The Effects of OxyContin Reformulation on Homicides[‡‡]

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Abstract

This study examines whether a supply-side intervention on prescription drugs, the introduction of an abuse-deterrent formulation of OxyContin, increased homicide victims. If so, could this effect be mitigated? First, leveraging cross-state variation in pre-reformulation OxyContin exposure, difference-in-difference estimates show that OxyContin reformulation led to an increase in homicide victims; this effect is strongest among those between 15 and 24, plausibly resulted from an increase in illicit market activities associated with the rise in post-reformulation demand for illicit opioids. The study then explores the potential of medical marijuana laws in mitigating these adverse effects, considering the analgesic properties of marijuana. Using pre-reformulation OxyContin exposure as an indicator to identify the specific demographic impacted and matches states with comparable exposure levels, difference-in-difference estimates show that medical marijuana legalization consistently led to a post-reformulation decrease in heroin use, heroin overdose deaths, and homicide victims in states where the exposure is the highest.

Keywords: OxyContin Reformulation, Homicide, Medical Marijuana Laws, Prescription Opioids, Illicit Drug Markets

JEL Codes: I12, I18, K32, K42

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1 Introduction

Between 1999 and 2021, the opioid crisis has claimed the lives of nearly 650,000 Americans, with little sign of slowing down. The sources of opioids involved in these overdoses, however, are pretty distinct by period. In the early phase (first wave) of the crisis, they often involved prescription opioids, whereas, after 2010 (second and third wave), illicit opioids (for example, heroin and fentanyl) have become increasingly the underlying causes of opioid overdose deaths (CDC, 2023). This observation highlights the critical role of illicit drug markets in understanding the impact of the opioid crisis. In fact, Maclean et al. (2022) attributes the relatively higher crime rates in more recent waves of the opioid crisis to more significant interaction with black markets by consumers of illicit drugs.

One prescription opioid, OxyContin, is particularly relevant in such interaction. Introduced in 1996 by Purdue Pharma, it is the second most prescribed pain management medication in the United States (CDC, 2023) thanks to its successful marketing campaign (Van Zee, 2009), but it is also one of the widely abused prescription opioids (Cicero et al., 2005) as its active ingredient, OxyContin, is highly addictive. In 2010, as part of the effort to deter widespread nonmedical use of OxyContin, the FDA approved a reformulated version of the drug that significantly reduced its potential for abuse. Consequently, dependent users substituted other alternatives, with heroin being the most common choice (Cicero and Ellis, 2015), igniting the heroin epidemic (Evans et al., 2019). This increase in demand for illicit alternatives will likely increase the level of illegal drug market activities, such as drug distribution involving gangs, which are often associated with homicide (Goldstein et al., 1997; Werb et al., 2011), yet there is limited causal evidence (Park, 2021) documenting this effect. Moreover, how this effect can be mitigated remains unanswered. This paper uses cross-state variation in pre-reformulation OxyContin nonmedical use rates obtained from survey data, difference-in-difference regression, and death certificate-based mortality data to answer whether the reformulation of OxyContin increased homicide victims and whether this effect is heterogeneous across victim age groups. Furthermore, this paper answers how the increase in homicide caused by this policy could be mitigated by medical marijuana legalization using a simple matching-based identification strategy.

To test whether OxyContin reformulation increased homicide rates, this paper follows the identification strategy of Alpert et al. (2018), which explores the cross-state variation in pre-reformulation exposure to OxyContin, measured as nonmedical use rates to predict the differential effect of the policy on homicide rates between states with high exposure and with low exposure. Furthermore, to test whether the impact of the OxyContin reformulation on homicide is heterogeneous across age groups, this paper follows Owens (2014). It extends the baseline fully parameterized specification into a triple-difference identification strategy where the additional difference in homicide rates is between a chosen age group and the rest of the age distribution. For the primary outcome and variable of interest, it uses National Vital Statistics System (NVSS) data to measure homicide victims per 100,000 and National Drug Use and Health (NSDUH) data for OxyContin and other pain reliever nonmedical use measures. The study period is between 2000 and 2017 (inclusive).

Both the event study and the fully parameterized regression results show that the OxyContin reformulation significantly increased homicide rates and that such increase is most prominent in states with the highest pre-reformulation exposure to OxyContin. The baseline fully parameterized regression estimates show that the differential increase is 0.945 homicide victims per 100,000 individuals, which is equivalent to roughly 16% of the average pre-reformulation homicide rate. Furthermore, this paper finds that this increase concentrates on younger victims. The estimation result shows an 8.152 homicide victim per 100,000 individuals difference between victims aged 15-24 and other age groups. Given that (1) homicide victims due to gang activities are generally younger than those due to the influence of drugs[1] and (2) the likelihood of becoming homicide victims is many times higher for youth gangs[2] members than that of the general population (Morales, 1992; Decker Scott and Van, 1996; Levitt and Venkatesh, 2000), this finding provides suggestive evidence that the increase in post-reformulation homicide victims is primarily due to illicit drug market activities.

These findings contribute to the growing literature on the unintended consequences of supply-side drug control policies, such as law enforcement outcomes (Mallatt, 2022; Doleac and Mukherjee, 2022; Deiana and Giua, 2021), labor market outcomes (Park and Powell, 2021), traffic accidents (Betz and Jones, 2022), hepatitis (Powell et al., 2019; Beheshti, 2019), and child maltreatment (Evans et al., 2022), by providing strengthening evidence that the reformulation of OxyContin led to increasing in homicide victims. In particular, they complement the findings by Park (2021). The analysis in this paper differs mainly by (i) the implication from the event study specification findings, (ii) the implementation of the difference-in-difference strategy, and (iii) the test for mechanisms. For (i), the results of this paper show that the estimated effect is transitory. For (ii), this paper complements her findings with results from the fully parameterized specification used in Alpert et al. (2018). For (iii), this paper follows Owens (2014) to test "market-based" homicides, that is, homicide victims resulting from illicit market activities.

Suppose substitution towards heroin due to the reformulation of OxyContin could be reduced by an intervention. In that case, it might lead to a reduction in homicide victims due to decreased demand for illicit drugs. Other adverse outcomes, such as heroin overdoses, might also be reduced due to this intervention. Though the number of studies documenting the adverse effects of OxyContin reformulation or similar supply-side disruptions is growing, empirical evidence on how such effects could be mitigated has been limited. To my knowledge, this study is one of the early attempts in this direction[3].

This paper posits that, since medical marijuana can effectively replace prescription opioids for pain management for specific conditions due to its analgesic properties (Reiman et al., 2017; Caldera, 2020; Carlini, 2018; Bicket et al., 2023), it could reduce the post-reformulation demand

[1] See figure 10.

[2] Youth gang is another name for street gangs, typically consists of members who are in their adolescence and early 20s.

[3] Evans et al. (2022) find that the adverse effects of the OxyContin Reformulation on Child Maltreatment Outcomes concentrated in counties without legal access to medical marijuana.

for illicit opioids such as heroin, thus diminishing violence associated with the illegal drug market. This is based on the economic principle that a rational individual in need of pain management will choose the option with the lowest opportunity cost, considering both the drug's properties and the legal ramifications of its use. Legalizing medical marijuana could lower its opportunity cost as opposed to illicit opioids, incentivizing more patients to opt for it over illegal substances. However, both existing medical (Hsu and Kovács, 2021; Shover et al., 2019; Bachhuber et al., 2014; Chihuri and Li, 2019) and economics (Mathur and Ruhm, 2023; Powell et al., 2018; Chan et al., 2020) literature lack consensus regarding the impact of medical marijuana laws on opioid overdose deaths. As summarized in Mathur and Ruhm (2023), this effect is fragile to the choice of studying periods: extending study periods (i.e., adding more recent data) tends to show larger, usually positive, estimated effects of these laws on opioid death outcomes. This inconsistency suggests that, without a clear understanding of how medical marijuana could reduce opioid overdoses, the economic argument that medical marijuana laws could reduce post-reformulation homicide victims is questionable.

This paper explains that this inconsistency is likely due to the evolving stringency of prescription opioid policies over time, which led to reductions of patients expected to be affected by legal access to medical marijuana. It uses a simple probabilistic framework of a healthcare system for pain treatment, featuring (1) random matching between physicians and patients (Eichmeyer and Zhang, 2022), (2) physicians who over-prescribe pain management medications (Schnell and Currie, 2018), and (3) system-generated substance use disorders by both over-treatment and under-treatment of pain, to illustrate this. First, it shows that positive shocks to over-prescription lead to an increase in the prevalence of substance use disorders. Next, it shows graphically that if marijuana cannot manage pain beyond a certain level, then depending on the relative position of this threshold and the pain distribution, access to medical marijuana may or may not be effective in reducing the prevalence of substance use disorder. This analysis suggests that this inconsistency might be an example of attrition bias caused by the relative stringency of prescription opioid policies between the early and recent waves of the opioid crisis. Using a Rubin causal inference framework, this paper further shows that such attrition biases the difference-in-difference estimate upward.

This paper highlights the difficulty in testing whether medical marijuana legalization reduced the post-reformulation homicide victims: characterizing or finding proxies for the affected population. It addresses this identification problem by matching states with similar pre-reformulation OxyContin misuse rates. The identifying assumption is that the cross-state variation in OxyContin nonmedical use before its reformulation can be primarily explained by variations in physicians' pain medication prescriptions targeting patients with non-cancer type pain management needs. Comparing states with similar pre-reformulation OxyContin misuse rates is therefore equivalent to controlling for the marginal population affected by legal access to medical marijuana post-reformulation. To complement outcomes along the causal pathway of the homicide-reduction hypothesis, it uses the National Survey of Drug Use and Health (NSDUH) to measure marijuana and heroin use and

the National Vital Statistics System (NVSS) to measure heroin overdose victims[4].

By grouping states into quantiles of pre-reformulation misuse rates, difference-in-difference estimates show that medical marijuana legalization led to decreases in post-reformulation (i) heroin use, (ii) heroin overdose deaths, and (iii) homicide rates *only* in states where pre-reformulation OxyContin misuse are the highest. Estimates from the preferred specifications show that medical marijuana legalization led to a 0.616 percentage points reduction in heroin use, 0.631 reduction in heroin overdose deaths per 100,000 individuals, and 0.961 reduction in homicide victims per 100,000 individuals, which are statistically significant at the 99% confidence level.

These findings have several limitations. First, although this paper provides a plausible explanation to help resolve the lack of consensus in both the medical and the economics literature on how medical marijuana laws affect opioid overdose deaths, it does not resolve this inconsistency [5]. Second, the identification strategy that this paper uses to test the homicide-reduction hypothesis cannot *isolate* the effect through the channel in which legal access to medical marijuana reduces illicit opioid use. Instead, it indicates *where* this effect is likely to occur. Lastly, this paper only concerns how medical marijuana laws could help mitigate increases in post-reformulation homicide victims. The test for medical marijuana dispensing laws is left for future research.

These findings contribute to a growing empirical literature in economics on supply-side policies aiming at curbing the opioid crisis, including OxyContin reformulation (Alpert et al., 2018; Evans et al., 2019; Powell and Pacula, 2021), triplicate prescription programs (Alpert et al., 2022), pain management clinic laws (the "pill mill" laws) (Mathur, 2021; Chisom, 2020; Kaestner and Ziedan, 2023; Mallatt, 2022), must-access prescription drug monitoring programs (PDMP) (Evans et al., 2022; Wang, 2021; Buchmueller and Carey, 2018), and Good Samaritan Laws (Rees et al., 2019). This contribution is two-fold. First, it provides an empirical test to the theory of the relationship between OxyContin reformulation and homicide. Moreover, it gives a concrete example where the adverse, unintended consequences caused by one supply-side drug policy could be mitigated by another *existing* supply-side drug policy intended for a different drug. This suggests that indirect interventions could be effective in similar future events, provided that the population affected by such interventions is identified. This paper can also relate to the emerging literature in economics on physician prescribing behaviors regarding prescription opioids and the opioid crisis (Eichmeyer and Zhang, 2022; Janssen and Zhang, 2023; Ellyson et al., 2022; Grecu and Spector, 2019) as it demonstrates that physician prescribing behavior is central in identifying the marginal population affected by legal access to medical marijuana.

The rest of this paper is organized as follows. The next section briefly summarizes the background information on the reformulation of OxyContin. Section 3 provides empirical evidence of whether OxyContin reformulation led to an increase in homicide rates and in which age group this

[4]The current version of of this paper considers heroin overdose victims involving only heroin. This likely underestimates the effect of legal access to medical marijuana on total heroin overdose deaths.

[5]To *resolve* it, one needs to come up with credible identification strategies accounting for the evolution of the marginal population expected to be affected by medical marijuana laws. Note that the identification strategy that this paper uses cannot account for such evolution in the time dimension.

effect is concentrated. Section 4 presents a probabilistic argument to respond to Mathur and Ruhm (2023). It then tests whether medical marijuana laws reduced post-reformulation heroin use, heroin overdose deaths, and homicides. Section 5 concludes the paper.

2 Background

OxyContin, introduced in 1996 by Purdue Pharma, is the brand name of the extended-release formulation of Oxycodone Hydrochloride. Its active ingredient, Oxycodone, is a semisynthetic opioid typically used for the management of pain (Drug Enforcement Administration, Diversion Control Division, Drug & Chemical Evaluation Section, 2023). The long-acting formula of OxyContin claimed to have approximately 12 hours of continuous pain relief, a substantial improvement over previous pain medications [6]. However, the abuse potential of OxyContin became evident as patients discovered that crushing or dissolving the pill released the entire Oxycodone dose instantly instead of continuously [7]. Moreover, Purdue Pharma conducted a highly effective marketing campaign that misrepresented OxyContin's potential for abuse to doctors and patients[8], resulting in significant market penetration but also widespread nonmedical use (Van Zee, 2009). By 2010, OxyContin's sales exceeded $3 billion, ranking it among the top-selling prescription drugs as well as one of the most commonly prescribed opioid painkillers in the United States (Bartholow, 2011). OxyContin's abuse potential and pervasive use have led experts to link OxyContin to the escalation of the opioid epidemic, with studies indicating its introduction as a critical factor in the rise of overdose deaths since 1996 (Kolodny et al., 2015; Alpert et al., 2022).

In April 2010, in response to the growing concerns over abuse, Purdue Pharma released a reformulated version of OxyContin and subsequently stopped the distribution of the initially formulated version in August of the same year. The reformulated version maintained the therapeutic benefits of the drug for legitimate pain management needs. However, compounded with the high-molecular-weight polyethylene oxide, the pills turn marshmallows when crushed with force and gooey when dissolved (Kibaly et al., 2021), making them difficult to abuse by traditional means, such as snorting or injecting. The reformulation of OxyContin significantly increased the effort needed to abuse the drug (Cicero and Ellis, 2015), successfully reducing the nonmedical use of OxyContin (Cicero et al., 2012). Consequently, it drove users toward alternative illicit opioids, notably heroin (Cicero et al., 2013).

This shift, first and foremost, can be attributed to the pharmacological parallels between oxycodone and heroin, both being opioid agonists[9] and possessing comparable potency (Kaiko et al.,

[6] It didn't. An investigation by LA Times (Ryan et al., 2016) concluded that OxyContin lasted shorter than 12 hours for many patients, regardless of dosage and formulation. On the two-pills-a-day scheme, these patients usually found their pain insufficiently managed.

[7] On the 12-hour schedule recommended by Purdue Pharma, many patients developed opioid withdrawal symptoms, including cravings for OxyContin.

[8] GAO (2003) concluded that Purdue Pharma's sales force actively promoted OxyContin to physicians, especially primary care specialists, for treating moderate-to-severe noncancer pain.

[9] To be more precise, both heroin and oxycodone are pure opioid agonists in the Phenanthrenes family, the prototypical opioid group including (but not limited to) morphine and hydrocodone (Trescot et al., 2008).

1981; Curtis et al., 1999). Their analgesic effects stem from the ability to bind to opioid receptors in the brain, effectively blocking the transmission of pain signals [10]. Moreover, the increasing availability of heroin from 2000 to 2010 further facilitated this shift. In the early 2000s, OxyContin was sometimes referred to as "poor man's heroin." due to its relatively lower cost when covered by health insurance (US Dept of Justice and of America, 2001). However, its street prices were much higher[11]. Consequently, uninsured nonmedical users of OxyContin have been consistently switching to heroin[12], a more affordable alternative[13], well before the reformulation of OxyContin. The increase in demand for heroin was swiftly met by an expansion of international drug trafficking activities in the United States, creating a well-established supply (of Justice, 2010). Following the introduction of the abuse-deterrent OxyContin reformulation was thus an escalated rate of heroin use and a sharp increase in heroin overdoses (Alpert et al., 2018; Powell and Pacula, 2021). Notably, the surge in post-reformulation demand for heroin did not significantly increase its street price (Alpert et al., 2018).

3 Effects of OxyContin Reformulation on Homicide

3.1 Data

3.1.1 Homicide Rates

I use the publicly available version of the National Vital Statistics System's (NVSS) Multiple Cause of Death data from CDC WONDER to construct state-level homicide rates from 2000 to 2017. Operated by the National Center for Health Statistics (NCHS), this is the census of certificate-based deaths in the United States. I define homicide victims as those whose cause of death was due to assault or sequelae of assault. This definition is consistent with CDC's Web-Based Injury Statistics Query and Reporting System (WISQARS) Fatal Injury Report's characterization of homicide and suicide in terms of ICD-10 codes[14]. Table 1 displays the ICD-10 coding schemes for homicide.

For constructing state-level homicide rates, I choose NVSS Multiple Cause of Death data over police report-based sources, such as the FBI's Uniform Crime Report (UCR) or National Incidence-Based Reporting System (NIBRS), because it is more representative of the United States residents

[10] There are three main types of opioid receptors in the human body: Mu (μ), Kappa (κ), and Delta (δ). Heroin and oxycodone primarily stimulate μ receptors (the "morphine receptors") to produce analgesia (Trescot et al., 2008)

[11] According to the Cincinnati Police Department, the markup could be over 1000%.

[12] A typical progression from OxyContin prescription to heroin use, according to LA Times (Ryan et al., 2016), is the following. Patients prescribed OxyContin under Purdue Pharma's 12-hour recommendation often found the drug's effects wearing off prematurely. This led physicians to either prescribe it more frequently or at higher dosages and patients to deviate from the recommendation to satisfy their pain management needs. However, insurance companies frequently denied claims for additional prescriptions, leaving patients with limited options and pushing some towards heroin as an alternative.

[13] Police reports indicated that typical nonmedical users of OxyContin could sustain their habit at a cost of one-third to one-half that of prescription opioids by switching to heroin.

[14] Excluding deaths due to terrorism. The underlying data for the WISQARS Fatal Injury Report is also NVSS's Multiple Cause of Death. In this sense, the two data sources are equivalent. Another data source of violent death is WISQARS's National Violent Deaths Reporting System (NVDRS); however, only a handful of states participated in this program in the early 2000s.

population. Police report-based data are known to systematically underreport yearly homicide counts due to non-participating police agencies and homicide cases not reported to the police. Moreover, some participating agencies only report part of the year's data (Kaplan, 2019). On the other hand, the limitation of the public version of this data is missing data due to privacy restriction[15]. I impute those missing values with murder counts obtained from the UCR Offenses Known and Clearances by Arrest data.

3.1.2 OxyContin and Pain Relievers Misuse

I follow Alpert et al. (2018) and use the National Survey of Drug Use and Health (NSDUH) for the 2004-2009 average of state-level nonmedical use rates of OxyContin as well as other pain relievers. Conducted by the Substance Abuse and Mental Health Services Administration (SAMHSA) annually, NSDUH is a nationally representative, household-level survey collecting self-reported substance use and mental health information on individuals aged 12 or above. In particular, the survey asks about its takers' history of nonmedical use of pain relievers, including OxyContin, since 2004. These measures are publicly available from NSDUH's Restricted-Use Data Analysis System (RADS)[16] in aggregated two-year, four-year, or eight-year waves. The 2004-2009 state-level measures are obtained by averaging the 2004-2005, 2006-2007, and 2008-2009 corresponding values to mitigate the concern for measurement errors (Powell and Pacula, 2021).

Although NSDUH has its limitation[17] The main advantage of NSDUH is that the survey questions are substance-specific. Moreover, they specify non-medical use of these substances[18]. This makes NSDUH ideal for information measuring OxyContin and other pain reliever misuse rates for the years before the reformulation of OxyContin.

3.2 Summary Statistics

I present the averages of state-level intentional death rates, substance misuse rates, socio-economic characteristics, and demographics between year 2000 and year 2009 (inclusive) in Table 2. Consistent with prior literature, I group these statistics into three groups. The "All" column corresponds to statistics of all 50 states plus the District of Columbia. I then characterize states either as "high" or "low," depending on whether their 2004-2009 OxyContin misuse rates are above or below the

[15]CDC WONDER has the following restriction regarding output on its data portal: if the requested number of deaths is smaller than 10, the number will be suppressed and labeled as such. If the number of deaths is lower than 20, this number will be reserved. However, the crude and age-adjusted rates will be labeled "Unreliable."

[16]https://datatools.samhsa.gov/. Note that the variable of interest, OXYCONTIN, with the label "EVER USED OxyContin NONMEDICALLY," is not directly available from RADS at the state level. When requested, the system will suppress the results on the grounds of confidentiality. However, the website allows its users to recode the requested variables. After grouping the original codes into either 0 or 1, state-level OxyContin can be accessed.

[17]Underreporting is common in such survey questions. Harrell(1997) documents that for questions in NSDUH asking about the use of illicit drugs, the accuracy rates range from 68 to 96 percent. Another limitation of NSDUH is that it only surveys individuals with valid addresses (i.e., it leaves out the homeless population).

[18]NSDUH does not have information on legitimate use of OxyContin and other pain relievers; such information would have to come from data sources such as DEA's Automation of Reports and Consolidated Orders System (ARCOS).

median.

A few patterns can be observed in this table. Regarding demographics, The "high" states are typically smaller in population sizes, whiter, and older. They also exhibit higher misuse rates of pain relievers other than OxyContin, suggesting a positive correlation between nonmedical use and related substances. Lastly, the "high" states, on average, saw lower homicide rates in the ten years before OxyContin reformulation. These findings suggest the possible omitted variable bias in the baseline specification. I will investigate this possibility in the main results and the robustness checks sections.

3.3 Descriptive Statistics on Homicide Rates

I begin the investigation of the effects of OxyContin Reformulation on homicide rates by describing how these rates evolved between 2000 and 2017 and how the patterns of evolution differ by pre-reformulation OxyContin misuse rates. To facilitate the description, I present the annual averages of homicide rates in Figure 1, grouped by states with greater-than-median 2004-2009 OxyContin misuse rates (the "high" states) and the rest (the "low" states).

From Figure 1, we can observe that homicide rates are consistently higher among "low" states than the rates among "high" states between year 2000 and year 2017. Moreover, Between the years 2000 and year 2013, homicide rates in both groups decreased, though not linearly, and eventually went up. However, they do not evolve in parallel. Instead, these two series display a tendency to converge, especially towards the end of the study period: the difference in homicide rates between the "low" and "high" states in 2017 is about 0.49 homicides per 100,000 residents, compared to 1.58 homicides per 100,000 residents in 2000. This pattern, graphically, is contributed by three observations. First, the decrease in homicide rates is more significant among "low" states, both in absolute and relative magnitude. Between the years 2000 and year 2013, there was a 1.11 homicide per 100,000 residents, or roughly 17 percentage points, decrease in the homicide rates among the "low" states, compared to 0.26 homicide per 100,000 residents, or about 5.4 percentage points decrease among their counterparts. Second, immediately after the year 2010, the homicide rates among "high" rates states displayed a more robust trend reversal pattern than those among "low" states. Third, the homicide rate among "high" states in 2017 exceeds its 2000 value, while the same pattern is not observed for the "low" states. These observations, though graphical, provide suggestive evidence that OxyContin reformulation is associated with an increase in post-reformulation homicide rates and that such association is stronger among states with relatively higher rates of pre-reformulation OxyContin misuse. At the same time, the observed differential evolving patterns of these rates pre-reformulation might raise the concern whether such association is, in fact, causal.

To further explore the graphical evidence that supports the differential impacts of OxyContin reformulation on intentional death rates, I present the differences between the "high" and "low" state homicide rates in Figure 2. I also include fitted lines mimicking pre- and post-reformulation trends under two different pre- and post-reformulation periods: the left panels display matched

lines for "symmetric" study periods, excluding 2010, and the right panels are for the entire study periods. It is relatively easy to spot a "trend break" behavior in the left panel of Figure 2, but not so for the right panel. These observations add to the conclusion that, at least graphically, OxyContin reformulation is associated with differential impacts on intentional death rates, depending on higher or lower levels of pre-reformulation OxyContin misuse. The question of whether this association is causal will be investigated in subsequent sections.

3.4 Empirical Strategy

I start by using an event study specification to study the causal effect of OxyContin reformulation on intentional deaths, leveraging the cross-state variations of nonmedical use of OxyContin before the reformulation:

$$y_{st} = \alpha_s + \beta_t + \delta_t * OXY\_MIS_s^{pre} + \gamma_t * OTHER\_MIS_s^{pre} + \sum_{i=m,f,o,h} \sigma_i trend_i + \epsilon_{st}$$

Next, I test a fully parameterized specification similar to that of (Alpert et al., 2018):

$$\begin{aligned} y_{st} = {} & \alpha_s + \beta_t + \sum_{i=m,f,o,h} \sigma_i trend_i \\ & + \eta_1(year - 2000) \times OXY\_MIS_s^{pre} \\ & + \eta_2(year - 2010) \times OXY\_MIS_s^{pre} \times \mathbf{1}\{year \geq 2010\} \\ & + \eta_3 OXY\_MIS_s^{pre} \times \mathbf{1}\{year \geq 2010\} + \theta_1(year - 2000) \times OTHER\_MIS_s^{pre} \\ & + \theta_2(year - 2010) \times OTHER\_MIS_s^{pre} \times \mathbf{1}\{year \geq 2010\} \\ & + \theta_3 OTHER\_MIS_s^{pre} \times \mathbf{1}\{year \geq 2010\} + \epsilon_{st} \end{aligned}$$

. In these two equations, subscripts s and t represent state and year, respectively. y is a vector of state-level homicide rates or suicide rates. OXY_MIS^{pre} and $OTHER\_MIS^{pre}$ are 2004-2009 state-level OxyContin misuse rates and other pain relievers misuse rates, respectively. Alternatively, I test the baseline specification where the independent variable of interests is replaced by ratios of OxyContin misuse rates and other pain relievers misuse rates as in (Alpert et al., 2018) as a robustness check. $\mathbf{1}\{year \geq 2010\}$ is an indicator variable for the passage of the reformulation of OxyContin. I define the year 2010 as the first year of receiving complete treatment even though it is partially treated[19]. α_s are state fixed effects, accounting for time-invarying cross-state differences. I include all 50 states plus the District of Columbia in this paper. I also have β_t to account for national-level shocks (e.g., federal regulation on drugs, economic recession) that vary across time. The study period is from 2000 to 2017. ϵ are the error terms assumed to be normally distributed. The standard errors of estimates are clustered at the state level. All regressions are weighted by state population estimates obtained from the SEER program.

[19]Previous literature uses 2011 as the first year of receiving full treatment (e.g., Alpert et al. (2018)

The estimates of interest for the event study specifications is the vector of δ_t, which captures the average would-have-been differences of each year's intentional death rates if there were a one percentage point increase in state-level non-medical use of OxyContin before the reformulation. The identifying assumption for these estimates to be causal is that the trends for these differences would not change if the reformulation had not happened. Although this assumption could not be tested directly, evaluating the pattern of pre-reformulation estimated coefficients would provide suggestive evidence of its validity. For this purpose, the interaction term of the pre-reformulation OxyContin misuse rates and the dummy variable for 2009 are excluded, with its coefficient normalized to zero. I report the point estimates of δ_t graphically, along with their upper and lower bands of 95% confidence intervals.

The estimates of interest for the fully parameterized specifications are vectors of η_i and $\theta_i, i = 1, 2, 3$. The specification allows pre-reformulation OxyContin misuse rates and other pain reliever misuse rates to have linear time trend effect ("pretrend") on homicide rates, captured by η_1 and θ_1. It also allows these variables to have separate post-reformulation average treatment effects ("shift"), captured by η_3 and θ_3, respectively. Furthermore, these two effects are allowed to have their post-reformulation time trends ("posttrend") on homicide rates, measured by η_2 and θ_2. Unless otherwise noticed, I report them in all regression tables in the current section.

$trend_i, i = m, f, o, h$ is a set of four linear pre-reformulation time trends, where m denotes states bordering Mexico[20], f denotes Florida, o denotes Hawaii and Alaska (the "offshore" states), and h represents states reported by the police to have frequent, significant heroin seizures before the reformulation of OxyContin[21]. The majority of illicit substances consumed within the United States are trafficked illegally into the country internationally, with heroin mainly originating from Central American and South American countries. While Florida was traditionally the primary first stop of South American heroin distribution, from 2000 onward, states bordering Mexico have gradually become hubs for initial trafficking and distribution of heroin within the United States. The relatively high level of drug trafficking and distribution activities in these areas had existed long before the reformulation of OxyContin, possibly driving up post-reformulation homicide rates in those states through mechanisms unrelated to the substitution toward heroin. Another reason to include these trends, especially for the states bordering Mexico, is to mitigate the concern for competing substitutes of heroin. It is well documented that the counterfeit drug markets in Mexico allow US medical tourists to obtain fake prescription drugs without a prescription (Mackey and Liang, 2011; Dégardin et al., 2014; Friedman et al., 2023). Suppose a fake original formulated Oxycontin could be obtained just by crossing the border post-reformulation. In that case, some nonmedial users of OxyContin might opt for the counterfeit version of the original OxyContin instead of heroin, thereby reducing the validity of the treatment in these states. Although the sign of the net effect of omitting these trends is ambiguous, not including them will likely bias the estimated effects of the reformulation of OxyContin on post-reformulation homicide rates.

[20] CA, AZ, NM, TX.

[21] AZ, CO, FL, IL, NY, WA. Source: Metropolitan Areas Most Often Identified as Origination and Destination Points for Seized Drug Shipments, by Drug, 2008-2009 (National Drug Threat Assessment, 2010)

The baseline fully parameterized specification also accounts for pre- and post-reformulation linear time trends whose intensities depend on pre-reformulation nonmedical use rates of OxyContin and other pain relievers. The inclusion of pre-reformulation time trends aims to address concerns that factors related to nonmedical OxyContin use could bias the estimated impact of its reformulation on homicide rates. For instance, it is documented that nonmedical users of OxyContin have switched to heroin well before the reformulation due to the significant cost difference between diverted OxyContin and heroin, which might lead to increased heroin demand and homicides. In other words, pre-reformulation OxyContin misuse could be a determinant of the pre-reformulation heroin market. Without addressing these concerns, it would be challenging to determine whether the rise in post-reformulation homicide rates is a result of violence linked to pre-existing heroin markets or new demand caused by the reformulation. Moreover, the nonmedical use of other pain relievers, which include a wide range of drugs, such as other prescription opioids, stimulants, and antidepressants, could also influence homicide rates through their regulatory measures and interactions with illegal markets.

On the other hand, including post-reformulation trends aims to discern the contemporary effect from the long-run impact on homicide due to the reformulation of OxyContin. For example, a surge in heroin demand among neighborhoods with previous nonmedical OxyContin users could temporarily increase homicide rates due to escalated illicit drug market activities. However, if abuse of the reformulated OxyContin persists or if heroin demand among former OxyContin users remains high, it would indicate a more enduring effect of the reformulation on homicide rates. Therefore, analyzing post-reformulation trends is essential to separate the short-term impact of OxyContin's reformulation from its more prolonged influences. Therefore, the inclusion of these pre- and post-reformulation trends is crucial for identifying how the reformulation of OxyContin affected homicide rates causally.

3.5 Main Event Study Results

3.5.1 Baseline Specification Result

Figure 3 graphically presents OxyContin reformulation's estimated effects on homicide rates from the basic event study specification. The estimated coefficients of interest, δ_t, show a downward trend in the early 2000s. However, this trend is quickly reversed and estimated coefficients remain close to zero before 2009. Moreover, all of them are statistically indistinguishable from zero at the 95% confidence level. These observations suggest that the "parallel trend" assumption is likely satisfied. In contrast, δ_{2010} is positive and is statistically significant at the 95% confidence level. δ_{2011} is observed to have a similar magnitude, though it is only significant at the 90% confidence level. Beyond 2011, the estimated coefficients show a decreasing trend, all statistically indistinguishable from zero.

The findings in Figure 3 suggest that the reformulation of OxyContin increased post-reformulation homicide rates and this increase is larger in states with higher pre-reformulation OxyContin misuse rates. Furthermore, this effect is likely transitory. This is in contrast to Park (2021), where the

author found that the increase in post-reformulation homicide rates persisted or even increased in magnitudes in the long run. As discussed in the empirical strategy section, this could be partly attributed to the pre-existing trends in homicide rates in certain states that are unlikely accounted for by the inclusion of demographic and economic variables.

3.5.2 Robustness Checks

I conduct additional tests by perturbing the basic event study specification to investigate whether the relationship between OxyContin Reformulation and homicide rates, observed in the previous section, is robust. The estimated coefficients on the independent variable of interest, as well as their 95% confidence intervals, are presented in Figures 4, 5, and 6 in the Appendix.

I start by testing the baseline event study specification, adding the pre-reformulation time trend for states on the US-Canada border. Panel (a) of Figure 4 presents the estimation results, which show a close pattern to Figure 3. This is not surprising as Canada was not a major origin of international heroin in the United States historically.

I then test whether the considered relationship holds when using an alternative measure of treatment. In panel (b) of Figure 4, I use ratios of OxyContin misuse rates and other pain relievers misuse rates instead of OxyContin misuse rates as an alternative measure of the independent variable of interest. The purpose is to better isolate the substance-specific effects of OxyContin misuse on homicide, given concerns that abusers of other pain relievers (e.g., hydrocodone) might also switch to heroin after the reformulation. Generally, the estimated coefficients are found to be greater in magnitude. Concerns about pre-existing trends in homicide rates might be raised; however, coefficients from the years just before the reformulation are relatively small and cannot be statistically distinguished from zero at a 95% confidence level. Moreover, the coefficients from 2001 and 2004 show a different trend than those from 2005 to 2008, with a smaller slope in the line connecting the 2005 and 2009 coefficients compared to the 2003 to 2005 period. The post-reformulation pattern of coefficients displays the same pattern observed in Figure 3 if not a stronger one.

Next, the following modifications are applied to the baseline event study specification: using state population estimates from the Census, shortening the study period from 2003 to 2017, using crude homicide rates instead of age-adjusted rates, and using robust standard errors instead of clustered standard errors. I present these test results in panels (c), (d), (e), and (f) of Figure 4, respectively. The observed patterns are similar to Figure 3, although in panel (f) δ_{2010} is only statistically significant at the 95% confidence level. These observations suggest that the effect is robust to these marginal perturbations to the baseline specification. However, serial correlation across different time periods needs to be accounted for to identify the effect of the reformulation of OxyContin on homicide rates.

To assess the impact of excluding one state at random on the estimated results of the baseline event study specification, I compute the leave-one-out distribution of the point estimates along with the upper and lower bounds of their corresponding 95% confidence intervals. I present the

means calculated from these sampling distributions in panel (a) of Figure 5. The observed pattern is very similar to that from the baseline specification. To further assess the extent of perturbation to my benchmark brought by the random exclusion of one state, in panel (b), I present the means of estimated coefficients and their corresponding extended upper and lower bounds, where the extended upper (lower) bound of an estimated coefficient is defined to be the 95% upper (lower) bound of its average upper (lower) bound, inferred from its sampling distribution. The extended bounds in panel (b), by design, are more expansive than their counterparts in panel (a). However, the deviations are generally minor, suggesting that the effect of the OxyContin reformulation on homicide is robust to random removal of one state.

Lastly, I investigate whether excluding states with specific ranges of pre-reformulation non-medical use rates of OxyContin would alter the pattern observed in Figure 3. States are divided into five (approximately) equally populated groups based on their 2004-2009 nonmedical OxyContin misuse percentile rankings. The baseline specification is then estimated, leaving out one state group each time. I present the estimation results of these five tests in panels (a) - (e) of Figure 6. The exclusion of states within either the 0th - 20th percentile or the 80th - 100th percentile range of pre-reformulation OxyContin misuse rates would dampen the pattern observed in Figure 3. This finding suggests that the average treatment effect of the reformulation primarily comes from comparing homicide rates between states between states at the extreme ends of pre-reformulation OxyContin misuse rates distribution.

3.5.3 Sensitivity Analysis

This section investigates how the benchmark relationship would vary if additional control variables were included. I consider adding the following six sets of time-varying covariates, all at the state level, to the baseline specification: (1) log state populations, (2) drug control policies, including naloxone laws, prescription drug monitoring programs (PDMP), medical marijuana laws, and recreational marijuana laws, (3) unemployment rate, (4) percentage of state population with college degrees (5) male percentage, percentage white, percentage black, percentage Hispanic, and (6) share of population aged between 20 and 40. I test the sensitivity to the inclusion of these variables in two ways. In the first set of tests, I test six specifications in which these covariates are added sequentially. In the second, I test six specifications with each set of covariates added individually. I present the estimated coefficients along with their 95% confidence intervals in panels (a) - (f) of Figure 7 and Figure 8, respectively.

From panels in Figure 7, I find that including these variables jointly strengthens or does not dampen the benchmark pattern observed in Figure 3. Generally, the pre-reformulation estimated coefficients between 2002 and 2008 are close to zero and do not show a clear upward trend. On the other hand, the estimated coefficient of 2010 stays statistically significant at the 95% confidence level throughout these six specifications. δ_2011 are typically significant at the 90% confidence level. Beyond 2011, coefficients show a downward trend toward zero, agreeing with Figure 3. Similarly, estimating the baseline specification with each of the six sets of covariates individually would not

significantly alter the identification of the effect of OxyContin reformulation on homicide rates. In particular, drug policies and economic controls help mitigate the concerns for differential pre-reformulation trends in homicide rates beyond 2005, as shown in panels (b), (c), and (d) of Figure 8.

These findings suggest that the baseline event study specification is generally insensitive to including control variables. Furthermore, the concerns over the "parallel trend" assumption for years more immediate to the reformulation can be mitigated by including a relatively small set of covariates, preferably drug policies and economic variables, in the baseline specification.

3.5.4 Falsification Test

This section investigates whether the reformulation of OxyContin influenced outcomes that are not directly associated with illicit drug market activities. Specifically, it examines the homicide rates of victims under five years old or over 70 years old. The hypothesis underlying this part of the paper is that post-reformulation increases in homicide are primarily driven by escalated illicit drug market activities, which typically involve the active participation of victims. However, indirect mechanisms, such as child maltreatment (Evans et al., 2022) and violence targeting non-participants due to intoxication, could also result in homicide. Since individuals younger than five or older than 70 are generally not active in the illicit drug market, concerns would be raised if the reformulation of OxyContin is found to have a similar impact on this population.

This section also explores the effect of the reformulation of OxyContin on victims between 15 and 24. Unlike the under-five or over-70 age groups, those between 15 and 24 are much more likely to be involved in illicit drug markets directly. According to the 2021 National Survey on Drug Use and Health (NSDUH), individuals between 18 and 25 have the highest rate of illicit drug use and dependency. Street gangs also consist of members primarily from this age range, with the younger ones being most vulnerable in conflicts between gangs. If the estimated coefficient patterns for this group mirror those in Figure 3, it would further mitigate concerns about the effect being primarily driven by indirect mechanisms.

Panel (a) in Figure 9 presents the estimated effects of the reformulation of OxyContin on the homicide rates of victims under five years old or over 70 years old. The estimated coefficients show an upward trend post-reformulation, with δ_{2010} statistically indistinguishable from zero at the 95% confidence level. Significant effects are consistently observed only after 2012, diverging from the pattern in Figure 3. Moreover, this pattern resembles previous findings on the impact of the reformulation on heroin overdose deaths (Powell and Pacula, 2021), suggesting the increase in homicides in this age group is indirectly linked to long-term opioid use,

Panel (b) in Figure 9 presents the estimated effects of the reformulation of OxyContin on the homicide rates of victims between 15 and 24. The pre-reformulation estimated coefficients show a consistent downward trend till 2010, where a significant, positive effect is observed. This increase is transitory, as the post-2011 coefficients resume the pre-reformulation downward trend. This pattern starkly contrasts with the one observed in panel (a) but aligns more with Figure 3. This

comparison indicates that indirect mechanisms do not dominate the effect of the reformulation of OxyContin on homicide rates, suggesting that the effect of the reformulation on homicide rates is less likely to be spurious.

3.6 Main Regression Results

In this section, I conduct additional econometric tests to provide strengthening evidence that the effect of OxyContin reformulation on homicide rates is causal. I test this effect using the fully parameterized or "trend-break" model. Furthermore, I investigate whether the findings on robustness checks, sensitivity analysis, and falsification tests from the event study specification hold if these tests are conducted using the current model. For each specification, I report both the point estimates and standard errors (in parenthesis) of the estimated coefficients on 2000-onward time trends (the "pretrend" term) and 2010-onward time trends (the "posttrend" term) of state-specific 2004-2009 OxyContin misuse rates as well as the captured average treatment effect (the "shift" term) on homicide rates. Unless otherwise specified, all specifications include a full set of state and year dummies and cluster their model errors at the state level.

3.6.1 Baseline Result/Sensitivity Analysis

I present the estimation result of my baseline "trend break" specification in column (1) of Table 3. The point estimate of the average treatment effect is 0.944 and is statistically significant at the 99% confidence level. The point estimate of the pre-reformulation trend is -0.0825 and is insignificant at the 90% confidence level. I then include demographic covariates[22] in the estimation of the baseline specification, and I present the results in columns (2) through (6). The point estimates of the average treatment effect are insensitive to the inclusion of demographic covariates: they maintain similar magnitudes across all specifications. Moreover, they are all significant at the 99% confidence level. In addition to this finding, the point estimate on the pre-reformulation time trend on pre-reformulation OxyContin misuse rates is small and insignificant at the 95% confidence level across these specifications. Similarly, the point estimates for the post-reformulation time trend are consistently near zero across all specifications. These findings are consistent with the observations from Figure 7 and Figure 8 that the effect of the reformulation of OxyContin on homicide is insensitive to the inclusion of demographic controls. However, including them does increase the point estimate of the shift parameter slightly without significantly increasing its standard error. On the other hand, the null findings of the posttrend term suggest that the effect of the reformulation of OxyContin on homicide is transitory[23].

I next test how the estimated treatment effect responds to the inclusion of drug policies and economic conditions with the specification used in column (6) of Table 4. I present the estimated

[22] Same as those used in estimating the event study specifications.

[23] Testing the joint statistical significance of linear combinations of the estimated shift and posttrend parameters, $\eta_3 + t\eta_2, t = 0, 1, ..., 7$, confirms that this effect is transitory. For all specifications, none of $\eta_3 + t\eta_2, t > 2$ is statistically significant at even the 90% confidence level.

coefficients of this specification with Prescription Drug Monitoring Program (PDMP), Medical Marijuana laws (MML), Recreational Marijuana laws (RML), Naloxone laws (NAL), state unemployment rates, and state percentage of the population with a college degree in columns (1) through (6) of table 4, respectively. The inclusion of MML blocks the effect of OxyContin Reformulation on homicide rates; the estimated average treatment effect drops about 7%, and its standard error increases by roughly 16%, making it only significant at the 95% confidence level. The inclusion of education attainment has a similar blocking effect, causing the estimated average treatment effect to drop about 15%. These findings are consistent with the conclusion from sensitivity analysis using event study specifications in that the inclusion of drug policies and economic conditions does not nullify the estimated causal effect. On the other hand, these findings provide suggestive evidence that there exists a potential interaction between OxyContin reformulation and other drug policies or economic outcomes.

3.6.2 Robustness Checks

I apply (a selected subset of) tests described in Figure 4 to check the robustness of the results estimated using the "trend break" model. I present these test results in columns (1), (3), (4) and (8) of table 5. In column (8), where an alternative measure of pre-reformulation exposure, ratios of OxyContin misuse rates and other pain relievers misuse rates, is used, the average treatment effect is found to be 6.355, and I fail to reject the null at the 95% confidence level. In column (1), I change the weighting population as in panel (c) of Figure 4 and record little deviation from the benchmark. Results in column (2) show that the unweighted regression inflates the shift parameter by roughly 25.6 percentage points and makes the pretrend parameter significant. In column (3), I use crude homicide rates instead of age-adjusted rates in the baseline specification and find an 11.7 percentage point increase in the estimated average treatment effect. In column (4), I use robust standard errors and see an approximately 22 percentage points increase in the standard error for the average treatment effect, making it only significant at the 95% percent confidence level. These findings suggest that the identified causal impact is generally robust to specification setup perturbations, though the shift parameter deviation could be considerable.

To complement the finding on the extent of errors induced by randomly excluding one state from the analysis using the baseline event study specification, summarized in Figure 5, I test the specification in column (6) of Table 4 with selected states excluded. In column (5) of Table 5, I exclude Hawaii and Alaska, and I find virtually the same in the estimated average treatment effect. In column (6), I exclude the state of California. This perturbation induces a roughly 27 percentage points decrease in the average treatment effect point estimate and a 25 percentage points increase in its estimated standard error, altogether making the estimated coefficient of interest only significant at the 90% confidence level. Column (7), excluding New York and Florida, reduces the point estimate of the average treatment effect by roughly 12 percentage points. However, it remains significant at the 95% confidence level. Since California is the largest state in the United States in terms of population (about 12% of the US total population), the result in column (6) alone

might suggest that large states primarily drive the effect of OxyContin Reformulation on homicide. However, the result in column (7) shows that this is likely an incomplete story as New York and Florida combined total roughly 12% of the US population. The difference between Florida and the other two states is that California and New York rank among the bottom 20% of states in terms of pre-reformulation OxyContin misuse ranking. In contrast, Florida is in the top fifth.

This observation leads to re-examining the tests described in Figure 6 using the fully parameterized specification. I conduct these tests with the specification in column (6) of Table 4, and I present their results in columns (1) through (5) of Table 6. Consistent with the finding in panel (a) of Figure 6, I find that excluding states at the bottom fifth of pre-reformulation OxyContin misuse rates distribution decreases the point estimate of average treatment effect by roughly 37 percentage points and more than doubles its standard errors, making it statistically insignificant at even the 90% confidence level. Similar to findings from panels (b), (c), and (d) of Figure 6, in columns (2), (3), and (4), I find these perturbations do not dampen the estimated average treatment effect. However, in column (5), excluding states at the top fifth of the distribution decreases the point estimate of the average treatment effect by about 18 percentage points and increases its standard error by roughly 38 percentage points, leaving it only significant at the 90% confidence level. These outcomes support the event study specification conclusions, suggesting that the states in the top and bottom fifths are key to identifying the causal effect of OxyContin reformulation on homicide rates.

I conducted two additional tests to further explore the role top and bottom fifth states in pre-reformulation OxyContin misuse rates distribution play in the identification. I present their results in columns (6) and (7) of table 6, respectively. In column (6), I exclude both the bottom and top 20% states by their rankings of pre-reformulation OxyContin misuse rates in the estimation. I find that the point estimate of the average treatment effect becomes negative, and there is little evidence for me to reject the null at the 90% confidence level. In column (7), I only include these two groups of states in the estimation. It strengthens the estimated average treatment effects: the point estimate is now 1.455, a roughly 49 percentage points increase from its benchmark, and its standard error decreases about 29 percentage points, from 0.329 to 0.232. These two findings suggest that the identification of the causal effect of OxyContin reformulation on homicide rates is primarily driven by states on both ends of the pre-reformulation OxyContin misuse rates distribution. These states only total roughly 41% of the US population[24]

3.6.3 Falsification Test

Lastly, I redo the falsification test on the homicide rates of victims under five years old or over 70 years old with the fully parameterized specification. The estimation result of the baseline specification is presented in column (1) of Table 7. The estimated "shift" coefficient is only weakly

[24]States at the bottom fifth: CA, DC, GA, ID, IA, MN, MS, NE, SD, TX. States at the top fifth: AK, IN, KY, MA, MT, NV, OK, RI, UT, WV, WI.

positive, suggesting a weak increase in homicide rates in this population post-reformulation[25]. This result is insensitive to including various combinations of demographic, drug policies, and economic conditions covariates, as shown in columns (2) through (6). These findings are consistent with those from Figure 9.

3.7 Heterogeneous Effects by Age Groups

In this section, I investigate the heterogeneous effects of OxyContin reformulation on homicide rates by age group. It is well documented that homicide occurrences are distributed unequally across the age spectrum (Almgren et al., 1998; Gartner, 1990; Pampel and Williamson, 2001; Cook, 1981). Typically, the victimization rate quickly climbs during the adolescent years, peaks around 20, and gradually declines afterward (Perkins, 1997). Whether OxyContin reformulation causally shifts this distribution remains to be tested.

I augment the baseline difference-in-difference specification by interacting with age group indicators to achieve this goal. Specifically, I test the following triple-difference specification:

$$
\begin{aligned}
y_{ast} = \alpha &+ \sum_s \delta_s State_s + \sum_t \gamma_t Year_t + \sum_a \beta_a Ag_a \\
&+ \eta_1 (year - 2000) \times OXY\_MIS_s^{pre} \\
&+ \eta_2 (year - 2010) \times OXY\_MIS_s^{pre} \times \mathbf{1}\{year \geq 2010\} \\
&+ \eta_3 OXY\_MIS_s^{pre} \times \mathbf{1}\{year \geq 2010\} \\
&+ \eta_4 (year - 2000) \times AGE \\
&+ \eta_5 (year - 2000) \times AGE \times \mathbf{1}\{year \geq 2010\} \\
&+ \eta_6 AGE \times \mathbf{1}\{year \geq 2010\} \\
&+ \eta_7 (year - 2000) \times OXY\_MIS_s^{pre} \times AGE \\
&+ \eta_8 (year - 2010) \times OXY\_MIS_s^{pre} \times AGE \times \mathbf{1}\{year \geq 2010\} \\
&+ \eta_9 OXY\_MIS_s^{pre} \times AGE \times \mathbf{1}\{year \geq 2010\} \\
&+ \epsilon_{ast}
\end{aligned}
$$

.

In this specification, y_{ast} is homicide per 100,000 individuals at age group a, year t, and state s. I consider seven age groups: 5-14, 15-24, 25-34, 35-44, 45-54, 55-64, and the reference group of victims below five or above 65. The age-specific homicide victim counts are similarly obtained from the National Vital Statistics System (NVSS) and complemented by Supplemental Homicide Reports (SHR)[26]. I estimate age-state homicide rates by dividing these counts by population estimates in their corresponding cells, calculated from the SEER program. The standard errors are clustered at the state and age group levels.

[25] The test for $\eta_3 + t\eta_2, t = 0, 1, ..., 7$ shows that, at best, these linear combinations are borderline significant at the 90% confidence level when t approaches 7.

[26] Due to missing data.

I include a set of state, year, and age group dummies in this specification. In particular, I have age group dummies instead of age group-by-state dummies. This is based on the following two observations. First, institutions and the culture in the United States display differential attitudes towards their subjects based on age. However, such differences are not likely to differ massively across states. Second, and more closely related to the current discussion, homicide-generating activities are associated with participants' age, and such associations are not likely to differ across states. For example, New York and Montana have different rates of homicide related to drug trafficking. This is most likely due to varying levels of organized crimes operated by similarly aged individuals in these two states. Still, it is unlikely (hypothetically) that in Montana, most of the drug trafficking activities are conducted by senior citizens.

AGE is an indicator of a specific age group, equals one if an observation is of that age group and zero otherwise. The specification allows pre-reformulation OxyContin misuse rates and the chosen age group to have a linear time trend effect on homicide rates, captured by η_1 and η_4. It also allows these variables to have separate post-reformulation average treatment effects, captured by η_3 and η_6, respectively. Furthermore, these two effects can have their post-reformulation time trends on homicide rates, measured by η_2 and η_5. However, the estimates of interests are η_7, η_8, and η_9 as they capture the various OxyContin misuse-induced effects between the chosen age group and the rest. Precisely, η_7 measures possible differential time trend of pre-reformulation OxyContin misuse rates on homicide rates between the chosen age group and all others. η_9 measures the age group-specific average treatment effect of reformulating the selected age group over the other age groups. η_8 captures this effect's possible lieanr post-reformulation time trend.

I conducted six tests, each regarding one designated age group excluding the reference group. I test six specifications for each age group-specific test with different combinations of year-state-age group level demographics, including log population, percentage male, percentage white, and percentage of Hispanic origin. I only report $\hat{\eta_9}$ for each specification. I organize test results into panels A through F in Table 8. Within each panel, I present the point estimate and standard error (in parenthesis) of η_9 in columns (1) through (6) for each specification.

In column (1) of Table 8, I find that the estimated age group-specific differential average treatment effect for victims aged 15-24 is positive at 8.152, the largest among all age groups. I cannot reject the null that it is statistically significant at the 95% confidence level. It is about 2.7 times the size of that for victims aged 25-34, measured at 3.062 and statistically significant at the 95% confidence level. In contrast, this effect is negative for all other victim age groups. This pattern largely persists through the rest of the specifications. In particular, the estimated impact for victims aged 15-24 is not sensitive to inclusions of demographic covariates. The significance level stays constant across these specifications, and the point estimate increases by about four percentage points when estimated with a full set of controls. These findings suggest that OxyContin reformulation induces a differential rise in young homicide victims aged 15-34 compared to other age groups. Moreover, this differential effect concentrates on homicide victims aged 15-24.

This research suggests that the observed increase in homicides is not primarily driven by psy-

chopharmacological violence, i.e., violence under the influence of drugs. This conclusion is drawn from the difference-in-difference estimator, which compares the impact in states with high and low levels of OxyContin misuse. The data does not support the hypothesis that heroin, a common substitute for Oxycodone, has a distinctly different effect on users' propensity for violence.

Similarly, the rise in homicides does not appear to be motivated by the need for financial resources to sustain drug habits. If this were the case, it would imply higher prices for OxyContin substitutes like heroin and hydrocodone, but the opposite is true. Heroin, notably cheaper than OxyContin by 2009, had already seen a switch by users before OxyContin's reformulation due to its lower cost. Cicero et al. (2013) also found that cost was not a primary factor in the choice of OxyContin over hydrocodone. Furthermore, police reports indicate that while robberies and assaults are common among drug users seeking money, homicides are rare in these circumstances.

Therefore, the increase in homicides likely stems from illegal market-generated violence. Three lines of evidence support this hypothesis. First, prior literature in criminology, sociology, and economics links gang activities to homicide, emphasizing violence in illicit markets due to the absence of formal contracting. Second, research shows that gang violence is often related to protecting territory, with younger, low-level gang members most involved in these conflicts and facing higher mortality rates. Third, Owen (2014) points out that homicide victims in market-based crimes tend to be younger, a pattern I have replicated in my research (Figure 11), further supporting the market-generated homicide hypothesis.

4 Homicide-Reduction Hypothesis of MML

Thus far, the analysis has been focusing on establishing a (plausibly causal) econometric relationship describing how OxyContin reformulation affects homicide victims. Still, building an applicable economic theory is far from complete without investigating the source of this chain reaction: the substitution towards heroin. Suppose we could find natural experiments that help reduce this incentive. In that case, they might educate us on how to mitigate the increase of homicide victims in the face of similar supply-side policy changes. Medical marijuana legalization seems to be a worthy candidate due to the analgesic potential of medical marijuana. However, this is baffled by the apparent inconsistency in both medical and economics literature on whether medical marijuana legalization increases or decreases opioid overdose deaths, notably Mathur and Ruhm (2023) and (Powell et al., 2018). This section first uses a stylized, probabilistic framework to provide a plausible explanation for this inconsistency. Next, it proposes a simple yet novel identification strategy to test the homicide-reduction hypothesis of medical marijuana laws. It then provides empirical evidence on whether these legislative changes reduce homicide victims post-reformulation and where this effect is concentrated.

4.1 A Response to Mathur and Ruhm (2023)

4.1.1 The Problem

Medical marijuana legalization presents itself as a worthy candidate for such intervention due to its analgesic properties (Reiman et al., 2017; Caldera, 2020; Carlini, 2018; Bicket et al., 2023). If medical marijuana can replace opioids as a pain management solution for specific conditions or at least help reduce its usage, a standard economic argument would suggest that medical marijuana legalization could help reduce homicide victims. Consider a rational agent with an urgent need for pain management faces two choices: medical marijuana and illicit opioids, such as heroin. He will choose the option with the lowest opportunity cost. However, the subjective evaluation of this economic cost likely depends not only on the intrinsic property of the drug but also on the associated uncertainty and stigma attached to its legal status. Medical marijuana legalization helps lower the opportunity cost of choosing medical marijuana by removing its otherwise illegal status, thereby providing more incentive for this individual to stay away from illicit opioids. If there is less such demand, it will likely lead to less violence-generating illicit market activities, such as drug distribution by gangs.

However, this argument lacks unanimous support from both the medical and economics literature on the effects of medical marijuana laws on opioid overdose deaths (Hsu and Kovács, 2021; Mathur and Ruhm, 2023; Powell et al., 2018; Shover et al., 2019; Bachhuber et al., 2014; Chihuri and Li, 2019; Chan et al., 2020). The core of the inconsistency is summarized in Mathur and Ruhm (2023)[27]: extending study periods (i.e., adding more recent years of data) makes the estimated effects of various medical marijuana laws on opioid death outcomes more considerable. Moreover, these estimates are usually positive. If medical marijuana could reduce homicide victims post-reformulation, it must also reduce illicit opioids, such as heroin, and overdose deaths.

To empirically test whether medical marijuana legalization reduces homicide victims post-reformulation, it is imperative to understand why the effects of such legislative change on opioid overdose deaths are mixed, and their magnitudes are dependent on the choices of study periods. The argument that it *could* prevent homicide deaths in the current context relies on the assumption that marijuana is a substitute for illicit opioids, such as heroin, which *should* lead to decreases in opioid overdose deaths. This does not gain unanimous support from the existing literature. Furthermore, even if we could posit that the inconsistency is likely caused by a myriad of underlying mechanisms, how to separate the effect due to the desired one - that medical marijuana legalization reduces opioid overdose deaths by incentivizing substitution towards marijuana - from the rest remains unanswered, which would also invalidate this argument.

[27] To my best knowledge, there is little discussion on this topic in recently published works or working papers in economics that cite this paper (Dave et al., 2023; Abouk et al., 2023; Drake and Ruhm, 2023; Ali et al., 2023).

4.1.2 Response

I begin with an observed pattern in the literature linking medical marijuana legalization and opioid overdose deaths: the earlier the publication date, the more likely the study finds a negative effect. Conversely, more recent studies incorporating newer data (i.e., more extended study periods) overwhelmingly report a positive association between the two. While the "gateway drug theory" (Sabia et al., 2021; Chu, 2015), the rise of fentanyl (Chihuri and Li, 2019; Mathur and Ruhm, 2023), the granularity of observations (Hsu and Kovács, 2021), and stringency of cannabis laws (Powell et al., 2018) could all contribute to this pattern, the literature has been consistently hesitant to discuss *who* are affected by this legislative change and *how* this marginal population evolves. Similar[28] to Lucas Jr (1976), the underlying population might respond to previous or contemporaneous policy changes other than medical marijuana laws and evolve accordingly, even if the implementation of these policies are entirely orthogonal to each other. If the assumption of a stable underlying population is broken, then the effect captured by standard econometric or statistical apparatus will likely reflect this change rather than the actual impact.

Between 2000 (or mid-1990s) and 2010, the drastic increase in prescription opioid use and overdoses was primarily due to an increase in the use of opioids to treat chronic noncancer pain (Boudreau et al., 2009; Von Korff et al., 2008), likely driven by a combination of aggressive pharmaceutical marketing similar to that of OxyContin (Van Zee, 2009) and social/regulatory policy advocating for the liberal use of opioid analgesics (Veterans Health Administration, 2000; Joint Commission on Accreditation of Health Care Organizations, 2001). Between 1991 and 2011, the estimated total number of opioid prescriptions in the United States increased from 76 million to 219 million (Compton et al., 2015). Between 2000 and 2010, the estimated prescription opioid users among adult Americans jumped roughly 60 percentage points, whereas major disability and health status metrics either declined or stayed unchanged among the users (Sites et al., 2014). The cross-state evaluation shows that variation in underlying health status cannot sufficiently explain variation in opioid prescription behaviors (Paulozzi et al., 2014). The amount of opioids prescribed in the United States peaked in 2010 and then decreased each year through 2015. Despite reductions, the amount of opioids specified remains approximately three times as high as in 1999. The substantial variation in opioid prescribing observed at the county level suggests inconsistent practice patterns and a lack of consensus about appropriate opioid use (Guy Jr et al., 2017). Patients routinely received opioid prescriptions from procedures as standard as wisdom tooth extraction, estimated to have been performed over 3.5 million times in 2004 (Harbaugh et al., 2018).

As the nation became increasingly aware of the ongoing opioid crisis, an array of restrictive supply-side policies aiming at reducing access to prescription opioids have been implemented at the national and state level, including OxyContin reformulation in 2010, pain management clinic laws (the "pill mill" laws), must-access prescription drug monitoring programs (PDMP), the rescheduling of Tramadol in 2014 (Gupta et al., 2023), and CDC Guideline for Prescribing Opioids for Chronic Pain in 2016 (Busse et al., 2016; Dowell et al., 2016), resulting consistent decrease in prescription

[28] Not the same, as it talks about the anticipatory effects of rational agents that render policies ineffective.

opioids consumption (Guy Jr et al., 2017). In addition, in recent years, Medicaid programs have been pursuing strategies for alternative, non-opioid solutions for managing chronic pain (Traylor, 2019). The post-2010 change of wind likely gears physicians toward the more conservative side of prescribing opioid painkillers. Recent studies have found that patients already on prescription opioids are routinely denied treatment (Lagisetty et al., 2019, 2021), which again leads to illicit drug use for certain patients (Ti et al., 2015). Overly restrictive applications of CDC's prescription guidelines have been reported (Dowell et al., 2019), leading to opioid dose tapering and sudden discontinuation even for patients with pain associated with cancer, surgical procedures, or acute sickle cell crises (Kroenke et al., 2019) and severe withdrawal symptoms, uncontrolled pain, psychological distress, and suicide (Food et al., 2019).

The inconsistency in consideration is, therefore, likely due to the reduction of the marginal population affected by legal medical marijuana over time, that is, patients whose underlying health conditions are manageable by medical marijuana but prescribed opioids instead. I use a simple probabilistic framework of a healthcare system for pain treatment to illustrate this point, especially before 2010. I assume all functions are C_1 in the following discussion.

Consider a continuum of patients with condition $c \in [0,1]$ and the associated pain level $p(c)$, where $p : [0,1] \longrightarrow \mathcal{R}^+$ is a bounded, strictly increasing function that maps each condition to some level of pain. Each patient pays a flat rate of $b > 0$ to be eligible to participate in a healthcare system, hoping to have their conditions treated.

There are two types of physicians working voluntarily for the system. One is the standard type, which always prescribes the right amount of prescription opioid for a given pain level, $s(p)$, where $s : \mathcal{R}^+ \longrightarrow \mathcal{R}^+$ is a bounded, strictly increasing function that maps pain levels to quantity of prescription. The other type of physicians are "quacks" who consistently overprescribe opioids for given levels of pain. This behavior is captured by a scalar $r \in [0,1)$; that is, the prescription from a quack of a given pain level is $s(p)(1+r)$.

The system does not directly control physician behaviors; it matches patients and physicians randomly. The fraction of quacks in the system is assumed to be q. Thus, the expected prescription of a patient with pain level $p(c)$ is $(1-q)s(p) + qs(p)(1+r)$.

Patients are sorted into the system by their pain levels but may or may not be treated due to scarcity of resources. The system aims to maximize coverage, but it can do so only by spending up its budget. Thus, the fraction of untreated patients, c_u, is determined by the following resource constraint:

$$\int_{c_u}^{1} [(1-q)s(p(c)) + qs(p(c))(1+r)]dc \leq b$$

. Thus, a patient with condition c is treated if $c \geq c_u$.

The outcome of interest, the prevalence of substance use disorder, is modeled as an externality concerning the system. I assume that both untreated patients and patients treated by quacks could develop substance use disorders, but the patients treated by normal physicians would not. Formally, the probability of an untreated patient with pain level p is $d_u(p)$, where $d_u : \mathcal{R}^+ \longrightarrow (0,1)$ is a non-decreasing function that predicts an untreated patient's chance of using opioid nonmedically.

For a treated patient by quacks, the probability of developing substance use disorder is the product of two parts: the baseline part and the penalty part. The baseline part depends only on its pain level p, $d_t(p)$, where $d_t : \mathcal{R}^+ \longrightarrow (0,1)$ is a non-decreasing function. On the other hand, the penalty term, $m(r)$, is a description of how quacks overprescribe painkillers. $m : [0,1) \longrightarrow [1,\infty)$ is a strictly increasing function with $m(0) = 1$. The expected probability of a treated patient with pain level p developing substance use disorder is therefore $qd_t(p)m(r)$. Taken together, the expected total prevalence of substance use disorder generated by this healthcare system is

$$E[Prevalence] = \int_0^{c_u} d_u(p(c))dc + \int_{c_u}^1 qd_t(p(c))m(r)dc$$

.

If we further assume $d_u(p) - qm(r)d_t(p) \geq 0$ for $\forall p$, that is, when physicians' current prescribing behavior does not make patients more likely to develop substance use disorder by treating them, one can show that.

Result 1.

$$\frac{\partial E[Prevalence]}{\partial q} > 0$$

Result 2.

$$\frac{\partial E[Prevalence]}{\partial r} > 0$$

Result 3.

$$\frac{\partial E[Prevalence]}{\partial b} > 0$$

. Results 1 and 2 suggest that when current overprescription is moderate, an increase in such behaviors will drive up the prevalence of substance use disorder in the system. On the other hand, result 3 suggests that an increase in this system's budget, an analog to medical expenditure on pain treatment, will drive up the prevalence of substance use disorder when overprescription is present in the system. Between 2000 and 2010, it is possible that *all* three of these factors changed expansively: more physicians are incentivized to prescribe prescription opioids for non-cancer pain, higher doses for the same conditions are deemed necessary, and more money is spent on pain relief. This probabilistic framework, though omitting many details from a real healthcare system, could help rationalize how physician prescribing behaviors elevate the prevalence of substance use disorder.

Now, suppose the patients in this healthcare system are presented with the choice of medical marijuana. Depending on the relative position of the pain distribution and the highest pain level manageable by marijuana, p_m, this intervention may or may not reduce substance use disorder prevalence even assuming that *all* patients that could switch do switch. This point is illustrated in Figure 12. In panel (a), when c_m, the condition corresponding to p_m, is to the right of c_u, all untreated patients and the treated patients with conditions $c \leq c_m$ could switch to medical marijuana, thereby reducing the prevalence of substance use disorder and the system might end up with an unspent budget. In panel (b), c_m is to the left of c_u, meaning that only a portion of the

untreated patients could switch, and the observed reduction in substance use disorder is less than that in panel (a). In panel (c), when the pain distribution is above p_m, no patient could switch; therefore, this intervention should have zero effect.

Since medical marijuana can only replace prescription opioids for certain conditions, likely those associated with mild to moderate pain but not severe, cancer-related pain, when the prescribing culture is liberal, there are likely a relatively more significant number of patients whose underlying health conditions are manageable by medical marijuana but somehow uses prescription opioid for pain management purposes. This was the situation before 2010 (or 2013). Hence, the net estimated effects of medical marijuana legalization on opioid overdose deaths suggest the protective potentials of marijuana during this period. However, as the overall prescription behavior becomes more restrictive, the relative proportion of this population will likely decrease. This is equivalent to introducing a positive shock to the pain distribution of patients admitted to a system designed to treat pain, as illustrated in panel (c) of Figure 12. The estimated net effects will now primarily reflect the impact from other channels or possibly spurious correlation, thus becoming more positive. This is precisely what we observe if newer periods of data are added.

In the language of causal inference, the inconsistency in consideration is an example of attrition bias, typical among studies utilizing social experiments or panel surveys with extended periods (Hausman and Wise, 1979). When using a difference-in-difference (DID) research design to study the effect of medical marijuana legalization on opioid overdose deaths, the presence of such bias will render estimated treatment effects inconclusive. One can show that this bias is positive if we assume that the potential switchers to medical marijuana had lower opioid overdose death rates than those non-switchers[29], which is likely the case as there are people who do not use medical marijuana but use diverted prescription opioids nonmedically.

This discussion is particularly relevant in the policy decision-making involving legalizing medical marijuana. Marijuana activist groups have long hailed medical marijuana legalization as a holy grail in combating the ongoing opioid crisis, stressing heavily on the potential of medical marijuana's ability to reduce prescription opioid consumption and the resulting opioid overdose deaths. My analysis and recent medical and economics literature developments suggest it might be an overstatement. In particular, it is recommended that policymakers carefully assess the evolving population to which medical marijuana laws might affect before reaching a decision.

4.2 Identification

4.2.1 The Identification Problem

The core of the discussion from the last section is that the identification of how medical marijuana laws affect opioid deaths requires the characterization of the population affected by them. Suppose we use difference-in-difference to estimate this effect, as with most of the economics literature on

[29] It can be shown that the bias is $(p_1 - p_2)[(E[Y_t|pre, U] - E[Y_t|pre, S]) + (E[Y_c|pre, U] - E[Y_c|pre, S])]$ where $p_1 - p_2$ is the rate of attrition for switchers, S represents potential switchers, U represents non-switchers, Y is opioid overdose incidence, t and c represent treatment and control groups by medical marijuana legalization.

this topic, formally, it is easy to see that.

$$\hat{\delta_{DID}} = \sum_{i}^{n} p_i \hat{\delta_{DID,i}}$$

where $\hat{\delta_{DID,i}}$ is the DID estimator of medical marijuana laws on subpopulation i whose proportion is p_i. These proportions are unobserved with state-level data, and these characteristics are often hidden even with individual-level data. If we want to estimate the legislation on a specific subpopulation, we also need knowledge of its characterization, even though we do not care about the rest. With state-level data, this is impossible for similar reasons. The third option is to find a proxy for this subpopulation. Even though we do not observe its characteristics directly, in states where this subpopulation is large, the net estimated effects will likely be dominated by the effect on this specific subpopulation. I discuss one such proxy in the next section.

4.2.2 The Identifying Assumption

Since my goal is to estimate how medical marijuana laws prevent homicide through the opioid-use-reduction channel post-reformulation, I need a proxy for its marginal population - patients whose underlying health conditions are manageable by medical marijuana but prescribed prescription opioids - pre-reformulation. According to the discussion from section 7.1, there is already one candidate. I claim that

Result 4. *Pre-reformulation OxyContin misuse rate is a proxy for individuals whose heroin usage could be reduced, post-reformulation and post-medical marijuana legalization.*

The argument is as follows. Results 1, 2, and 3 from section 7.1, along with numerous academic studies and news articles, suggest that it is the liberal style of how physicians prescribe opioids that contributed significantly to the widespread nonmedical use of prescription opioids between 2000 and 2010. Since the cross-state variation in opioid prescription could not be explained by cross-state variations in underlying health conditions, it is likely due to or contributed mainly by how physicians prescribe opioids as a solution to various pain conditions. Furthermore, the main difference likely occurs among patients with non-cancer related pain or mild to moderate level pain. Therefore, the states with high pre-reformulation misuse rates are states where the prescription of opioids is more liberal, which in turn indicates a larger population whose underlying health conditions are manageable by marijuana but use opioid painkillers instead.

By this argument, the identification strategy will analyze the subsample by grouping states with similar pre-reformulation OxyContin misuse rates. The specific identification strategy will be left for further discussion in the next section, but I make four predictions here. Each prediction corresponds to a particular outcome relevant to the homicide-reduction pathway by medical marijuana laws. Note that I do not make precise predictions for states with lower OxyContin misuse rates. This is because the identification strategy relies on the *relative* variation in the proportion of individuals whose underlying health conditions are manageable by medical marijuana. If this

proportion is small, the net estimated effects of medical marijuana legalization on these outcomes post-reformulation will be challenging to predict. At best, the estimated impacts will be noisy and inconclusive[30].

Prediction 1. *The use of marijuana will likely increase post-reformulation and post-medical marijuana legalization among states with the highest level of pre-reformulation OxyContin misuse rates.*

Prediction 2. *The use of heroin will likely decrease post-reformulation and post-medical marijuana legalization among states with the highest level of pre-reformulation OxyContin misuse rates.*

Prediction 3. *Heroin overdose deaths will likely decrease post-reformulation and post-medical marijuana legalization among states with the highest level of pre-reformulation OxyContin misuse rates.*

Prediction 4. *Homicide victims will likely decrease post-reformulation and post-medical marijuana legalization among states with the highest level of pre-reformulation OxyContin misuse rates.*

4.2.3 Empirical Strategy

The goal of this section is to construct an empirical model to test the hypothesis that medical marijuana legalization reduces post-reformulation homicide victims. The identifying assumption from the last section is that this effect should manifest itself when the marginal population - patients whose underlying conditions are manageable by medical marijuana but used prescription opioids instead, pre-reformulation - is relatively large. Note that the focus is different from the theory linking pre-reformulation exposure to OxyContin to homicide victims. In this established theory, the marginal population *is* described by this exposure. In the development of current theory, the marginal population is associated with this exposure, but only because of the underlying physician behaviors. Therefore, the identification of this effect does not depend on this specific exposure but on whether the states in the study sample have similar pre-reformulation prescribing behaviors, which are *correlated* with the exposure.

There is a hidden bonus in testing the preventive channel of medical marijuana legalization against opioid use and overdoses in the particular event of OxyContin reformulation. People use marijuana for several reasons, for example, to cope with opioid withdrawal symptoms (Wiese and Wilson-Poe, 2018), to take advantage of the synergistic effect of combining cannabinoids and opioids (Nielsen et al., 2017), or to co-use with prescription opioids nonmedically (Rogers et al., 2019; McCabe et al., 2012). After the reformulation, likely, the incentive to use marijuana generally increases. However, the extent of whether this behavior results in a *substitution* toward marijuana still depends on the underlying health conditions of patients. If marijuana use increased for *all* states after the reformulation, but the reduction in heroin use, heroin overdose deaths, and homicide victims are *only* observed among states with the highest level of pre-reformulation OxyContin exposure and access to legal medical marijuana, the identifying assumption will be significantly strengthened.

[30]Not for marijuana use, as explained below.

Specifically, I test the following specifications:

$$\begin{aligned} y_{st} = \alpha &+ \sum_s \delta_s State_s + \sum_t \gamma_t Year_t + X_{st} \\ &+ \eta_1 MML_{st} + \eta_2 \mathbf{1}\{year \geq 2010\} \\ &+ \eta_3 MML_{st} \times \mathbf{1}\{year \geq 2010\} \\ &+ \epsilon_{st} \end{aligned}$$

In this specification, y is the outcome of interest of state s and year t. To test the predictions from the last section, I consider four different outcomes: past month marijuana use rates, the percentage reporting ever-used heroin, heroin overdose victims per 100,000 state residents, and homicide victims per 100,000 state residents. I chose past month's marijuana use instead of other commonly used measures, such as ever-used marijuana or usage in the past year, because it better reflects regular, need-based use other than using marijuana for recreational or situational purposes. The coverage of states includes all 50 states plus the District of Columbia. Study periods, on the other hand, depend on outcomes. For past month marijuana use rates and percentage reporting ever used heroin, I use 2002-2003, 2004-2005, 2006-2007, 2008-2009, 2010-2011, 2012-2013, 2014-2015, 2015-2016, 2016-2017, and 2017-2018 waves of National Survey of Drug Use and Health (NSDUH) data, which leaves me with ten survey periods. For heroin overdose rates and homicide rates, the data source is the National Vital Statistics System (NVSS), and the study period is from year 2000 to the year 2017.

Regarding independent variables, MML_{st} is an indicator variable that equals one if state s has fully legalized medical marijuana in year t. $\mathbf{1}\{year \geq 2010\}$ is an indicator variable that represents OxyContin Reformulation. X_{st} is a vector of controls[31]. $State_s$ and $Year_t$ set of dummies representing state and year fixed effects, respectively.

The estimates of interest are η_3, which describes how the effects of medical marijuana laws on various outcomes change post-reformulation. This is the only estimate to be reported, labeled "$oxy \times mml$."

For testing each outcome, I consider two configurations of states. One is the set of all 50 states plus District of Columbia. The second is the states with fixed medical marijuana legalization status after 2010. The reason to exclude states that legalized medical marijuana after 2010 is to mitigate the concern of policy endogeneity. Certain states might choose to legalize medical marijuana to respond to the rising overdose deaths after 2010[32]. The estimation results for these two configurations will be reported under panels A and B, respectively, in each table.

I divide each state configuration into subsamples based on quantiles of pre-reformulation OxyContin misuse rates. This is to utilize the identifying assumption: states with similar exposure to OxyContin pre-reformulation should have similar portions of individuals that could be incentivized

[31] More details to be added

[32] Note that states that legalized medical marijuana before 2010 could also respond to the opioid crisis. This configuration could not control for this type of policy endogeneity.

to choose marijuana post-reformulation if access to marijuana is legal. Furthermore, I consider both 4- and 5-quantile divisions. The purpose is to see whether the last section's predictions are accurate and robust to small changes in division granularity. For each division, I test the whole sample as well as each quantile of states according to their configuration. In addition, for the 4-quantile division, I also run specifications that include the bottom half and top half of states. Similarly, for the 5-quantile division, I run specifications that form the bottom 60% and top 40% of states. For each 5-quantile division with states that did not change their medical marijuana laws status after 2010, all states in its third quantile passed medical marijuana laws before 2010. Thus, the estimated η_3 will not be reported.

I use robust standard errors for most of these specifications instead of clustering them at the state level. This is because these specifications usually include a small number of states (<30). As Cameron, Gelbach, and Miller (2008) suggest, though ignoring the within-correlation of a cluster (state) usually underestimates the actual standard errors, clustering them only makes sense when the number of clusters is at least 30. Therefore, I only use cluster standard errors when all misuse rates quantiles of states are included.

It is important to remember that this identification strategy needs several limitations. First, I do not provide a way to *isolate* the intended effect; instead, I give an argument *where* this effect likely manifests itself. Second, since the analysis is at the state and year level and it relies on grouping states with similar levels of pre-reformulation OxyContin misuse rates, its statistical power to detect the intended effect is limited due to small sample sizes, and it might not be robust if certain states are included or excluded. Lastly, I assume that all relevant cross-state variations in institutional details, such as insurance coverage, are factored into physician behaviors that drive variations in pre-reformulation misuse rates. For institutional details that are not in this category but related to opioid use and homicide victims, such as substance use disorder treatment programs and policing efforts, I assume that their effects on these outcomes are similar between states groupings based on pre-reformulation OxyContin misuse rates. This assumption needs to be tested and might undermine the conclusions.

4.3 Estimation Results and Discussion

I present the 5-quantile division estimation results on the past month's marijuana use rates, the percentage of state residents that have ever used heroin, heroin overdose death rates, and homicide rates in tables 9, 11, 13, and 15, respectively. The top 20% states in the distribution of pre-reformulation OxyContin exposure see an increase in past month marijuana use, post-reformulation, and post-medical marijuana legalization, but so as the 4th quantile of states. The increase in this measure among the top 40% states is smaller than the rest of the states combined. This is not surprising and does not invalidate the prediction, as people use marijuana for several different reasons.

However, on heroin use and overdose death rates, the top 20% states in the distribution of pre-reformulation OxyContin exposure see consistent reduction at the 95% or higher confidence level.

In contrast, all other individual quantiles of states do not see this change. Lastly, among respective quantiles, only the top 20% states in the distribution of pre-reformulation OxyContin exposure see a reduction in homicide rates, and it is at the 99% confidence level. The exact predictions still hold if we look at estimates from the last column. The predictions also hold if we use a coarser, 4-quantile division scheme, though, for heroin use, the evidence is weak.

These findings fit well with the predictions from the last section: states on the higher end of pre-reformulation OxyContin exposure distribution see an increase in marijuana use, a decrease in heroin use and overdose deaths, and, consequently, a reduction in homicide victims. In addition, they are robust to the choice of subsample divisions. To add to this observation, the estimated η_3s from states with lower pre-reformulation exposure to OxyContin are often noisy, thus making them inconclusive.

In the appendix[33], I conduct two more tests: (1) replacing the trends by a set of demographic variables, including percentage male, percentage white, percentage people of Hispanic origin, and share of population age between 20 and 60, (2) including PDMP, recreational marijuana laws, naloxone laws, unemployment rate, and percentage of state population with a college degree. These changes, in general, do not invalidate the predictions.

The theory linking pre-reformulation OxyContin nonmedical use to homicide victims hinges on nonmedical users substituting for heroin post-reformulation. The current analysis shows that a decrease in this incentive can lead to a reduction in homicide victims, and its causal pathway is consistent with the assumption that medical marijuana is a substitute for illicit opioids due to its analgesic properties. For certain users that use prescription opioids nonmedically, such as OxyContin, easier access to medical marijuana would reduce their incentive to transition to illicit opioids, such as heroin, leading to lower usage and fewer overdose deaths post-reformulation. The decrease in demand for heroin or similar illicit opioids would lead to a lower level of illicit drug market activities and fewer homicide deaths. However, the identification of the effects along this causal pathway, or more precisely, *where* these effects are most likely to occur, requires an identification of *who* are most likely to be affected by the presence of a substitute to illicit opioids post-reformulation. The analysis of physicians' prescribing behaviors and pre-reformulation OxyContin misuse, though argumentative, provides a plausible way to mitigate this identification problem and thus makes detecting the intended effects possible.

On the policy-making front, this analysis provides empirical support that indirect intervention could be effective in reducing the unintended, adverse effects on public health and crime outcomes due to the implementation of restrictive, supply-side drug policies. This is in stark contrast to lessons learned from past direct intervention practices, which often led to escalating, contagious violence (Dell, 2015). On the other hand, caution must be used when considering its implications in the real world. As the analysis demonstrates, the effectiveness of medical marijuana legalization in preventing opioid overdoses depends on the size of the underlying population whose opioid use and overdose outcomes are expected to be affected by legal access to marijuana. It is recommended

[33] TBA

that a critical evaluation of this population must proceed with any legal or political procedure in legalizing medical marijuana, at least out of consideration for its efficacy.

5 Conclusion

This study examines the effect of one of the most significant supply-side drug control policies, the reformulation of OxyContin, on homicide, documenting the following results. First, difference-in-difference estimates show that this policy generates a positive gap in homicide rates between states with high pre-reformulation exposure to OxyContin and the rest, and this effect is primarily concentrated among young homicide victims, especially for those between 15 and 24. This empirical evidence primarily reflects an increase in illicit drug market activities due to greater demand for substitutes such as heroin. Furthermore, basic economic principles predicts that medical marijuana legalization could mitigate this adverse effect due to the analgesic potential of marijuana. However, determining the specific demographic affected by such a policy is challenging. Using pre-reformulation OxyContin misuse rates as a proxy for this population and comparing states with similar exposure, difference-in-difference estimates show that medical marijuana legalization led to post-reformulation increase in marijuana use for all states, but a decrease in heroin use, a decrease in heroin overdose deaths, and a decrease in homicide victims only for states with the highest pre-reformulation OxyContin exposure.

These results demonstrate how medical marijuana legalization reduces the incentives to substitute illicit opioids, thereby indirectly mitigating the adverse effects on public health and crime outcomes post-reformulation. However, a critical evaluation of *who* will be affected is recommended to accompany considerations involving legalizing medical marijuana. I call for research in this area.

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APPENDIX

A Data

Table 1: ICD-10 Codes in the Assault Category (NVSS)

ICD-10 codes	description
X85	Assault by drugs, medicaments and biological substances
X86	Assault by corrosive substance
X87	Assault by pesticides
X88	Assault by gases and vapours
X89	Assault by other specified chemicals and noxious substances
X90	Assault by unspecified chemical or noxious substance
X91	Assault by hanging, strangulation and suffocation
X92	Assault by drowning and submersion
X93	Assault by handgun discharge
X94	Assault by rifle, shotgun and larger firearm discharge
X95	Assault by other and unspecified firearm discharge
X96	Assault by explosive material
X97	Assault by smoke, fire and flames
X98	Assault by steam, hot vapours and hot objects
X99	Assault by sharp object
Y00	Assault by blunt object
Y01	Assault by pushing from high place
Y02	Assault by pushing or placing victim before moving object
Y03	Assault by crashing of motor vehicle
Y04	Assault by bodily force
Y05	Sexual assault by bodily force
Y87.1	Sequelae of assault

Notes: ICD-10 coding for death certificates-based homicide rates. These codes only include categories specified as the underlying cause of death for the deceased from the National Vital Statistics System's (NVSS) Multiple Causes of Death data.

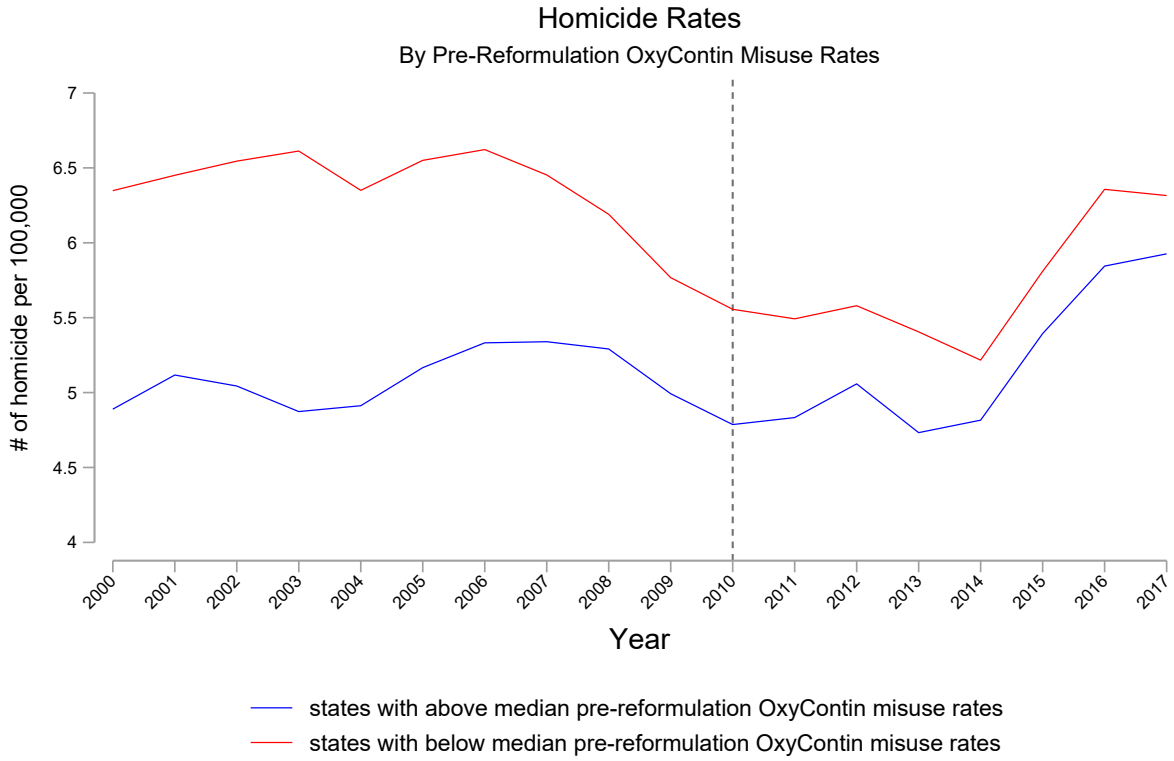
Table 2: Summary Statistics, 2000 - 2009

	All	High	Low
Outcomes (per 100,000)			
Homicide Rate	5.71	4.35	7.12
Heroin Overdose Rates	0.38	0.42	0.34
Misuse Prior to Reformulation (%)			
2004-2009 OxyContin Misuse Rate	0.67	0.86	0.48
2004-2009 Other pain relievers Misuse Rate	6.48	7.13	5.79
Unemployment Rate (%)	5.24	5.11	5.37
College Degrees (%)	18.17	17.51	18.85
Gender (%)			
male	49.23	49.41	49.04
Race/Ethnicity (%)			
Hispanic	9.12	8.21	10.06
White	81.97	88.15	75.55
Black	11.72	6.67	16.98
Age (%)			
0-19	27.66	27.53	27.80
20-39	28.72	28.54	28.91
40-59	20.87	21.30	20.42
60+	9.07	9.19	8.95
Population	5770436.27	3431583.07	8202843.60
# of States	51	26	25

Notes: This table presents the average of state-level homicide death rates, heroin overdose death rates, substance misuse rates, socio-economic characteristics, and demographics between the years 2000 and year 2009 (inclusive). I report these statistics over three groupings for states: all 50 states in the US plus the District of Columbia, states with pre-reformulation OxyContin misuse rates above the median (the "high" states), and states with pre-reformulation OxyContin misuse rates below the median (the "low" states). The homicide and heroin overdose rates are calculated by dividing the year-state counts of deaths obtained from the National Vital Statistics System's (NVSS) Multiple Cause of Death data by corresponding state population estimates. Misuse rates are obtained using data from the National Survey of Drug Use and Health (NSDUH) and averaging over the 2004-2005, 2006-2007, and 2008-2009 waves. Unemployment rates are from Federal Research Economic Data (FRED). The population's percentage of people with at least a college degree is calculated using American Community Service (ACS) data. Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

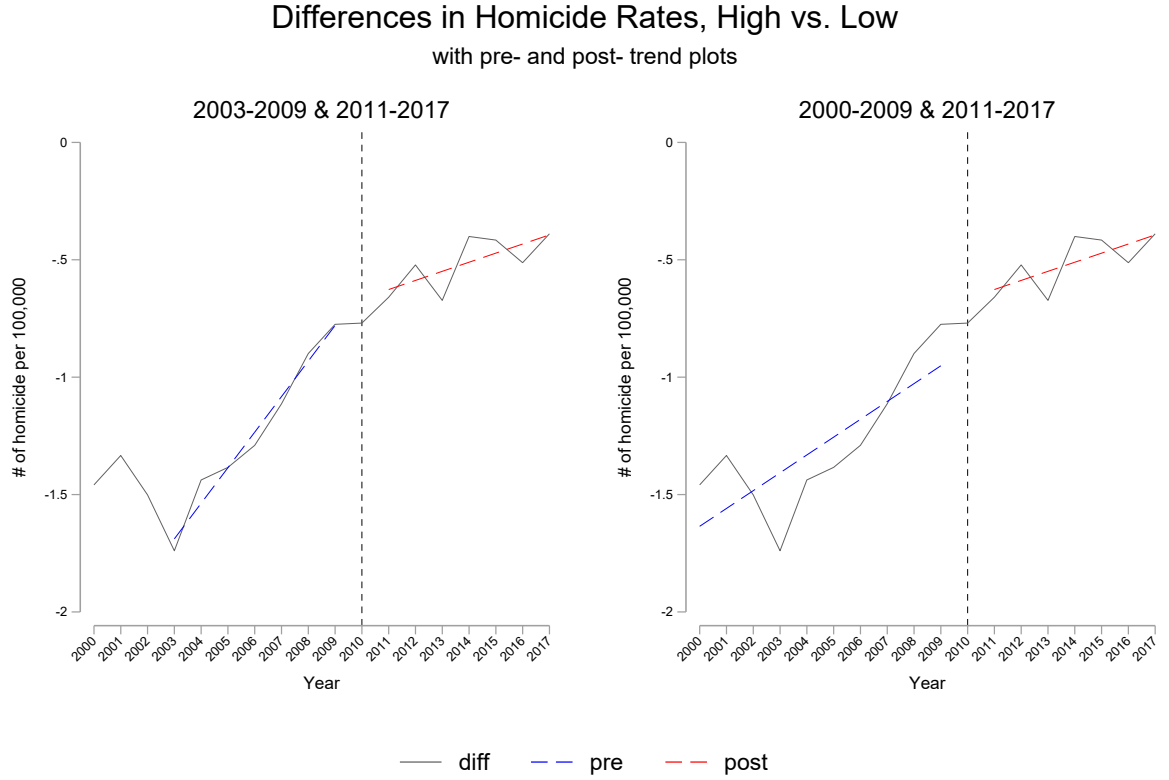
B Trends in Homicide Rates

Figure 1: Homicide Rates by Relative Level of Pre-Reformulation OxyContin Misuse, 2000 - 2017



Notes: This figure presents the average state-level homicide rates between the year 2000 and the year 2017 by the relative level of pre-reformulation OxyContin misuse rates, weighted by state-year population estimates from the SEER program. The "high" states are those states with 2004-2009 OxyContin misuse rates above the median, and the "low" states are the rest. The data source for OxyContin misuse rates is the National Survey of Drug Use and Health (NSDUH).

Figure 2: Differences in Homicide Rates Between States with Different Levels of Pre-Reformulation OxyContin Misuse, 2000 - 2017

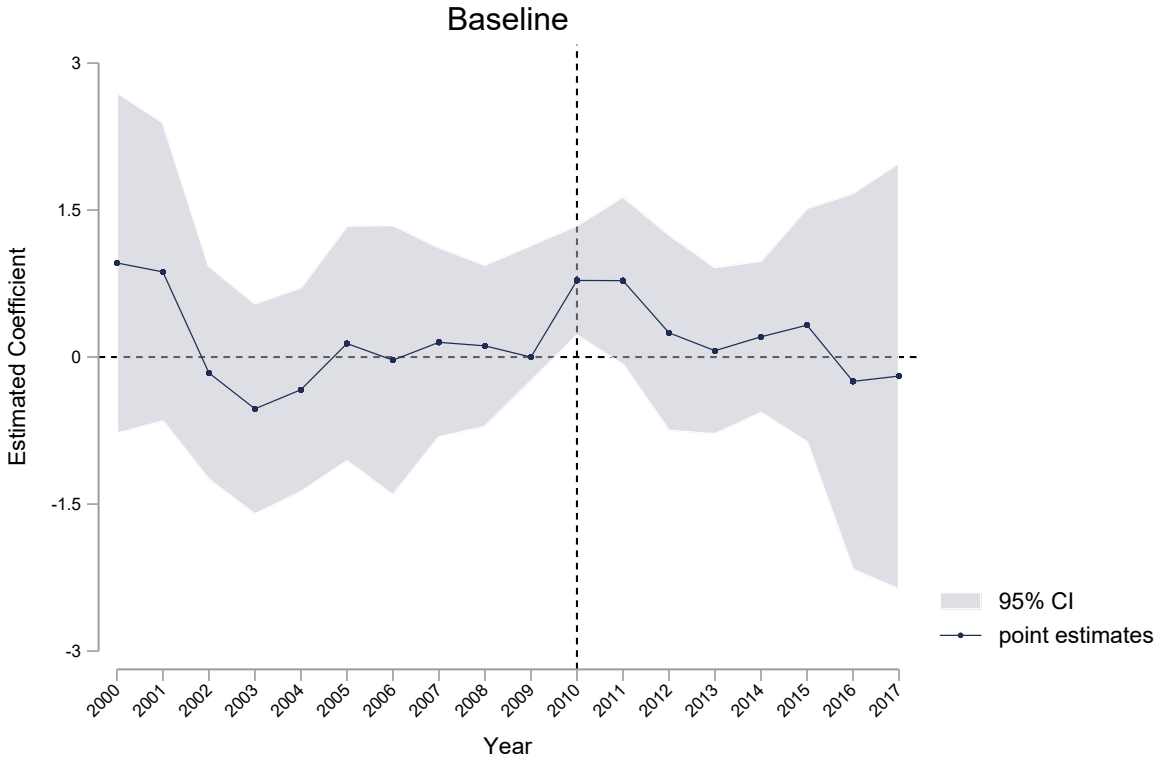


Notes: This figure presents the differences in average state-level homicide rates between the "high" and "low" states from the year 2000 and year 2017. The averages are weighted by state-year population estimates from the SEER program. The "high" states are those states with 2004-2009 OxyContin misuse rates above the median, and the "low" states are the rest. The data source for OxyContin misuse rates is the National Survey of Drug Use and Health (NSDUH).

C The Effects of OxyContin Reformulation on Homicides

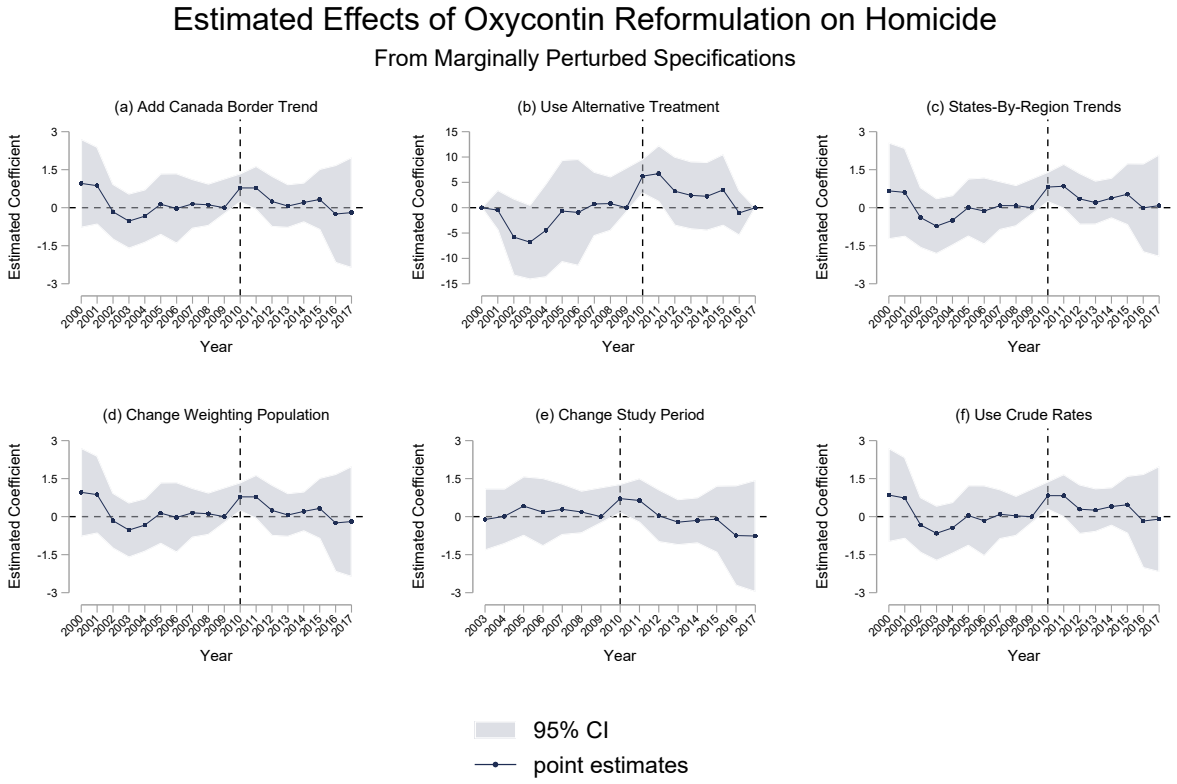
C.1 Event Study Plots

Figure 3: Estimated Coefficients of State-Level Pre-Reformulation OxyContin Misuse Rates on Homicide Rates, 2000 - 2017



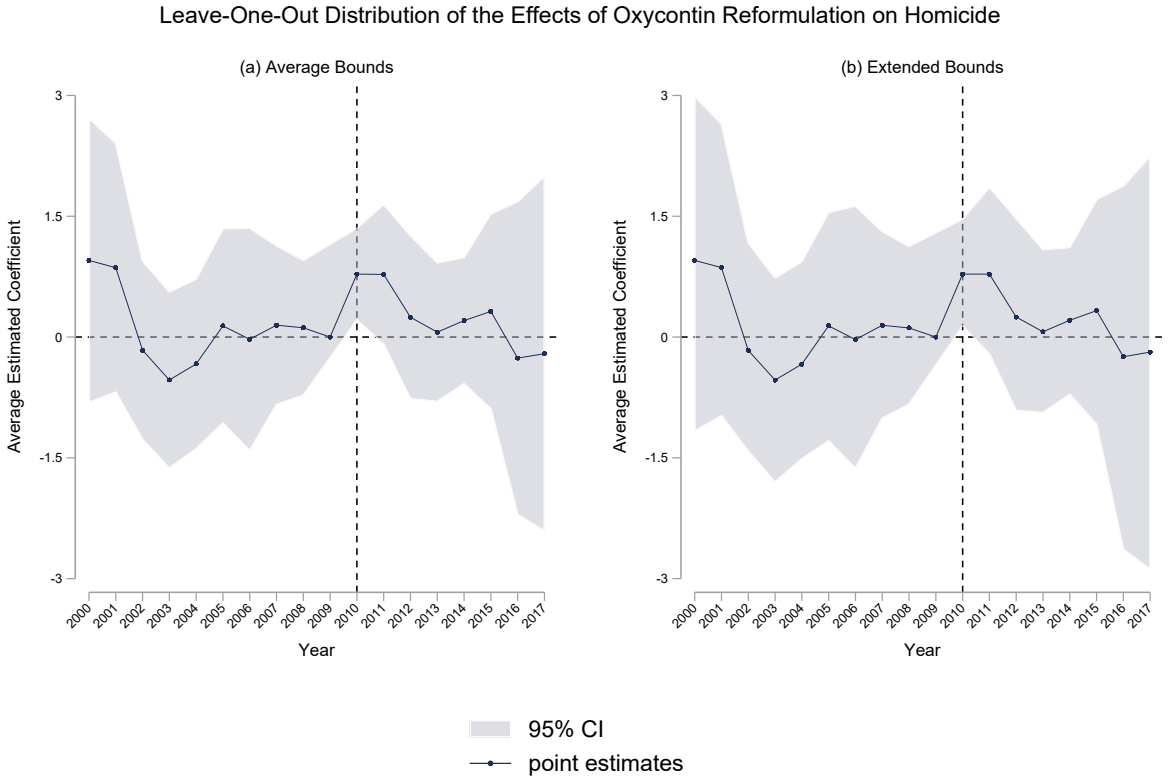
Notes: This figure presents the estimated coefficients of state-level pre-reformulation OxyContin misuse rates on homicide rates using the baseline event study specifications. The study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. For all regressions, standard errors are clustered at the state level. The confidence interval for all estimated coefficients is set at 95% level. The regression includes a full set of state and year dummies and a set of time trends for selected groups of states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates obtained from the National Survey of Drug Use and Health (NSDUH).

Figure 4: Robustness Checks for the Baseline Event Study Specification: Alternative Measures and Estimation Parameters



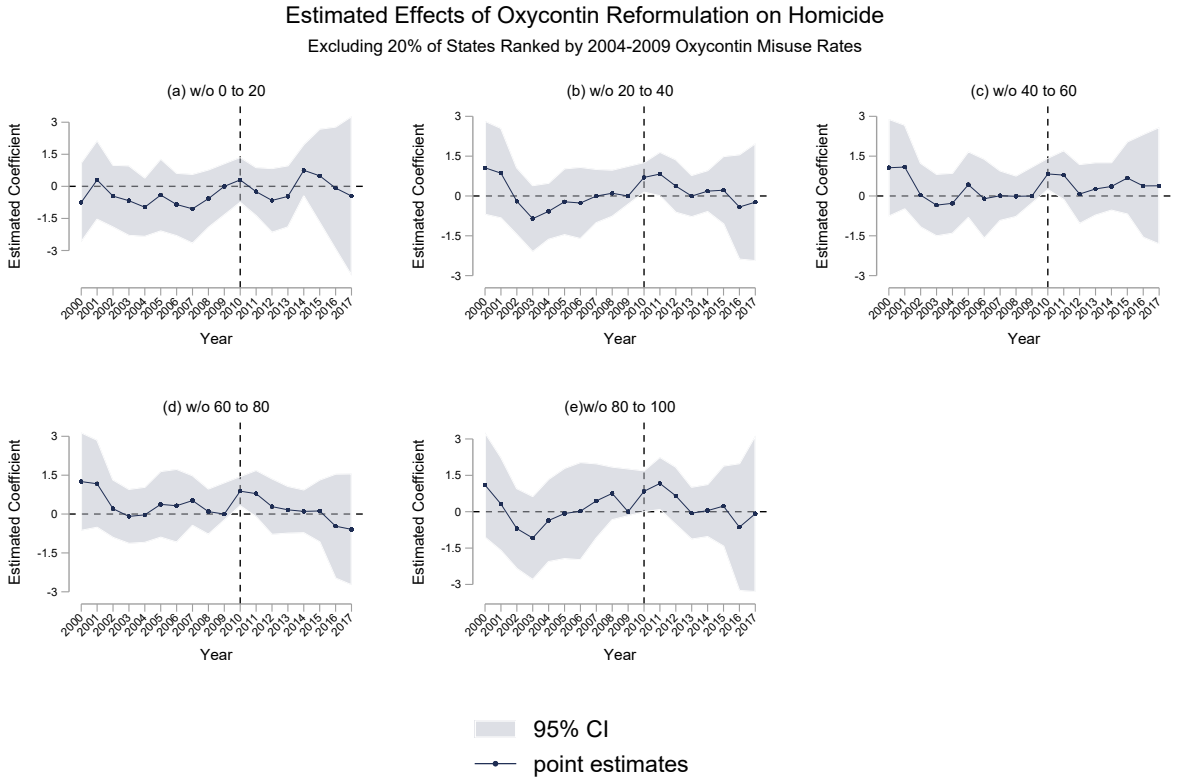
Notes: This figure presents the estimation results from six perturbations applied to the baseline event study specification, which are (a) adding pre-reformulation time trend for the states on the US-Canada border, (b) using ratios of pre-reformulation OxyContin and other pain relievers misuse rates as treatment intensity, (c) allowing states-by-region time trends for selected states considered sources and distribution hubs of heroin (d) using weighting population from Census, (e) changing starting period to 2003, and (f) using crude rates instead of age-adjusted rates. For all regressions, the study period is from the year 2000 to the year 2017, and standard errors are clustered at the state level. Data are for all 50 US states plus the District of Columbia. The confidence intervals for all estimated coefficients are set at 95% level. Unless otherwise noted, all regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates are from the National Survey of Drug Use and Health (NSDUH).

Figure 5: Robustness Checks for the Baseline Event Study Specification: Random Deletion of Observations From One State



Notes: This figure presents the estimation results of the baseline event study specification from randomly deleting one state. Panel (a) presents the mean point estimates of the regression coefficients and the mean values of the confidence interval limits. Panel (b) presents that the upper (lower) limit of a calculated coefficient corresponds to the upper (lower) limit of the 95% confidence interval within the distribution of the upper bounds. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia, minus one state randomly. Standard errors are clustered at the state level. Both regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates are from the National Survey of Drug Use and Health (NSDUH).

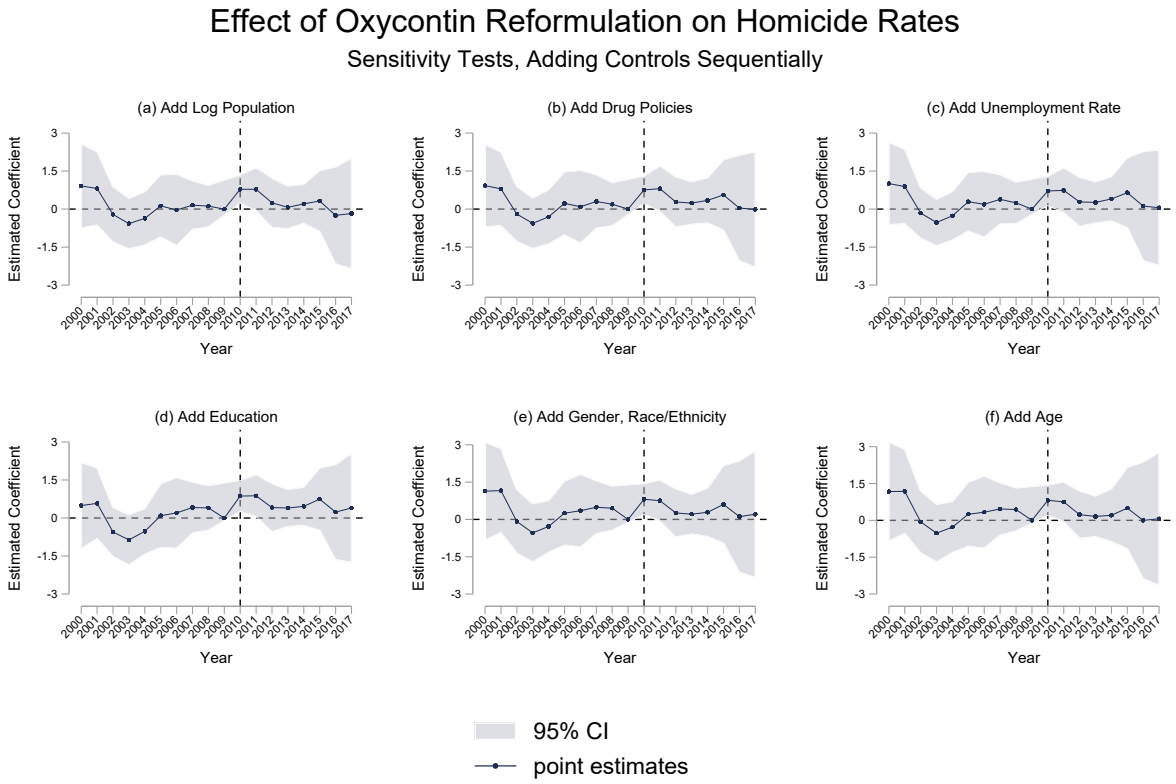
Figure 6: Robustness Checks for the Baseline Event Study Specification: Deletion of 20% of States Ranked by Pre-reformulation OxyContin Misuse Rates



Notes: This figure presents the estimation results of the baseline event study specification from removing states from one of the five quantiles in the distribution of pre-reformulation OxyContin misuse rates. Unless otherwise noticed, for all regressions, the study period is from the year 2000 to the year 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. The confidence interval for all estimated coefficients is set at 95% level.

The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates are from the National Survey of Drug Use and Health (NSDUH). All regressions include a full set of state (only for states with the designated range of pre-reformulation OxyContin misuse rates) and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.

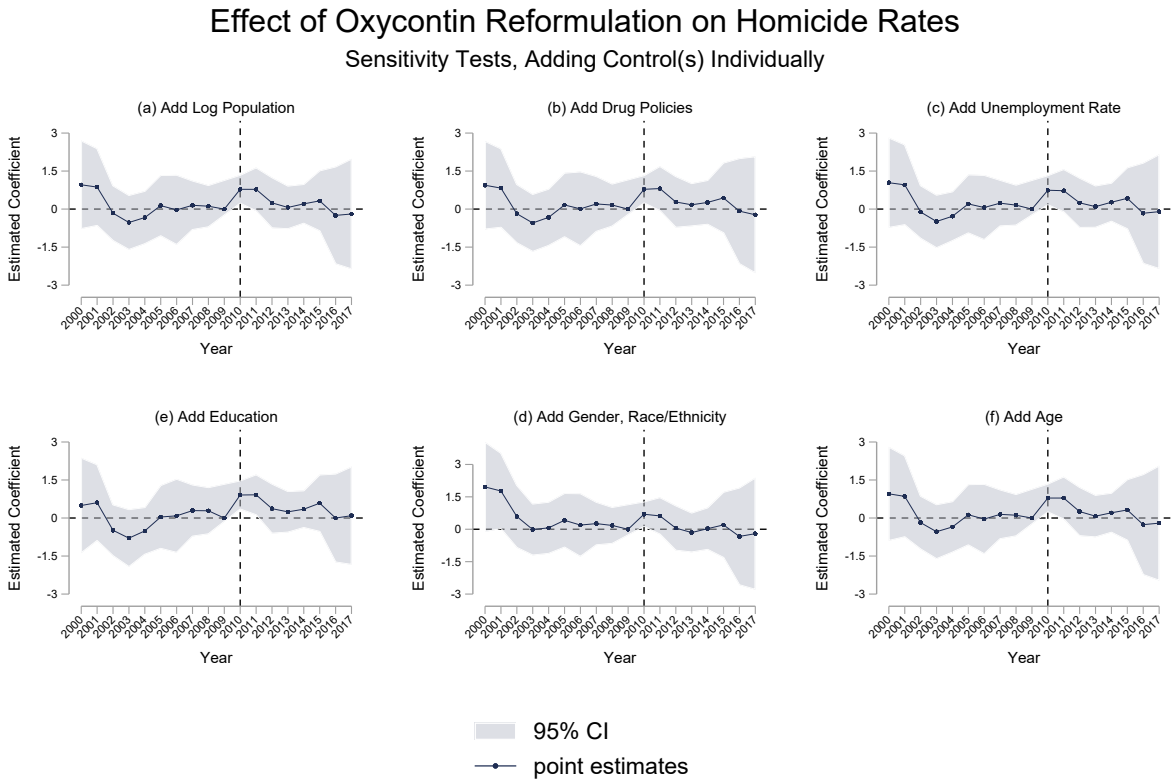
Figure 7: Sensitivity Test for the Baseline Event Study Specification: Adding Controls Sequentially



Notes: This figure presents the estimation results from adding state-level control variables to the baseline event study specification, which are (a) log state populations, (b) drug control policies, including naloxone laws, prescription drug monitoring programs (PDMP), medical marijuana laws, and recreational marijuana laws, (c) unemployment rate, (d) the percentage of state population with college degrees (e) male percentage, percentage white, percentage black, percentage Hispanic, and (f) share of population aged between 20 and 40. The addition of these variables is accumulative. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. The confidence interval for all estimated coefficients is set at 95% level. All regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.

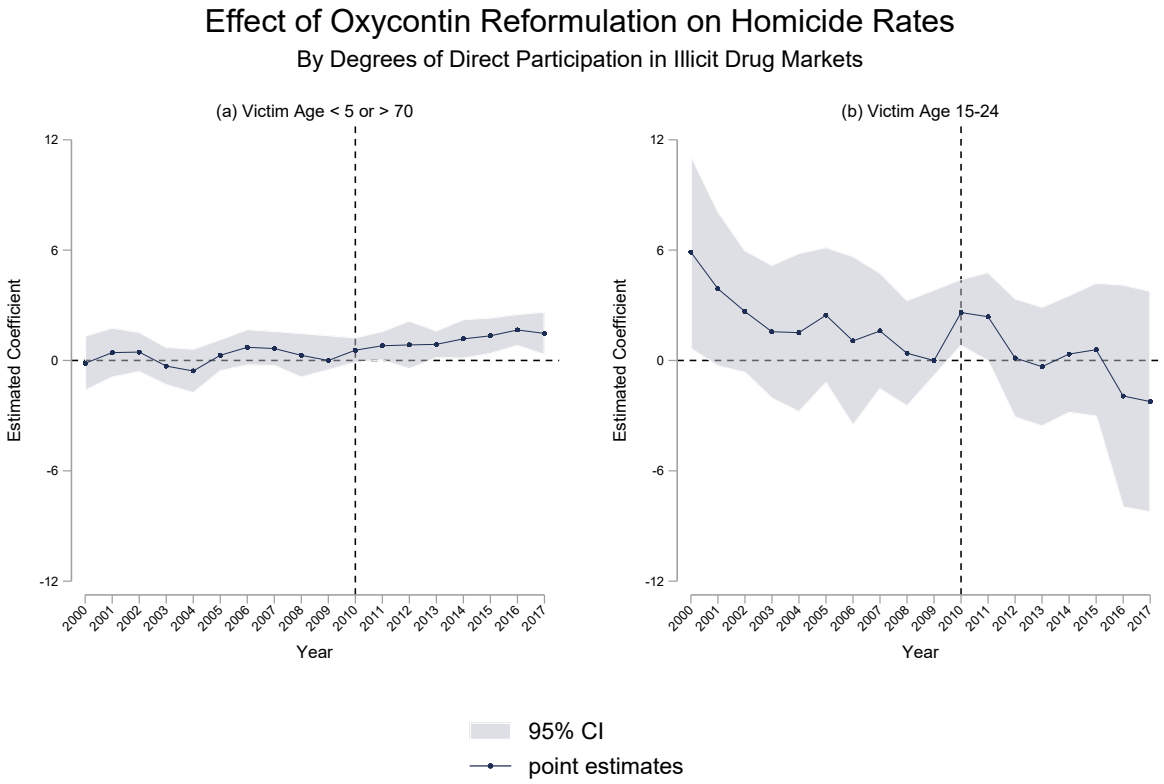
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program. Unemployment rates are from Federal Research Economic Data (FRED). The population's percentage of people with at least a college degree is calculated using American Community Service (ACS) data. Drug policy data are from Rand's OPTIC-Vetted Policy Data Sets.

Figure 8: Sensitivity Test for the Baseline Event Study Specification: Adding Controls Individually



Notes: This figure presents the estimation results from adding state-level control variables to the baseline event study specification, which are (a) log state populations, (b) drug control policies, including naloxone laws, prescription drug monitoring programs (PDMP), medical marijuana laws, and recreational marijuana laws, (c) unemployment rate, (d) the percentage of state population with college degrees (e) male percentage, percentage white, percentage black, percentage Hispanic, and (f) share of population aged between 20 and 40. Control variables or sets of control variables are added individually. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. The confidence interval for all estimated coefficients is set at 95% level. All regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program. Unemployment rates are from Federal Research Economic Data (FRED). The population's percentage of people with at least a college degree is calculated using American Community Service (ACS) data. Drug policy data are from Rand's OPTIC-Vetted Policy Data Sets.

Figure 9: Falsification Test for the Baseline Event Study Specification



Notes: This figure presents the estimation results from estimating OxyContin reformulation's effect on the homicide rates of victims under five years old or over 70 years old (panel (a)) and victims between 15 and 24 (panel (b)) using the event study specification. The study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. The confidence interval for all estimated coefficients is set at 95% level. Both regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates are from the National Survey of Drug Use and Health (NSDUH).

C.2 Regression Tables

Table 3: Sensitivity Test for the Fully Parameterized Specification: Demographic Controls

	Dependent Var.: Homicide per 100,000							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2004-2009 OxyContin Misuse Rates								
pretrend	-0.0609	-0.0533	-0.174	-0.0595	-0.158	-0.162	-0.0280	-0.0847
	(0.094)	(0.091)	(0.108)	(0.102)	(0.115)	(0.120)	(0.108)	(0.127)
posttrend	-0.0770	-0.0834	0.0476	-0.0815	0.0246	0.00929	-0.0751	0.00975
	(0.212)	(0.210)	(0.250)	(0.227)	(0.265)	(0.274)	(0.213)	(0.258)
shift	0.945***	0.925***	0.971***	0.958***	0.958***	0.970***	0.946***	0.950***
	(0.328)	(0.341)	(0.313)	(0.342)	(0.320)	(0.328)	(0.329)	(0.331)
2004-2009 Other Pain relievers Misuse Rates								
pretrend	0.0179	0.0208	0.0356*	0.0145	0.0350*	0.0326*	0.0198	0.0321
	(0.017)	(0.017)	(0.021)	(0.018)	(0.021)	(0.019)	(0.019)	(0.023)
posttrend	0.0197	0.0197	0.00307	0.0274	0.00565	0.0139	0.0198	0.0123
	(0.038)	(0.038)	(0.044)	(0.043)	(0.044)	(0.048)	(0.038)	(0.047)
shift	-0.152	-0.157	-0.156	-0.147	-0.157	-0.150	-0.152	-0.162
	(0.115)	(0.116)	(0.112)	(0.118)	(0.113)	(0.113)	(0.115)	(0.118)
pop.	No	Yes	No	Yes	Yes	Yes	No	Yes
gender/race/ethnicity	No	No	Yes	No	Yes	Yes	No	Yes
age	No	No	No	No	No	Yes	No	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) through (5) of this table present the estimation results from the sensitivity tests of the baseline fully parameterized specification with state-level demographic controls, including (1) log state populations, (2) male percentage, percentage white, percentage black, percentage Hispanic, and (3) share of the population aged between 20 and 40. For all regressions, the study period is from 2000 to 2017. Column (7) presents the estimation results of baseline fully parameterized specification with states-by-region time trends for states considered sources and distribution hubs of heroin. Column (8) estimated the specification in column (7) with a full set of demographic controls. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. Unless otherwise noted, all regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 4: Sensitivity Test for the Fully Parameterized Specification: Drug Policy and Economic Variables

	Dependent Var.: Homicide per 100,000					
	(1)	(2)	(3)	(4)	(5)	(6)
2004-2009 OxyContin Misuse Rates						
pretrend	-0.162	-0.145	-0.172	-0.162	-0.158	-0.0780
	(0.120)	(0.118)	(0.123)	(0.120)	(0.116)	(0.108)
posttrend	0.0105	0.0130	-0.0200	0.00916	0.0298	-0.0438
	(0.274)	(0.282)	(0.262)	(0.290)	(0.287)	(0.241)
shift	0.954***	0.899**	1.027***	0.970**	0.884***	0.818**
	(0.325)	(0.383)	(0.365)	(0.369)	(0.300)	(0.390)
2004-2009 Other Pain relievers Misuse Rates						
pretrend	0.0330*	0.0319*	0.0335*	0.0326*	0.0344*	0.0180
	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)	(0.017)
posttrend	0.0133	0.00524	0.0270	0.0140	0.00650	0.0207
	(0.047)	(0.047)	(0.048)	(0.049)	(0.051)	(0.043)
shift	-0.149	-0.128	-0.159	-0.150	-0.144	-0.165
	(0.113)	(0.130)	(0.116)	(0.117)	(0.112)	(0.118)
pop.	Yes	Yes	Yes	Yes	Yes	Yes
gender/race/ethnicity	Yes	Yes	Yes	Yes	Yes	Yes
age	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table displays results from sensitivity analysis incorporating drug policy and economic factors. It examines the effects of Prescription Drug Monitoring Program (PDMP), Medical Marijuana Laws (MML), Recreational Marijuana Laws (RML), Naloxone Laws (NAL), state unemployment rates, and the proportion of the population with a college degree across columns (1) to (6). For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. All regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program. Lastly, All specifications are estimated with a full set of demographic controls, including (1) log state populations, (2) male percentage, percentage white, percentage black, percentage Hispanic, and (3) share of the population aged between 20 and 40.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 5: Robustness Checks for the Fully Parameterized Specification: Perturbing Specification Setup and Excluding Selected States

	Dependent Var.: Homicide per 100,000							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2004-2009 OxyContin Misuse Rates								
pretrend	-0.162	-0.283***	-0.167	-0.162***	-0.162	-0.0595	-0.0838	
	(0.120)	(0.097)	(0.124)	(0.063)	(0.120)	(0.111)	(0.134)	
posttrend	0.00930	-0.0240	0.0104	0.00929	0.00929	-0.258	0.0178	
	(0.274)	(0.208)	(0.272)	(0.137)	(0.274)	(0.289)	(0.269)	
shift	0.969***	1.225***	1.084***	0.970**	0.970***	0.681*	0.850**	
	(0.328)	(0.447)	(0.335)	(0.401)	(0.328)	(0.402)	(0.376)	
2004-2009 Other Pain relievers Misuse Rates								
pretrend	0.0326*	0.0530***	0.0327*	0.0326***	0.0326*	0.0197	0.0335	
	(0.019)	(0.017)	(0.019)	(0.012)	(0.019)	(0.017)	(0.020)	
posttrend	0.0141	0.0112	0.0140	0.0139	0.0139	0.0528	-0.00919	
	(0.048)	(0.039)	(0.046)	(0.024)	(0.048)	(0.049)	(0.043)	
shift	-0.150	-0.0783	-0.157	-0.150*	-0.150	-0.114	-0.117	
	(0.113)	(0.168)	(0.114)	(0.085)	(0.113)	(0.119)	(0.116)	
Ratios								
pretrend								-0.867
								(0.796)
posttrend								-0.272
								(1.868)
shift								6.282***
								(2.253)
pop.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
gender/race/ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimation results from the robustness checks of the fully parameterized specification. In column (8), the ratio of pre-reformulation OxyContin misuse and other pain relievers misuse is used as the independent variable of interest. In column (1), an alternative weighting population is used. In column (2), no weighting population is used. In column (3), crude homicide rates instead of age-adjusted rates are used as the outcome. In column (4), robust standard error is used. In column (5), Hawaii and Alaska are excluded from the estimation. In column (6), California is excluded. In column (7), New York and Texas are excluded. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. Unless otherwise specified, all regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program. Lastly, all specifications are estimated with a full set of demographic controls, including (1) log state population, (2) male percentage, percentage white, percentage black, percentage Hispanic, and (3) share of the population aged between 20 and 40.

The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 6: Robustness Checks for the Fully Parameterized Specification: Deletion of States Ranked by Pre-reformulation OxyContin Misuse Rates

	Dependent Var.: Homicide per 100,000						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2004-2009 OxyContin Misuse Rates							
pretrend	-0.0147	-0.199	-0.221*	-0.215	-0.0716	0.404*	-0.405***
	(0.118)	(0.134)	(0.128)	(0.135)	(0.157)	(0.216)	(0.123)
posttrend	0.0441	0.00430	0.203	-0.0572	-0.163	-0.216	0.412
	(0.308)	(0.292)	(0.307)	(0.278)	(0.381)	(0.573)	(0.351)
shift	0.584	1.183***	0.974**	1.024***	0.787*	-0.477	1.466***
	(0.811)	(0.232)	(0.397)	(0.324)	(0.456)	(1.509)	(0.227)
2004-2009 Other Pain relievers Misuse Rates							
pretrend	0.00726	0.0370	0.0444*	0.0418*	0.0375	0.00714	0.0970***
	(0.015)	(0.022)	(0.022)	(0.022)	(0.023)	(0.018)	(0.027)
posttrend	0.0447	0.0102	-0.0180	0.0236	-0.00268	0.0256	-0.0641
	(0.046)	(0.047)	(0.061)	(0.051)	(0.055)	(0.058)	(0.076)
shift	-0.0997	-0.216**	-0.120	-0.162	-0.152	-0.0904	-0.258**
	(0.133)	(0.085)	(0.136)	(0.122)	(0.133)	(0.157)	(0.109)
pop.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
gender/race/ethnicity	Yes	Yes	Yes	Yes	Yes	Yes	Yes
age	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimation results from removing certain quantiles (out of five) of states in the pre-reformulation OxyContin misuse rates distribution. Columns (1) through (5) correspond to the estimation results from removing the states that fall into the bottom fifth, 20th-40th percentiles, 40th-60th percentiles, 60th-80th percentiles, and the top fifth of the pre-reformulation OxyContin misuse rates distribution, respectively. In column (6), the estimation removes the top and bottom fifth of states. In column (7), the estimation removes all but the top and bottom fifth of states. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. All regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program. Lastly, all specifications are estimated with a full set of demographic controls, including (1) log state populations, (2) male percentage, percentage white, percentage black, percentage Hispanic, and (3) share of the population aged between 20 and 40.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

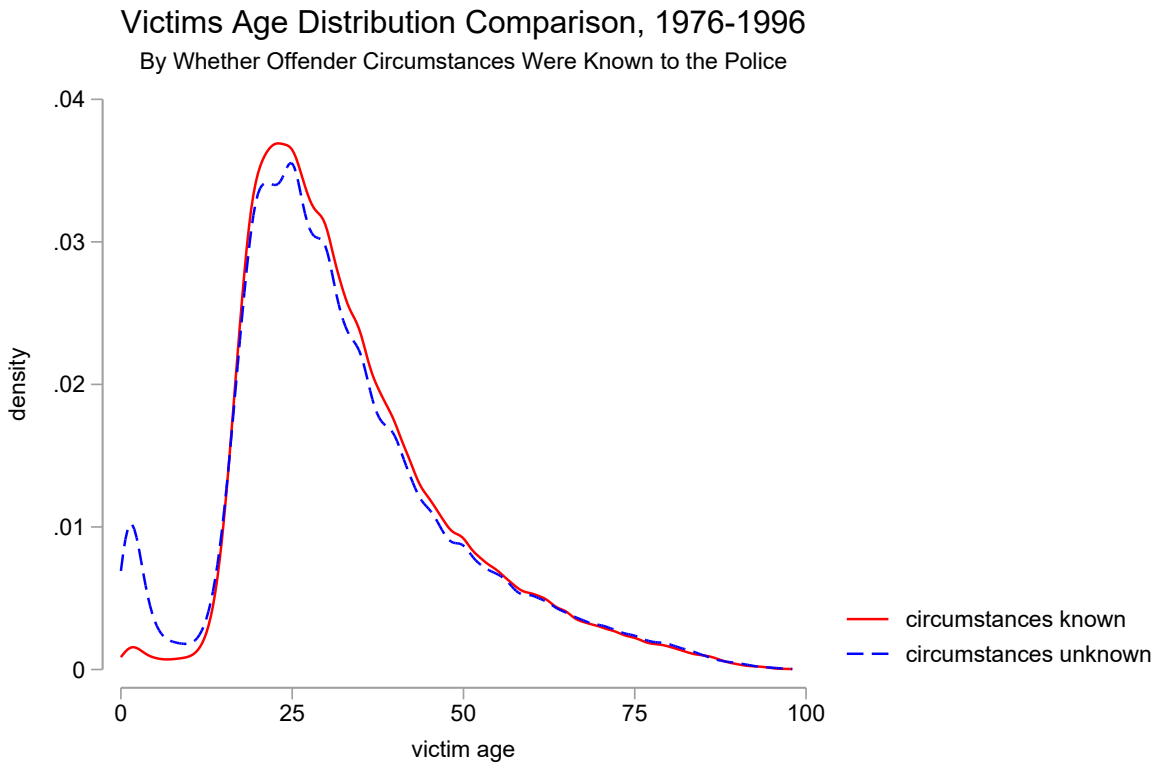
Table 7: Falsification Test of the Fully Parameterized Specification

	Dependent Var.: Homicide Rates for Victims Younger Than 5 or Older Than 70							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2004-2009 OxyContin Misuse Rates								
pretrend	0.0311	0.0336	0.0350	0.0315	0.0329	0.0304	0.0136	0.0301
	(0.061)	(0.064)	(0.055)	(0.061)	(0.059)	(0.060)	(0.054)	(0.048)
posttrend	0.117	0.164	0.0910	0.0822	0.146	0.124	0.115	0.0667
	(0.096)	(0.114)	(0.071)	(0.081)	(0.095)	(0.097)	(0.096)	(0.076)
shift	0.223	0.251	0.246	0.341	0.253	0.328	0.221	0.264
	(0.261)	(0.292)	(0.290)	(0.265)	(0.315)	(0.320)	(0.261)	(0.285)
2004-2009 Other Pain relievers Misuse Rates								
pretrend	0.00388	0.00492	0.00386	0.00247	0.00593	0.00434	-0.00188	-0.00560
	(0.011)	(0.013)	(0.011)	(0.011)	(0.012)	(0.013)	(0.012)	(0.011)
posttrend	-0.0150	-0.0264	-0.00887	-0.00594	-0.0213	-0.0137	-0.0152	-0.00560
	(0.021)	(0.023)	(0.022)	(0.021)	(0.023)	(0.024)	(0.021)	(0.023)
shift	0.0226	0.0116	0.0159	0.0124	0.00868	0.00412	0.0225	0.0121
	(0.081)	(0.076)	(0.078)	(0.079)	(0.074)	(0.074)	(0.081)	(0.077)
demographics	No	Yes	No	No	Yes	Yes	No	Yes
drug policies	No	No	Yes	No	Yes	Yes	No	Yes
economic conditions	No	No	No	Yes	No	Yes	No	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimation results from estimating OxyContin reformulation's effect on the homicide rates of victims under five years old or over 70 years old using the baseline fully parameterized specification with state-age group level demographic controls, including (1) log state population, (2) male percentage, percentage white, percentage black, percentage Hispanic, and (3) share of the population aged between 20 and 40. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered at the state level. All regressions include a full set of state and year dummies and a set of trends for selected states considered sources and distribution hubs of heroin. Additionally, each regression is weighted by state-year population estimates from the SEER program.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

D Heterogeneous Effects by Age Groups

Figure 10: Victims Age Distribution: Known vs. Unknown Offender Circumstances



Notes: This figure presents the kernel density estimations of victim age distribution from the year 1976 to the year 2021 by whether the offender circumstances were known. The data source is the Supplemental Homicide Reports (SHR).

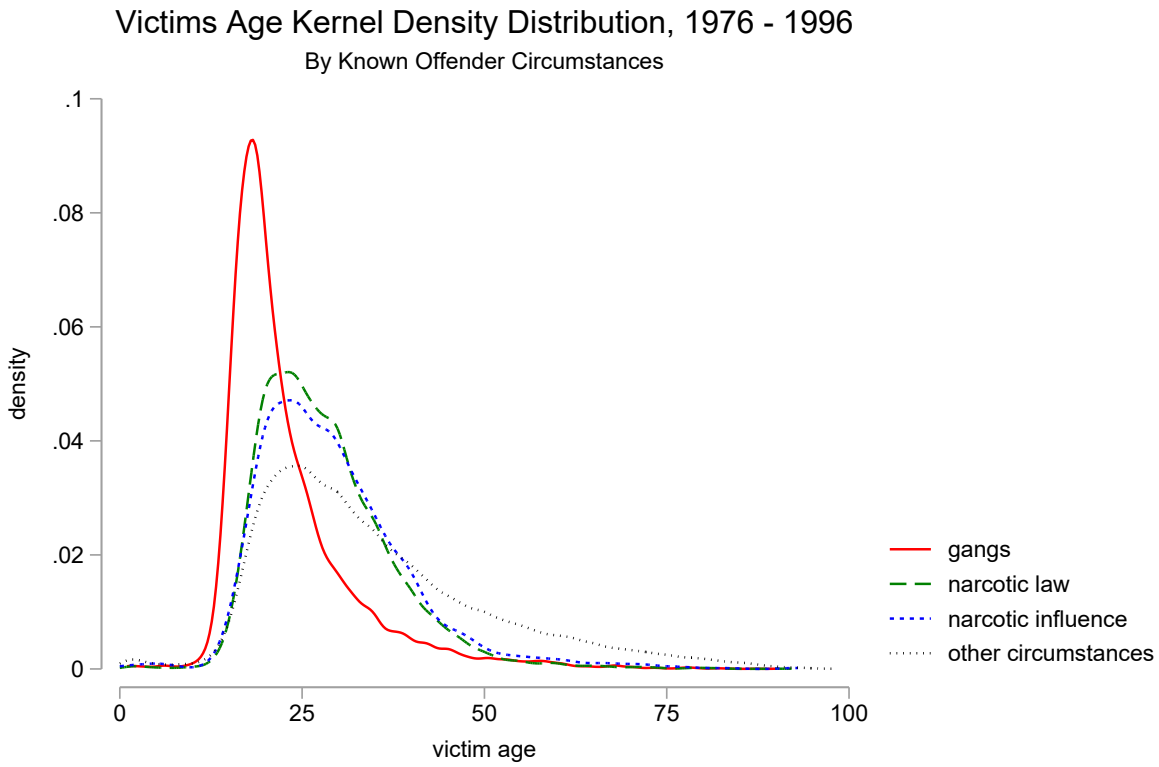
Table 8: Heterogeneous Effects of OxyContin Reformulation on Homicide Across Age Groups

	Dependent Var.: Homicide per 100,000					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 5-14						
$oxy \times \mathbf{1}\{5 \leq age \leq 14\} \times \mathbf{1}\{year \geq 2010\}$	-3.081**	-2.969**	-3.155**	-3.309**	-3.043**	-3.265**
	(1.270)	(1.302)	(1.255)	(1.357)	(1.286)	(1.351)
Panel B: 15-24						
$oxy \times \mathbf{1}\{15 \leq age \leq 24\} \times \mathbf{1}\{year \geq 2010\}$	8.152**	8.202**	8.441**	8.179**	8.495**	8.526**
	(3.329)	(3.362)	(3.375)	(3.309)	(3.409)	(3.381)
Panel C: 25-34						
$oxy \times \mathbf{1}\{25 \leq age \leq 34\} \times \mathbf{1}\{year \geq 2010\}$	3.062**	3.290**	3.059**	2.963*	3.288**	3.212**
	(1.501)	(1.554)	(1.512)	(1.568)	(1.567)	(1.628)
Panel D: 35-44						
$oxy \times \mathbf{1}\{35 \leq age \leq 44\} \times \mathbf{1}\{year \geq 2010\}$	-1.401	-1.408	-1.360	-1.406	-1.365	-1.341
	(1.380)	(1.351)	(1.397)	(1.353)	(1.370)	(1.344)
Panel E: 45-54						
$oxy \times \mathbf{1}\{45 \leq age \leq 54\} \times \mathbf{1}\{year \geq 2010\}$	-2.131*	-2.285*	-2.188*	-2.053*	-2.342**	-2.230**
	(1.206)	(1.174)	(1.195)	(1.163)	(1.162)	(1.121)
Panel F: 55-64						
$oxy \times \mathbf{1}\{55 \leq age \leq 64\} \times \mathbf{1}\{year \geq 2010\}$	-1.724	-2.164**	-1.878*	-1.592	-2.308**	-2.161**
	(1.144)	(1.099)	(1.127)	(1.115)	(1.076)	(1.079)
log population	No	Yes	No	No	Yes	Yes
gender	No	No	Yes	No	Yes	Yes
race/ethnicity	No	No	No	Yes	No	Yes
age FE	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents evidence that the effect of OxyContin reformulation on homicide differs between a chosen victim age group and all the other victim age groups. Six victim age groups are considered: 5-14, 15-24, 25-34, 35-44, 45-54, 55-64. Within each panel, columns (1) through (6) correspond to the estimation results with various combinations of demographic variables at the state, year, and age group level, including (1) log state populations, (2) male percentage, percentage white, percentage black, percentage Hispanic, and (3) share of the population aged between 20 and 40. For all regressions, the study period is from 2000 to 2017. Data are for all 50 US states plus the District of Columbia. Standard errors are clustered by state and victim age group. All regressions include state, year, and victim age group dummies and are weighted by state-year population estimates from the SEER program.

The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). 2004-2009 state-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Figure 11: Victims Age Distribution: Offender Circumstances Known

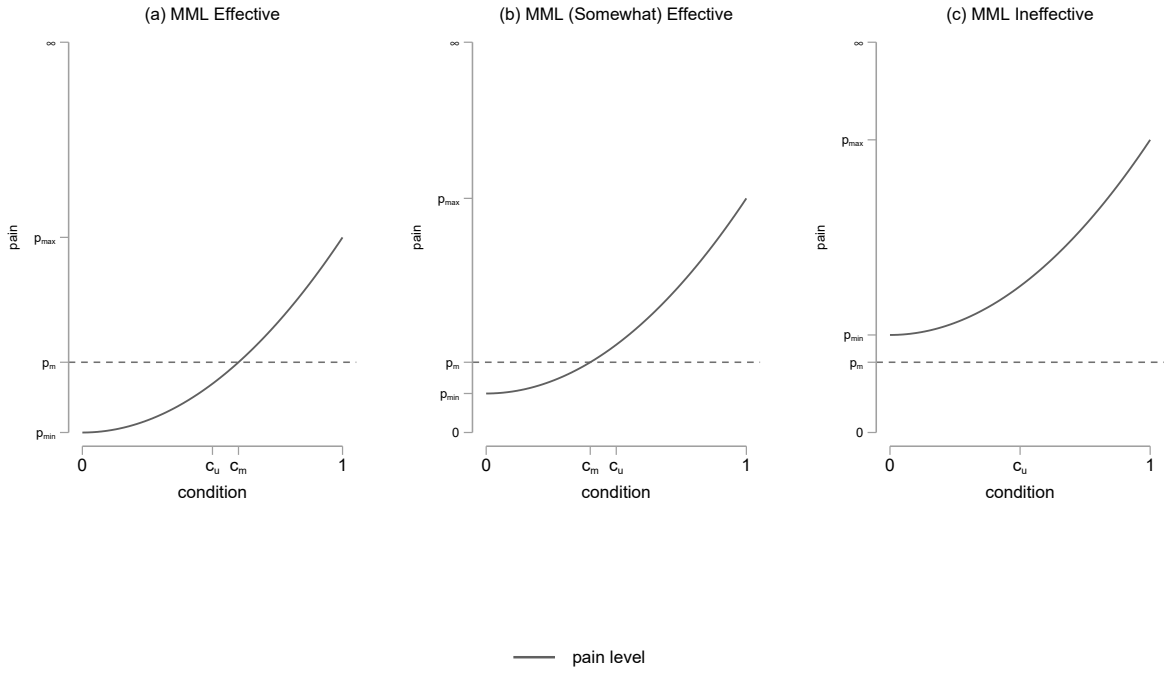


Notes: This figure presents the kernel density estimations of victim age distribution due to gang activities, violation of narcotic laws, influence of illicit drugs, and all other circumstances. The study period is from 1976 to 2021. The data source is the Supplemental Homicide Reports (SHR).

E The Homicide-Reducing Hypothesis of Medical Marijuana Laws

E.1 Illustration

Figure 12: Effectiveness of Medical Marijuana Laws in Substance Use Disorder Prevention: Position of the Max Level Manageable Pain



Notes: This figure illustrates how the position of the max level manageable pain by medical marijuana, p_m, relative to patient pain distribution, affects the size of possible switchers, c_m, from prescription opioids to medical marijuana if legal access to medical marijuana is provided. In all three panels of this figure, c_u is the fraction of untreated patients.

E.2 Main Regression Tables

Table 9: Effects of Medical Marijuana Laws on Post-Reformulation Marijuana Use by Pre-Reformulation OxyContin Misuse Rates, 5-Quantiles

	Dependent Var.: Pct. Used Marijuana Past Month							
	all	bottom 20%	20%-40%	40%-60%	60%-80%	top 20%	bottom 60%	top 40%
Panel A: all states								
mml × *post*	2.248***	-0.479	1.341**	3.876***	2.066**	1.592***	2.419***	2.078***
	(0.433)	(0.764)	(0.623)	(0.843)	(0.855)	(0.597)	(0.386)	(0.440)
Panel B: states with fixed mml status after 2010								
mml × *post*	2.272***	-0.206	0.872	-	2.135**	1.726***	2.454***	2.079***
	(0.624)	(0.772)	(1.448)	-	(0.964)	(0.619)	(0.394)	(0.445)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No	No
# of obs., panel A	510	110	100	100	100	100	210	200
# of obs., panel B	360	80	70	-	90	90	180	190

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimated effects of medical marijuana laws on post-reformulation marijuana use by five different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation marijuana use is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017, but it includes only ten specific years (2002, 2004, 2006, 2008, 2010, 2012, 2014, 2015, 2016, 2017) as the National Survey of Drug Use and Health (NSDUH) data is biennial. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.

The data source for the past month marijuana use rates, state-level OxyContin misuse rates, and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 10: Effects of Medical Marijuana Laws on Post-Reformulation Marijuana Use by Pre-Reformulation OxyContin Misuse Rates, 4-Quantiles

	Dependent Var.: Pct. Used Marijuana Past Month						
	all	q1	q2	q3	q4	q1 & q2	q3 & q4
Panel A: all states							
mml × *post*	2.248***	-0.184	1.537**	4.105***	1.779***	1.921***	2.829***
	(0.433)	(0.705)	(0.630)	(0.705)	(0.544)	(0.405)	(0.495)
Panel B: states with fixed mml status after 2010							
mml × *post*	2.272***	0.274	2.744*	3.954***	2.089***	1.957***	2.135**
	(0.624)	(0.755)	(1.587)	(0.808)	(0.551)	(0.426)	(0.964)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No
# of obs., panel A	510	130	130	130	120	260	250
# of obs., panel B	360	100	70	80	110	230	200

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimated effects of medical marijuana laws on post-reformulation marijuana use by four different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation marijuana use is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017, but it includes only ten specific years (2002, 2004, 2006, 2008, 2010, 2012, 2014, 2015, 2016, 2017) as the National Survey of Drug Use and Health (NSDUH) data is biennial. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.

The data source for the past month marijuana use rates, state-level OxyContin misuse rates, and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 11: Effects of Medical Marijuana Laws on Post-Reformulation Heroin Use by Pre-Reformulation OxyContin Misuse Rates, 5-Quantiles

	Dependent Var.: Pct. Ever Used Heroin							
	all	bottom 20%	20%-40%	40%-60%	60%-80%	top 20%	bottom 60%	top 40%
Panel A: all states								
mml × *post*	0.0801	0.458*	0.0417	0.364	0.0850	-0.590*	0.221	-0.198
	(0.116)	(0.236)	(0.399)	(0.544)	(0.472)	(0.311)	(0.174)	(0.240)
Panel B: states with fixed mml status after 2010								
mml × *post*	-0.0204	0.357	0.791*	-	0.160	-0.616**	0.228	-0.203
	(0.142)	(0.299)	(0.404)	-	(0.492)	(0.289)	(0.178)	(0.243)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No	No
# of obs., panel A	510	110	100	100	100	100	210	200
# of obs., panel B	360	80	70	-	90	90	180	190

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimated effects of medical marijuana laws on post-reformulation heroin use by five different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation heroin use is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017, but it includes only ten specific years (2002, 2004, 2006, 2008, 2010, 2012, 2014, 2015, 2016, 2017) as the National Survey of Drug Use and Health (NSDUH) data is biennial. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.

The data source for the past month heroin use rates, state-level OxyContin misuse rates, and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 12: Effects of Medical Marijuana Laws on Post-Reformulation Heroin Use by Pre-Reformulation OxyContin Misuse Rates, 4-Quantiles

	Dependent Var.: Pct. Ever Used Heroin						
	all	q1	q2	q3	q4	q1 & q2	q3 & q4
Panel A: all states							
mml × *post*	0.0801	0.480**	-0.00908	0.248	-0.370	0.256	-0.0503
	(0.116)	(0.209)	(0.422)	(0.342)	(0.276)	(0.200)	(0.194)
Panel B: states with fixed mml status after 2010							
mml × *post*	-0.0170	0.399	-0.505	0.399	-0.439	0.282	-0.103
	(0.144)	(0.245)	(0.801)	(0.484)	(0.274)	(0.206)	(0.213)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No
# of obs., panel A	510	130	130	130	120	260	250
# of obs., panel B	360	100	70	80	110	230	200

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimated effects of medical marijuana laws on post-reformulation heroin use by four different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation heroin use is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017, but it includes only ten specific years (2002, 2004, 2006, 2008, 2010, 2012, 2014, 2015, 2016, 2017) as the National Survey of Drug Use and Health (NSDUH) data is biennial. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.

The data source for the past month heroin use rates, state-level OxyContin misuse rates, and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 13: Effects of Medical Marijuana Laws on Post-Reformulation Heroin Overdose by Pre-Reformulation OxyContin Misuse Rates, 5-Quantiles

	Dependent Var.: Heroin Overdose Deaths per 100,000							
	all	bottom 20%	20%-40%	40%-60%	60%-80%	top 20%	bottom 60%	top 40%
Panel A: all states								
mml × *post*	-0.464**	0.112	-0.581**	0.552	-0.444*	-0.651***	-0.336**	-0.513***
	(0.227)	(0.219)	(0.280)	(0.357)	(0.261)	(0.178)	(0.137)	(0.139)
Panel B: states with fixed mml status after 2010								
mml × *post*	-0.153	0.0282	-0.271	-	-0.487*	-0.631***	-0.375***	-0.505***
	(0.140)	(0.108)	(0.177)	-	(0.271)	(0.192)	(0.136)	(0.140)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No	No
# of obs., panel A	908	197	180	173	178	180	550	358
# of obs., panel B	645	143	126	-	160	162	496	340

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimated effects of medical marijuana laws on post-reformulation heroin overdose death rates by five different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation heroin overdose death rates is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.
The data source for heroin overdose death rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). State-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 14: Effects of Medical Marijuana Laws on Post-Reformulation Heroin Overdose by Pre-Reformulation OxyContin Misuse Rates, 4-Quantiles

	Dependent Var.: Heroin Overdose Deaths per 100,000						
	all	q1	q2	q3	q4	q1 & q2	q3 & q4
Panel A: all states							
mml × *post*	-0.433*	0.00549	-0.460*	0.375*	-0.781***	-0.291*	-0.189
	(0.227)	(0.186)	(0.236)	(0.191)	(0.131)	(0.151)	(0.126)
Panel B: states with fixed mml status after 2010							
mml × *post*	-0.106	-0.0891	0.468	-0.202	-0.710***	-0.346**	-0.220*
	(0.149)	(0.109)	(0.351)	(0.237)	(0.163)	(0.146)	(0.124)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No
# of obs., panel A	908	233	234	225	216	467	441
# of obs., panel B	645	179	126	142	198	413	423

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table presents the estimated effects of medical marijuana laws on post-reformulation heroin overdose death rates by four different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation heroin overdose death rates is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.
The data source for heroin overdose death rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). State-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 15: Effects of Medical Marijuana Laws on Post-Reformulation Homicide Rates by Pre-Reformulation OxyContin Misuse Rates, 5-Quantiles

	Dependent Var.: Homicide per 100,000							
	all	bottom 20%	20%-40%	40%-60%	60%-80%	top 20%	bottom 60%	top 40%
Panel A: all states								
mml × *post*	-0.296	-0.258	-0.663*	-0.0724	-0.722*	-1.010***	-0.133	-0.615***
	(0.210)	(0.360)	(0.392)	(0.396)	(0.369)	(0.278)	(0.167)	(0.214)
Panel B: states with fixed mml status after 2010								
mml × *post*	-0.547**	-0.00390	0.695*	-	-0.597	-0.961***	-0.122	-0.596***
	(0.223)	(0.324)	(0.402)	-	(0.377)	(0.274)	(0.169)	(0.216)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No	No
# of obs., panel A	918	198	180	180	180	180	378	360
# of obs., panel B	648	144	126	-	162	162	324	342

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents the estimated effects of medical marijuana laws on post-reformulation homicide rates by five different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation homicide rates is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). State-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.

Table 16: Effects of Medical Marijuana Laws on Post-Reformulation Homicide by Pre-Reformulation OxyContin Misuse Rates, 4-Quantiles

	Dependent Var.: Homicide per 100,000						
	all	q1	q2	q3	q4	q1 & q2	q3 & q4
Panel A: all states							
mml × *post*	-0.248	-0.339	-0.388	0.0731	-0.993***	-0.305	-0.434**
	(0.237)	(0.314)	(0.368)	(0.250)	(0.292)	(0.192)	(0.175)
Panel B: states with fixed mml status after 2010							
mml × *post*	-0.475**	-0.137	1.295*	-0.578	-0.817***	-0.313	-0.505***
	(0.232)	(0.289)	(0.683)	(0.375)	(0.270)	(0.196)	(0.191)
trends&log population	Yes	Yes	Yes	Yes	Yes	Yes	Yes
time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cluster standard error	Yes	No	No	No	No	No	No
# of obs., panel A	918	234	234	234	216	468	450
# of obs., panel B	648	180	126	144	198	414	162

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. This table presents the estimated effects of medical marijuana laws on post-reformulation homicide rates by four different levels of pre-reformulation OxyContin misuse rates. In panel A, all 50 states plus the District of Columbia are included in the estimations. In panel B, states with a fixed medical marijuana legalization status after 2010, including those legalized in the first half of 2010, are included. Within each panel, each column represents the estimated result from states within the pre-reformulation OxyContin misuse rate quantile(s) specified in the column header. In Panel B, the estimated effect of medical marijuana laws on post-reformulation homicide rates is not reported for states that fall into the 40th to the 60th percentile of these rates, as all these states passed medical marijuana laws before 2010. The study period spans from 2000 to 2017. For the regression including all state quantiles in the pre-reformulation OxyContin misuse rate distribution, standard errors are clustered by state. In all other regressions, robust standard errors are used due to the limited number of clusters (states). All regressions include a set of state and year dummies and are weighted by state-year population estimates from the SEER program. They also include log state population, a saturated set of pre-trend, shift, and post-trend terms of the pre-reformulation OxyContin misuse rates and other pain relievers misuse rates, and a set of state-by-census region trends for states considered as sources and distribution hubs of heroin.
The data source for homicide rates is death certificate-based homicide data from the National Vital Statistics System (NVSS). State-level OxyContin misuse rates and other pain reliever misuse rates are from the National Survey of Drug Use and Health (NSDUH). Demographic characteristics are calculated using the age-adjusted version of county-level population files from the SEER program.