is intended here as a proxy for the supply of health services. The assumption is that a larger relative ratio of physicians, pharmacists, and health care professionals per inhabitant is likely to translate into higher availability of suppliers. As a matter of fact, highly exposed counties consume almost three more grams per capita than less exposed areas. This variable and the quantity of MGE per capita dispensed in 2000 pick up cross-county heterogeneities in prescribing practices.³⁴ Mortality rates for drugs and alcohol refer to 1990, a period that is antecedent to the outbreak of prescription opioids that was characterised by the abuse of other substances such as crack cocaine (Fryer et al. 2013). As such, they are meant to capture structural differences in risky behavior across local communities.

For each proxy, we consider the levels at the beginning of the period, where baseline years vary depending on data availability, in order to limit potential issues of reverse causality. We exploit the within-state distribution of each indicator to determine whether a county is subject to high, medium or low “exposure”. That is, each area is ranked with respect to the counties within the same state, to ensure that the exposure does not simply capture geographical differences. Then, we interact each law with three dummy variables that take value 1 if the county’s initial level of exposure is below the 33rd percentile (low), between the 33rd and the 66th percentiles (medium) and above the 66th percentile (high) of its state distribution and 0 otherwise. This specification allows understanding whether the enactment of the opioid state laws yields differential effects on the outcomes of interest depending on the initial conditions of a given county relative to other areas within the same state.

First, we run this exercise on the amount of MGE per capita dispensed. Table C.1 reports the estimated coefficients for each of the specified exposures. Column 1, 2 and 3 refer to income per capita, the share of graduates and the proportion of people with health insurance coverage at the beginning of the period, respectively. As discussed above, they all serve as proxies for the social and economic composition of the population in the county and, as expected, they yield similar results. We find that in

³⁴ The share of workers in the health sector is computed as the number of workers employed in the NAICS 62 in 1998 divided by the total population in 2000. Krueger (2017) assumes that the differences in prescribing rates across areas are exogenously determined by medical practices and norms. Harris et al. (2020) use the number of high-volume prescribers as an instrument for prescription rates, given the extensive anecdotal and empirical evidence documenting that a large fraction of opioid prescriptions can be attributed to heterogeneity in providers.

Table C.1: Effect on drug quantities: heterogeneity.

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PDMP * low exposure	0.031 (0.027)	0.027 (0.025)	0.023 (0.025)	-0.053** (0.026)	-0.062*** (0.022)	-0.067** (0.033)	-0.077*** (0.026)
PDMP * medium exposure	-0.030 (0.025)	-0.033 (0.026)	-0.006 (0.026)	-0.041* (0.022)	-0.009 (0.021)	-0.042** (0.020)	-0.035* (0.020)
PDMP * high exposure	-0.053** (0.021)	-0.052** (0.020)	-0.053** (0.021)	-0.007 (0.029)	0.016 (0.025)	-0.025 (0.025)	-0.015 (0.028)
PMCL * low exposure	-0.127*** (0.028)	-0.098** (0.037)	-0.090** (0.042)	-0.183*** (0.050)	-0.158*** (0.024)	-0.155*** (0.057)	-0.151*** (0.050)
PMCL * medium exposure	-0.134*** (0.032)	-0.137*** (0.027)	-0.110*** (0.035)	-0.136*** (0.031)	-0.139*** (0.033)	-0.129*** (0.026)	-0.109*** (0.029)
PMCL * high exposure	-0.164*** (0.031)	-0.163*** (0.029)	-0.166*** (0.028)	-0.159*** (0.037)	-0.099*** (0.035)	-0.174*** (0.025)	-0.196*** (0.030)
DSL * low exposure	0.081** (0.035)	0.098* (0.051)	0.015 (0.058)	0.047 (0.038)	0.031 (0.035)	0.016 (0.054)	0.066 (0.044)
DSL * medium exposure	0.058 (0.041)	0.067* (0.038)	0.059 (0.038)	0.039 (0.039)	0.049 (0.040)	0.045 (0.035)	0.032 (0.041)
DSL * high exposure	0.019 (0.036)	0.012 (0.036)	0.029 (0.037)	0.018 (0.041)	0.029 (0.044)	0.032 (0.038)	0.020 (0.035)
Observation	50,032	50,032	50,032	50,032	50,032	50,032	50,032
R-squared	0.987	0.987	0.987	0.987	0.987	0.987	0.987
Heterogeneity	Income	Education	Health Insurance	Health Sector	MGepc	Drug Mortality	Alcohol Mortality

Dependent variables are expressed in natural logs and in per capita terms. Population-weighted OLS estimates. All regressions include year, county and region-year fixed effects. Errors are clustered at the state level. The interaction terms are defined on the basis of three dummy variables that take value 1 if the initial county level is below the 25th percentile (Low), between the 25th and the 75th percentile (Medium) and above the 75th percentile (High) of the state distribution and 0 otherwise, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

wealthier and highly-educated counties the enactment of supply-side laws has an overall negative impact on the quantity of MGE units per capita dispensed. Specifically, PDMP bring about a decrease in the outcome in highly exposed areas only, while the enforcement of PMCL always yields a drop in prescription rates at all levels of exposure, though the magnitude of the coefficients is larger in relatively better-off areas. Conversely, DSL have a mildly positive impact on the outcome in poorer and less educated counties, suggesting that their enforcement is possibly detrimental among communities that are relatively more deprived at the beginning of the period.³⁵

In columns 4 and 5 we consider the share of workers in the health sector as a measure of the supply of health services and the amount of MGEs per capita dispensed in 2000, respectively, while columns 6 and 7 refer to mortality rates due to drugs and alcohol in 1990. All these measures are meant to capture different dimensions of what Ruhm (2019) refers to as “drug environment”. Our estimates suggest that supply-side laws are indeed more effective in areas that are relatively less familiar with substance and alcohol abuse and where drug suppliers are less densely localized. As far as DSL, coefficients do not display a clear pattern and are hardly statistically different from zero.

Results on arrest rates for possession or sale of opium-based and synthetic drugs are reported in Table C.2. We find that the enforcement of supply- and demand-side laws yield higher levels of crime in relatively more deprived areas compared to wealthier counties (columns 1–3). This result, in combination with the one discussed above, suggests that when opioid state laws are introduced, although they bite less in poorer areas, they induce a positive shock to the illicit market for drugs. Conversely, the response in counties that are better-off in relative terms possibly translates into lower drug-related crime rates.

When it comes to differences in exposure to the drug environment, coefficients do not differ substantially across groups (columns 4–7). On the one hand, state laws are more effective in reducing the amount of legally-dispensed opioids in less exposed areas; here, more people might turn to the black market to compensate for the absence of medical prescription drugs, thus increasing crime rates. This suggests the existence of a substitution effect across the legal and illegal markets for drugs. On the other hand, counties with a relatively higher initial supply of drugs and mortality rates, already characterized by high crime rates, do not display

35 Our initial level of health insurance coverage refers to the year 2000, that is prior to Obamacare. Thus, this is likely to be highly correlated with income. Moreover, given that over the years private and public insurers have increased their share of payments compared to out-of-pocket expenditure by consumers (Zhou, Florence, and Dowell 2016), patients with health insurance coverage should have less binding financial constraints when buying prescription drugs. Then, we would expect higher rates of health insurance coverage to be correlated with larger quantities of prescription opioids consumed per capita. As a matter of fact, our results imply that state laws are more effective in reducing abusive behaviour when individuals have better access to medical insurance.

Table C.2: Effect on crime: heterogeneity.

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Possession & sale: Opium & synthetics			
PDMP * low exposure	0.106 (0.131)	0.061 (0.168)	0.212 (0.141)	0.052 (0.115)	-0.110 (0.099)	0.058 (0.130)	0.028 (0.115)
PDMP * medium exposure	-0.006 (0.130)	0.023 (0.123)	0.088 (0.134)	-0.044 (0.106)	0.047 (0.127)	-0.017 (0.125)	0.004 (0.120)
PDMP * high exposure	-0.079 (0.101)	-0.089 (0.101)	-0.096 (0.100)	-0.169 (0.180)	0.150 (0.149)	-0.094 (0.103)	-0.151 (0.118)
PMCL * low exposure	0.216* (0.119)	0.430*** (0.118)	0.286*** (0.096)	0.173 (0.121)	0.202* (0.117)	0.293** (0.127)	0.245** (0.098)
PMCL * medium exposure	0.264** (0.115)	0.209* (0.104)	0.304*** (0.107)	0.249** (0.114)	0.259** (0.105)	0.289** (0.123)	0.244** (0.120)
PMCL * high exposure	0.189* (0.108)	0.197* (0.113)	0.185 (0.118)	0.187 (0.125)	0.265*** (0.096)	0.121 (0.115)	0.189 (0.116)
DSL * low exposure	0.177 (0.221)	0.101 (0.225)	0.102 (0.184)	-0.177 (0.151)	-0.038 (0.167)	-0.150 (0.190)	-0.037 (0.178)
DSL * medium exposure	-0.050 (0.166)	-0.032 (0.163)	-0.009 (0.170)	-0.028 (0.167)	-0.050 (0.175)	-0.079 (0.173)	-0.103 (0.177)
DSL * high exposure	-0.051 (0.170)	-0.052 (0.174)	-0.052 (0.171)	0.108 (0.205)	0.095 (0.209)	0.054 (0.155)	0.060 (0.163)
Observations	50,032	50,032	50,032	50,032	50,032	50,032	50,032
R-squared	0.940	0.940	0.940	0.940	0.940	0.940	0.940
Heterogeneity	Income	Education	Health	Health	MGEpc	Drug	Alcohol
			Insurance	Sector		Mortality	Mortality

Dependent variables are expressed in natural logs and in per capita terms. Population-weighted OLS estimates. All regressions include year, county and region-year fixed effects. Errors are clustered at the state level. The interaction terms are defined on the basis of three dummy variables that take value 1 if the initial county level is below the 25th percentile (Low), between the 25th and the 75th percentile (Medium) and above the 75th percentile (High) of the state distribution and 0 otherwise, respectively.

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

significant increases in the outcome variable.³⁶ Possibly, such unfavorable conditions make it harder for opioid regulations to have a significant role in reducing drug-related crime rates. This is especially evident in the case of PMCL.

Appendix D: Additional Tables and Figures

Table D.1: Effect on drug quantities: quarterly data.

Dependent variable	(1) MGEpc	(2) MGEpc
PDMP	-0.036* (0.018)	-0.039** (0.019)
Mandate PDMP		-0.035 (0.022)
Pain management clinic law	-0.164*** (0.026)	-0.153*** (0.026)
Doctor shopping law	0.011 (0.041)	0.012 (0.041)
Observations	200,376	200,376
R-squared	0.987	0.987

The dependent variable is the natural log of MGE per capita. Population-weighted OLS estimates, where the weight is computed as the share of the population in the county relative to the national population. All regressions include two dummy indicators capturing the enforcement of Naloxone Access Laws and Good Samaritan Laws, plus quarter, county and region-year fixed effects. Errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.2: Effect on drug quantities: robustness checks II.

Dependent variable	(1) MGEpc	(2) MGEpc	(3) Employment rate	(4) Unemployment rate
PDMP	-0.038* (0.020)	-0.038* (0.020)	-0.005 (0.004)	-0.017 (0.028)
Pain management clinic law	-0.152*** (0.026)	-0.153*** (0.026)	0.006 (0.007)	-0.032 (0.039)
Doctor shopping law	0.037 (0.036)	0.036 (0.036)	0.004 (0.010)	-0.059* (0.034)
Employment rate	0.023 (0.188)	0.012 (0.174)		
Unemployment rate		-0.016 (0.050)		
Observations	50,032	50,032	50,032	50,032
R-squared	0.987	0.987	0.920	0.899

The dependent variable is expressed in terms of natural log. Population-weighted OLS estimates. All regressions include two dummy indicators capturing the enforcement of Naloxone Access Laws and Good Samaritan Laws, plus year, county and region-year fixed effects. Errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

36 Arrest rates are 30% higher in counties with high drug and alcohol mortality rates at the beginning of the period compared to those with low mortality rates.

Table D.3: Effect on drug quantities using Horwitz et al. (2018)

	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	(9)	(10)
		Enactment	Contingent on funding		Electronic	User access		Enabling legislation	Operational				
Dep. var.:	-0.037*	-0.037*	-0.037*	-0.034*	-0.025	-0.022	-0.045**	-0.039*	-0.016	-0.039*	-0.039*	-0.007	
MGEpc	(0.020)	(0.019)	(0.019)	(0.019)	(0.024)	(0.021)	(0.021)	(0.023)	(0.020)	(0.021)	(0.021)	(0.022)	
Observations	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	
R-squared	0.987	0.987	0.987	0.987	0.987	0.987	0.987	0.987	0.987	0.987	0.987	0.987	

We replicate column 5 of Table 2 using the dates for PDMP provided by (Horwitz et al. 2018). Errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: Effect on drug-related crime: by age.

Dependent variable	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		
	Age 16–24: Poss/Sale		Age 25–44: Poss/Sale		Age 45–64: Poss/Sale		Age 65+: Poss/Sale		Opium		Synthetics		Opium		Synthetics		
PDMP	0.006 (0.083)	-0.077 (0.114)	0.017 (0.095)	0.017 (0.129)	0.014 (0.078)	0.014 (0.088)	0.014 (0.078)	0.014 (0.088)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)	0.014 (0.078)
Pain management clinic law	0.148 (0.132)	-0.011 (0.090)	0.216** (0.100)	0.051 (0.124)	0.151** (0.069)	0.051 (0.086)	0.151** (0.069)	0.051 (0.086)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)	0.151** (0.069)
Doctor shopping law	-0.086 (0.134)	0.000 (0.143)	-0.098 (0.118)	0.009 (0.177)	-0.139 (0.099)	0.000 (0.142)	-0.139 (0.099)	0.000 (0.142)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)	-0.139 (0.099)
Observations	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032	50,032
R-squared	0.930	0.931	0.933	0.931	0.923	0.926	0.923	0.926	0.923	0.923	0.926	0.926	0.926	0.926	0.926	0.926	0.926

Dependent variables are the natural log of crime rates. Population-weighted OLS estimates. All regressions include year, county and region-year fixed effects. Errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

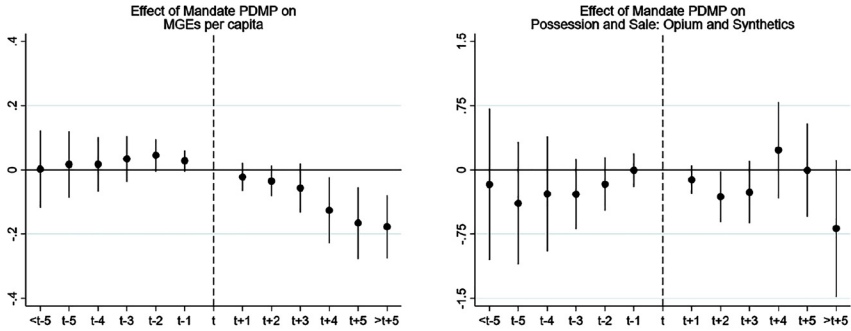


Figure D.1: Event-study analysis: effect of mandate PDMP.

Note: Coefficients are the sum of PDMP and mandate PDMP (as in column 6, Table 2). The coefficient associated to $t - 5$ pertains to all periods prior to the fifth year before the implementation of the law. The coefficient associated to $t + 5$ refers to all years from the fifth after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

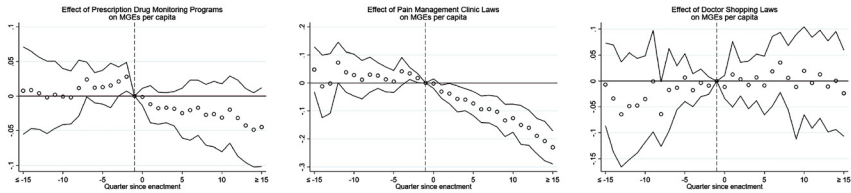


Figure D.2: Event-study analysis: effect on drug quantities, quarterly data.

Note: Based on quarterly data. Coefficients estimated as in Eq. (2), where year fixed effects are replaced with quarter fixed effects. The coefficient associated to $t - 15$ pertains to all periods prior to the 15th quarter before the implementation of the law. The coefficient associated to $t + 15$ refers to all quarters from the 15th after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

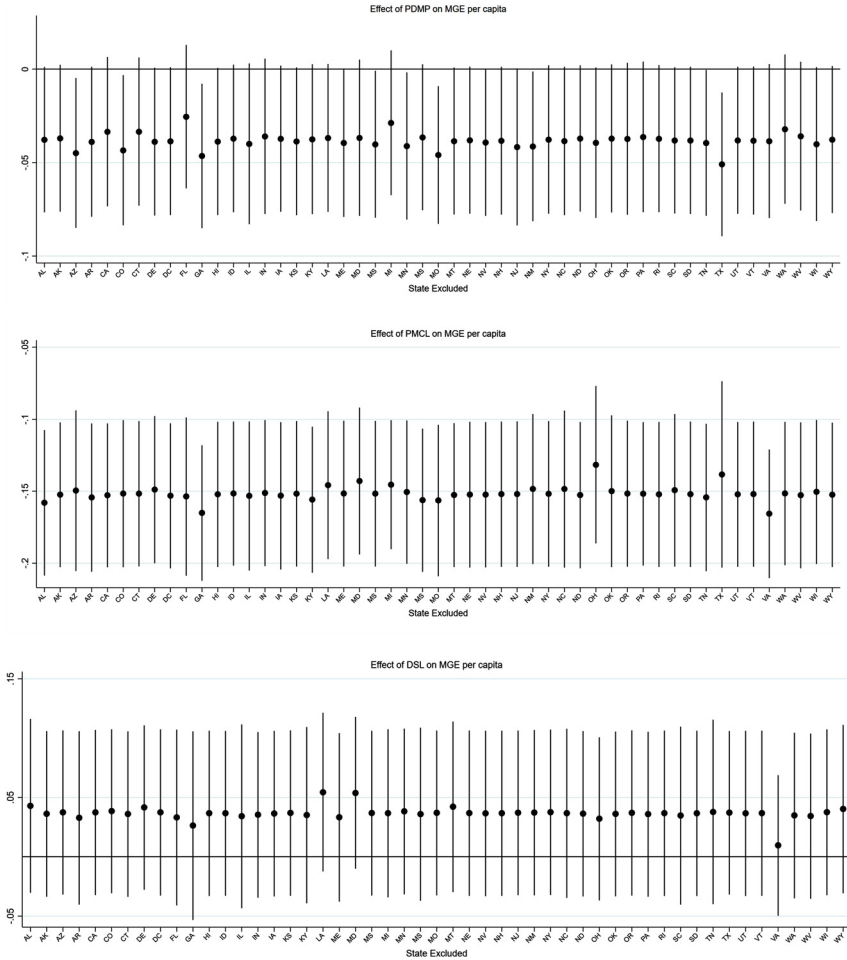


Figure D.3: Effect on drug quantities: excluding states one-by-one note: Coefficients estimated as in Eq. (1). Standard errors clustered at state level, 95% confidence intervals are shown.

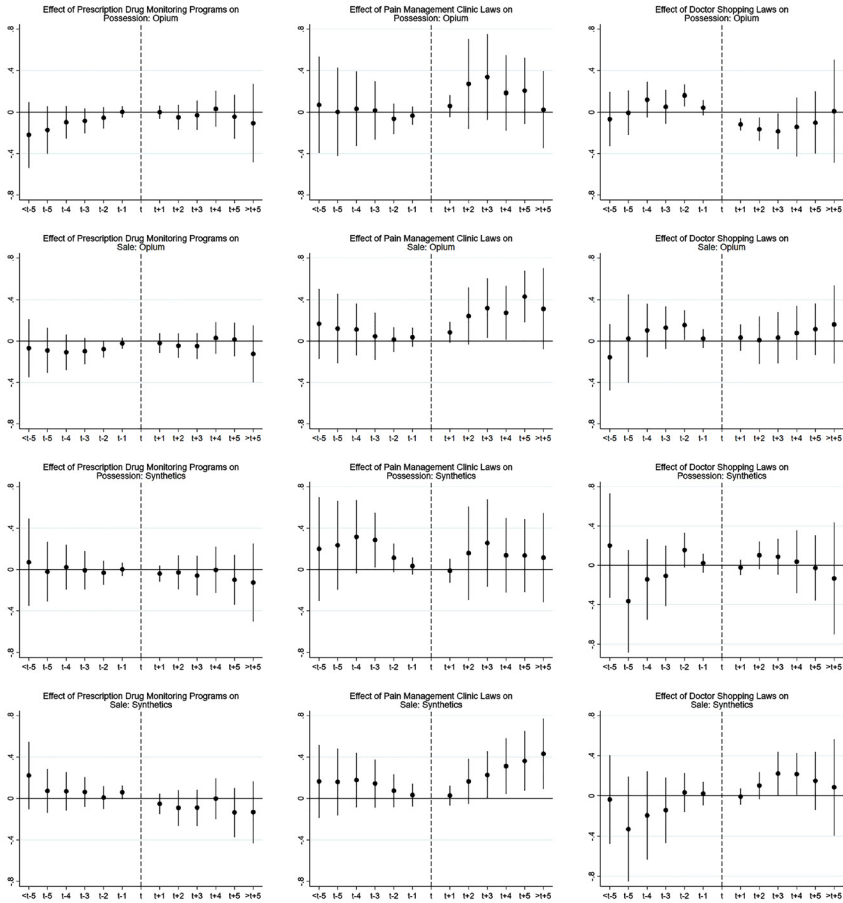


Figure D.4: Event-study analysis: Effect on drug-related crime by type.
 Note: Coefficients estimated as in Eq. (2). The coefficient associated to $t - 5$ pertains to all periods prior to the fifth year before the implementation of the law. The coefficient associated to $t + 5$ refers to all years from the fifth after the implementation of the law. Standard errors are clustered at the state level and 95% confidence intervals are shown.

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