

# The Spillover Effects of OxyContin's Introduction on Crime

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## Abstract

Since the late 1990s, the U.S. has experienced a substantial rise in drug overdose and overdose deaths due to the increase in the usage of opioid drugs. This may also impact crime; however, empirical evidence on the same remains limited. Thus, this study estimates the effects of the opioid epidemic on crime. Consequently, the geographic variation in the distribution of OxyContin—a prescription opioid drug, driven by initial state drug prescription policies—is assessed. Using the Uniform Crime Reports (UCR) data, I find that compared to states with stringent prescription policies, the rate of property and violent crimes in states exposed to OxyContin increased by 12% and 25%, respectively. This suggests that the supply shock of opioids combined with loose prescription policies on drugs can create unintended and negative consequences in non-health issues, such as crime.

**Keywords:** triplicates, oxycontin, prescription opioids, crime.

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# 1 Introduction

The opioid epidemic has had devastating effects on various aspects of Americans' lives over the last two decades. Notably, it has contributed to a reduction of life expectancy as opioid-involved mortality rate increased from 3.67 per 100,000 in 1999 to 12.46 per 100,000 in 2015 (Case and Deaton 2015, 2017; Ruhm 2018)—more than 200% increase over 16 year. Recent studies have suggested that the epidemic has been facilitated by a combination of liberalized medical practices dealing with patients' pain in the 1990s and aggressive marketing of a pharmaceutical firm, Purdue Pharma. In the former scenario, based on the concern that pain was not treated properly, physicians started aggressively treating pain with opium based drugs which caused a rapid increase in the number of prescription opioid addicts (US Government Accountability Office 2003; Kolodny et al. 2015; Jones et al. 2018). Among the prescription drugs, OxyContin has been perceived as the primary contributor of the opioid epidemic (Cicero et al. 2005; Alpert et al. 2019). OxyContin, a long-acting pain reliever, was introduced to the market in 1996 by Purdue Pharma to replace their old product, MS Contin. Purdue aggressively marketed OxyContin as a pain-relieving drug not only for cancer patients but also those who experienced moderate to severe pain. This played a crucial role in expanding the market for prescription opioid analgesics (GAO 2003).

To understand the economic costs of the opioid crisis, researchers have examined the causal relationship between the availability of prescription opioids and a wide range of social outcomes, such as drug overdose, overdose-related mortality rates (Ruhm 2018; Alpert et al. 2019), labor market outcomes (Krueger 2017; Aliprantis et al. 2019), and child well-being (Evans 2019; Buckles et al. 2020). However, the unexpected consequences of this epidemic on crime remain ambiguous. It is important to analyze the evolution of crime rate during the opioid crisis because it severely affects society.

In this paper, I study the effects of the OxyContin’s introduction to the market on crime by leveraging geographic variation in the distribution of OxyContin throughout the U.S. Like Alpert et al. (2019), a state-level prescription policy called the triplicate prescription program was used to identify the cross-state variation in the supply of OxyContin. Preceding the modern prescription program’s goal was to prevent the diversion of controlled substances such as opioid drugs. This policy mandated physicians to write three copies of prescription using serially-numbered state-issued forms while prescribing Schedule II drugs.<sup>1</sup> The prescription program was effective against the prescription of OxyContin due to the hassle costs of prescribing Schedule II drugs and monitoring by the state government. When OxyContin was introduced in the U.S., the triplicate prescription system was operational in five states (California, Idaho, Illinois, New York, and Texas), which naturally created a cross-state variation in the degree of exposure to OxyContin. Additionally, physicians in states without the triplicate prescription policy (henceforth non-triplicate states) played a role in enlarging the gap. Fernandez and Zejcirovic (2017) showed that doctors who received a promotion for opioid drugs, for example Purdue Pharma’s marketing strategy, tended to write more prescriptions for opioid analgesics.

Using data from the Offense Known segment of the FBI’s Uniform Crime Reports (UCR) combined with a difference-in-differences (DID) approach, I find that states that did not implement triplicate prescription program at the time of OxyContin’s introduction experience a relative rise in overall crime by 13% compared to states with the triplicate prescription policy (triplicate states). The rise is reflected in both property (12%) and violent (25%) crimes. The largest effects for property crime are concentrated among the first five years after OxyContin entered market (until 2000). Non-triplicate states experienced a persistent rise in violent crime

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1. Drugs are classified into one of the five schedules based on their respective potential for abuse and dependency. For further details on drug scheduling, see the Appendix Table 1.

before declining in 2014-2016, though effects for these years are elevated. Among property and violent crimes, burglary and aggravated assault increased the most, respectively.

Further, the heterogeneous effect of OxyContin on crime by ethnic/racial groups is also examined. The drug overdose mortality rate indicates that White Americans have been severely hit by the opioid epidemic (Case and Deaton 2017; Alexander, Kiang, and Barbieri 2018). Based on the information above, it can be ascertained that White Americans in triplicate states may have been less exposed to OxyContin than those in non-triplicate states. Alpert et al. (2019) found that since OxyContin entered the market, drug-related death rates among White Americans in non-triplicate states have been much higher than White Americans in triplicate states. Consequently, it can be estimated that the evolution of crime among triplicate and non-triplicate states differs according to race and ethnicity.

Using the Arrest data segment of UCR, I do not find differential patterns in all types of crime arrests committed by White offenders across triplicate and non-triplicate states. However, I find increases in both property and violent crime arrests committed by African American offenders in non-triplicate states relative to triplicate states, and the magnitudes of estimates are broadly similar with the main findings (obtained from the Offense Known data).

To shed light on the structural effects of the introduction of OxyContin on crime, I exploit an instrument variable approach by using the number of opioid (OxyContin and oxycodone) prescriptions obtained from Medicaid data. I instrument for the number of opioid prescriptions per 1,000 Medicaid beneficiaries using the status of the triplicate prescription program. In line with extant studies on the deterrence effects of the triplicate prescription policy against overprescribing opioid drugs (Berina et al. 1985; Alpert et al. 2019), I find that opioid drugs were less prescribed in triplicate states by 44 per 1,000 Medicaid beneficiaries. The triplicate status-based IV estimates show that both property and violent crimes increase with an additional

opioid prescription per 1,000 Medicaid beneficiaries by 0.3% and 0.5%, respectively. In turn, these estimates indicate that non-triplicate states experienced rises in both property and violent crimes by 13.2% and 22% relative to triplicate states. The size of the IV estimates is comparable to that of the main DID estimates.

This study also assesses three potential channels through which OxyContin might have impacted crime. First, individuals exposed to OxyContin could have experienced mental health problems, such as violent tendencies and/or illegal behavior (Roth 1994; Jaffe and Jaffe 1995; Fazel et al., 2006; Moore et al. 2010, 2011). This study provides suggestive evidence on the fact that individuals in non-triplicate states suffered from mental health problem more frequently than those in triplicate states after the introduction of OxyContin in 1996. Second, the increased opioid consumption could raise crime rates, particularly violent crime, through an increase in alcohol consumption. If opioid addicts consumed alcohol more frequently, we would expect to see a rise in violent crime based on empirical findings that alcohol consumption is positively associated with violent crime (Carpenter and Dobkin 2008; Markowitz 2005; Heaton 2012; Anderson et al. 2017; Hansen and Waddell 2018). Thus, this study presents evidence on the increase in the consumption of alcohol in non-triplicate states after the introduction of OxyContin in 1996. Finally, the demand for OxyContin itself could have become a motive of criminal behavior. For example, Lavine (1997) revealed that opioid addicts may commit property crime to generate income to purchase drugs. Besides, the expanding market for the prescription opioid drugs could have generated the illegal drug market, driving up the prevalence of violent crimes (Miron 1999; Maher and Dixon 2001; Levitt and Rubio 2005; Sullivan and Elkus 2008; Dave et al. 2020).

This paper adds empirical evidence to the extant literature on the effects of stringent prescription monitoring programs on opioid misuse and other outcomes. Buchmueller and Carey (2018) show that mandatory-access PDMPs are associated with reduction in opioid misuse among Medicare Part D beneficiaries. Mandatory-

access PDMPs also reduce opioid misuse and opioid-related mortality for the general population (Ali et al. 2017; Grecu et al. 2019). Few studies have examined the effects of prescription opioids on crime outcomes using the supply-side intervention—PDMPs. Mallat (2018) showed that the implementation of PDMPs increased heroine-related crime outcomes by 48%, especially in the counties with high levels of opioid consumption.<sup>2</sup> However, by using the same policy intervention, Dave, Deza and Horn (2020) found that mandatory-access PDMPs decrease overall crime rate by 5% whereas voluntary PDMPs show null effects. Their results are driven by declines in property and violent crimes. The aforementioned studies exploit policy interventions—either voluntary or mandatory PDMPs—designed to curb the growth of opioid abuse and its negative effects. In contrast, this study uses the initial and persistent gap in the supply of OxyContin across states due to the triplicate prescription policy to examine the spillover effects of opioid crisis on crime.

Finally, this study does not speak to the potential benefits of OxyContin (or increased accessibility to prescription opioids) on the drug users’ health outcomes, such as better pain management but instead attempts to analyze the unexpected harms that prescription opioids can cause to society. The total economic burden with prescription opioids overdose and dependence was \$78.5 billion in 2019, according to the National Institutes of Health (NIH).<sup>3</sup>

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2. The author also investigated whether PDMPs has affected opioid prescriptions and found that PDMPs reduce oxycodone consumption by 10%.

3. NIH followed the estimates that Florence et al. (2016) provided. A report from the Council of Economic Advisers (CEA) in 2017 argues that the estimated total costs are underestimated because they did not use the conventional method to measure the value of lives federal agencies often use. Combining the non-fatality costs in Florence et al.(2016) with the adjusted fatal costs, the total costs of the opioid crisis could reach \$500 billion according to CEA.

## 2 Background

In 1996, Purdue Pharma introduced a new product to the market—OxyContin, which is an extended-release pain reliever containing oxycodone. Due to its high potential for abuse and dependency, OxyContin is classified as a Schedule II drug under the Controlled Substances Act, administered by the Drug Enforcement Administration (DEA). Initially, Purdue Pharma spent large amounts of money for aggressively marketing and promoting their new product.<sup>4</sup> Their goal was to expand the market for prescription opioid drugs in general including their own product. As noted in Alpert et al. (2019), before OxyContin, prescription opioids were usually prescribed to patients with late-stage cancer or severe pain. However, from the beginning, OxyContin was promoted for non-cancer pain as well. To encourage physicians to prescribe OxyContin, Purdue Pharma used various marketing approaches, including funding more than 20,000 pain-related educational programs and hosting more than 40 national pain-management conferences (GAO 2003; Van Zee 2009). They advertised that the probability of addiction is less than one percent and it was not subject to abuse because of its nature due to sustained-release technology.

However, Purdue’s claim turned out to be false. OxyContin users were able to consume the entire dose of opioid in the tablet by crushing or dissolving it in water or injecting it. While Purdue Pharma enjoyed the rapid increase in sales of OxyContin, the Drug Enforcement Agency (DEA) expressed their concerns on the high potential for abuse and diversion of the drug. In fact, in the early 2000s, news articles on the problem of OxyContin abuse began to surface from rural communities in several states, such as Kentucky, Maine, Ohio, Pennsylvania, Virginia, and West

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4. Purdue Pharma increased its sales forces from 318 in 1996 to 767 in 2002 and spent about \$200 million in marketing and promoting OxyContin in 2001 alone (GAO 2003; Van Zee 2009). In fact, the sales force reached 1,067 in 2002 after including sales representatives from Abbott Laboratories.

Virginia (GAO 2003). Several local and state governments filed lawsuits against Purdue Pharma for the false advertisement and overpromotion.<sup>5</sup>

Convinced by Purdue Pharma’s campaign and promotion, physicians began prescribing opioid drugs more often, even to patients with non-cancer-related pain. This caused substantial growth of the opioid drugs market in general. In 1999, 86% of all prescribed opioid drugs was for non-cancer-related pain (Van Zee, 2009; Floyd and Warren, 2017). Among other opioid drugs, OxyContin prescriptions increased approximately tenfold between 1997 and 2002 (Van Zee, 2009). Consequently, the sales of OxyContin skyrocketed from \$50 million in 1996 to \$1.1 billion in 2001, constituting 90% of the total prescription sales of Purdue Pharma by 2001 (GAO, 2003).

One of the key marketing strategies of Purdue was to target doctors with a history of prescribing opioid drugs. To identify such doctors, the pharmaceutical firm closely tracked the patterns of doctors’ prescribing behaviors across the country and directed its sales workers to focus on doctors who were willing to use OxyContin. Purdue Pharma target doctors from a variety of specialties, including cancer specialists and primary care physicians. Based on the accumulated data, Purdue Pharma realized that doctors in states with triplicate prescription programs were reluctant to use the Schedule II drug for their patients. The firm tried to eliminate the prescription regulation by lobbying but their primary focus was to promote OxyContin in non-triplicate states (Alpert et al., 2019).

The triplicate prescription program was an early versions of PDMPs. As mentioned in the introductions, doctors in states with triplicate prescription program were required to make three copies of the prescription using serially numbered state-issued prescription forms for prescribing any Schedule II drugs. Doctors had to keep a copy of the prescriptions for records for years, and the other two copies were given

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5. Van Zee (2009) reported that Purdue Pharma pled guilty to the criminal charges of misrepresenting their product and agreed to make a payment of over \$600 million as fines in 2007.



to the patients. The patients, then, submitted the two copies to the pharmacy. One of the two copies that the pharmacist received was sent to the state government.

Researchers have explored the effectiveness of the triplicate prescription program in deterring Schedule II drug prescribing. Berina et al. (1985) reported that physicians in states with triplicate prescription program were reluctant to prescribe opium-based drugs due to the fear of the state government’s monitoring of their prescribing practice. Citing Purdue’s internal document, Alpert et al. (2019) presented some evidence that Purdue knew that physicians in a state with triplicate program would reluctantly use their new product due to the inconvenience of prescribing.<sup>6</sup>

Triplicate prescription programs were implemented decades ago in California in 1939. California implemented the prescription program due to the increasing diversion of opioid drugs (Simoni-Wastila and Tolder, 2001). Since then, several states have followed California’s model of PDMP, for example, Idaho (1967), Illinois (1971), Indiana (1987), Michigan (1988), New York (1972), and Texas (1982) (Fishman et al., 2004). Among these states, the following five retained triplicate prescription program when Purdue Pharma introduced OxyContin to the market: California, Idaho, Illinois, New York, and Texas.

The presence of a triplicate prescription program in 1996 created a dramatic differential in the distribution of OxyContin across states over time. Alpert et al. (2019) revealed that individuals in a state without a triplicate program have been explicitly exposed to OxyContin than individuals in a state with triplicate program. They showed that the distribution of OxyContin was 50% higher in non-triplicate states since its entry into the market. The gap induced by triplicate status across states is the primary source of variation that I use as an identification strategy in this paper.

Following Alpert et al. (2019), the five states mentioned above are considered

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6. Alpert et al. (2019) obtained Purdue Pharma’s internal documents from recently unsealed court documents in multiple lawsuits against the pharmaceutical firm.

as triplicate states in this study. All the other states are defined as non-triplicate states. Although the triplicate program was discontinued in all states by 2004, triplicate status in this paper will be fixed over the sample periods. This is reasonable because the gap in the distribution of OxyContin widened even after 2004 rather than narrowing down (Alpert et al., 2019).

## 3 Data

### 3.1 *Uniform Crime Reporting*

I use data from the Uniform Crime Reporting (UCR) from 1990 to 2016 to understand the effects of OxyContin on crime. UCR contains the following three segments: Offenses Known and Clearances by Arrests, Arrest Data, and Supplementary Homicide Reports (SHR). Each segment of the UCR dataset has its strength. For the primary analysis of this study, I use the Offenses Known and Clearances by Arrests data. This data presents the most commonly reported (index) crimes across the country that can be divided into property-related and violent crimes. Specifically, there are eight index crimes: robbery, assault, rape, murder and non-negligent manslaughter, burglary, larceny, and motor vehicle theft.

The UCR dataset comprises self-reporting by local and state law enforcement agencies. It is noteworthy that not every agency reports for every period. This heterogeneity in reporting across agencies could cause reliability issues in the main analysis of this study. To address this concern, I only use agencies that reported crime in all 12 months in every year of the sample periods following Maltz and Targonski (2002). This yields a total of 7325 agencies. For the analysis of OxyContin's launch on crime, crimes are each crime per 100,000 residents in a given agency.

A limitation of using the Offenses Known Crimes data is that it does not provide demographic information on offenders and victims, and drug-related crimes; this

hinders the investigation on the direct effects of OxyContin’s introduction on drug-related crimes. To address these shortcomings of the Offenses Known Crime data, I supplement the main analyses with UCR Arrest Data.

The UCR Arrest data contains basic demographic information of offenders, such as age, gender, and race, as well as detailed information on drug-related arrests. These data enable me to study whether the arrest trends differ across racial groups and crime types (specifically drug-related crime) after the introduction of OxyContin. One caveat of using the Arrest data is that it does not separate cocaine and heroin, which prevents me from evaluating the effects of OxyContin’s introduction on opioid-related arrests.<sup>7</sup> Another issue with the Arrest data is that in the context of the criteria used to include data in this study, only 1,058 agencies with 26,026 observed arrests were included.

### 3.2 *Other Data*

In addition to the above-mentioned, I use Medicaid State Drug Utilization Data (SDUD) during 1991–2005 for the number of oxycodone and OxyContin prescriptions per state.<sup>8</sup> SDUD contains information on the number of prescribed outpatient drugs paid for by state Medicaid agencies including state, year, drug name, number of prescriptions, and dollars reimbursed. Following Alpert et al. (2019), I use the sample period up to 2005.<sup>9</sup> Using data from the University of Kentucky Center for Poverty Research (UKCPR), the number of opioid drug (oxycodone and OxyContin) prescriptions is determined by the annual Medicaid OxyContin prescriptions per 1,000 beneficiaries.<sup>10</sup> The potential drawback of using SDUD is the

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7. In terms of drug-related arrests, the UCR Arrest includes information on cannabis, cocaine/heroin, synthetic narcotic drugs, other drugs. Note that heroin is an opioid drug but is classified as a Schedule I drug.

8. Medicaid State Drug Utilization Data is available back to 1991.

9. From January of 2006, Medicare started covering outpatient drug prescription due to the introduction of Medicare Part D.

10. UKCPR provides a state-level panel data series called “National Welfare Data”. It covers population, employment, unemployment, welfare, and politics. More importantly, it contains information on the number of Medicaid beneficiaries.

representativeness of the Medicaid population to the general population that could be affected by OxyContin. However, the Medicaid population is considered a good proxy for those who are disproportionately affected by the opioid crisis (e.g., CDC 2009; Sharp and Melnik 2015; Alpert et al. 2019).

I use the Current Population Survey (CPS) data obtained from the IPUMS as a control for the basic socioeconomic characteristics at the state level. I construct state-level variables such as poverty rate, share of minorities, individuals aged between 18 and 25 years, males, share of males aged between 18 and 25, and share of individuals' educational attainment.<sup>11</sup> In addition, I collected information on the unemployment rate and minimum wage from the Bureau of Labor Statistics (BLS) and Vaghul and Zipperer (2016), respectively, to control for economic conditions that may affect crime.<sup>12</sup> Additionally, I use data from the Law Enforcement Officers Killed and Assaulted Program (LEOKA) from 1990 to 2016 to include the number of police officers in a state.<sup>13</sup> Further, I include policies that might affect crime and substance abuse, including Prescription Drug Monitoring Programs (PDMPs), SNAP/TANF availability for drug-related felonies, medical marijuana laws, and beer tax rates following the relevant literature.<sup>14</sup>

## 4 Empirical Strategy

I exploit a DID approach to estimate the impacts of OxyContin's launch on crime following the identification strategy suggested by Alpert et al. (2019). Whether a state had a triplicate program when OxyContin was introduced in 1996 creates a

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11. Educational attainment is categorized into: less than high-school degree, high-school graduates, some college degree, and college graduates.

12. The minimum wage dataset contains information on federal, state and sub-state level. For more details, see [https://github.com/equitablegrowth/VZ\\_historicalminwage/release](https://github.com/equitablegrowth/VZ_historicalminwage/release).

13. I scaled the number of the sworn police officer to the number of officers per 100,000 residents.

14. I used the Prescription Drug Abuse Policy System (PDAPS) website to obtain information on when (date) PDMPs were implemented by a state. I referenced Yang (2017) for SNAP/TANF availability for drug-related felonies. For marijuana laws, I refer to <https://norml.org/laws/decriminalization/>.

natural experimental setting that researchers can use to discover the causal link. In this study, five states had a triplicate system, and thus can be used as baseline group: California, Idaho, Illinois, New York, and Texas. All other states are regarded as treatment states. I consider the following DID specification as a baseline model to study the effects of OxyContin’s launch on crime:

$$Y_{ast} = \beta_0 + \beta_1 \text{Non-Triplicate}_s * \text{Post}_t + \beta_2 X'_{st} + \gamma_a + \delta_t + \epsilon_{ast} \quad (1)$$

where  $Y_{ast}$  represents the natural logarithm of crime rate known to police per 100,000 residents in a given agency  $a$ , in a state  $s$ , and in year  $t$ .<sup>15</sup>  $\text{Non-Triplicate}_s$  is an indicator variable for whether a state had triplicate system in 1996 and is fixed to the value of one over the entire period of this study.  $\text{Post}_t$  is an indicator variable that turns to the value of one for year greater than or equal to 1996. The coefficient of primary interest,  $\beta_1$ , represents the causal effect of OxyContin’s introduction on crime rate in the U.S.

$X_{st}$  is a vector of control variables that account for characteristics of each state to which agencies are belong. To control for unobserved and time-invariant agency-specific heterogeneity, I include agency-fixed effects,  $\gamma_a$ . In addition, year fixed effects,  $\delta_t$ , is included in all specifications to take into account national trends in crime. I also show estimates from models that include state-specific trends to control for systematic time-varying confounding factors that other control variables cannot capture across states.  $\epsilon_{ast}$  is an idiosyncratic error term. Standard errors are clustered at state level and results from all models are weighted by the relevant population size covered by the agency. It is noteworthy that standard clustered-robust standard errors may be too small given the small number of treated (or untreated) states of this study. Conley and Taber (2011) argues that this may cause over-rejection problem. To address this concerns, I also report p-values from

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15. I added 1 to each variable when converting them into the natural logarithmic form for the case of having the value of zero.

the wild cluster bootstrap with a 6-point weight distribution suggested by Webb (2014).

The key identification assumption in the DID research design is that trends in the crime rate should be parallel between triplicate states and non-triplicate states in the absence of OxyContin’s introduction (Angrist and Pischke 2007). To test the parallel trend assumption, I conduct the event-study exercise by using the following model:

$$Y_{ast} = \theta_0 + \sum_{\substack{t=1990 \\ t \neq 1995}}^{2016} \beta_t * 1(Non-Triplicate_s) * 1(Year = t) + \theta_1 X'_{st} + \gamma_a + \delta_t + \epsilon_{ast} \quad (2)$$

where Triplicate status is interacted with a full set of year dummies. I normalize  $\beta_t$  in year 1995 to zero. By exploiting this event-study model, coefficients on interaction terms present the dynamics of the main DID effects obtained from Equation (1) over all years.

Case and Deaton (2015, 2017) show that the opioid epidemic is closely related to the decline in life expectancy of Americans, especially low-income white Americans without college degree.<sup>16</sup> In contrast, they find no clear negative results of the opioid crisis on African Americans and Hispanics. The fact that there is variation in the degree of the impacts of the opioid crisis among ethnic/racial groups implies the possible heterogeneity in the evolution of crime rate before and after the introduction of OxyContin by ethnic groups. To check the heterogeneity in the impacts of OxyContin on crime outcomes by ethnic groups, I estimate the Equation (1) using the UCR Arrest data.

Finally, I conduct the synthetic control method to account for the small number of states which operated the triplicate prescription program in my research setting. In this practice, I aggregate triplicate states into a single treatment unit following Abadie et al. (2010). Furthermore, I perform a permutation test suggested by

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16. In the work of "deaths of despair", they found that the death rate of the low-income white non-hispanic group has increased substantially since the mid 1990s relative to non-white groups in the U.S. and relative to death rates in other wealthy countries.

Fisher (1935) to check whether my main results are large and/or unique. In this test, I randomly assign a fake treatment status to randomly chosen agencies in non-triplicate states sample, and estimate the effects of fake treatment by using Equation (1), and repeat this procedure 1,000 times. Then, I create a distribution of the fake-treatment effects with the coefficient obtained from main results.

## 5 Results

In this section, I start with presenting the discrepancies in crime rates and demographic characteristics between triplicate and non-triplicate states. Then, I estimate the causal relationship between the opioid crisis and crime. Moreover, I conduct the event-study exercise to check the existences of pre-trends in crime and the dynamics of OxyContin’s effects on crime. I also perform the synthetic control estimations and permutation tests for robustness checks for my main analyses.

### 5.1 Summary Statistics

Table 1.1 presents the descriptive statistics for Part I crime rates. Throughout the sample period, there were on average 3587 reported crimes per 100,000 residents. Property crimes account for approximately 90% of total crime, and violent crime constitutes 10%. Looking at disaggregated crime types in property crime, the most prevalent crime is larceny with 2296 per 100,000 residents, which is 71% of the entire property crime. For violent crime, aggravated assault is the most common crime with 244 crimes per 100,000 residents, and this accounts for 65% of the entire violent crime. The overall crime rates are higher in triplicate states than in non-triplicate states.

Over this time period, crime rates were going down across the country (Levitt 2004; Farrell, Tilley, and Tseloni 2014). Table 1.2, however, reveals crime rates in non-triplicate states reduced at a slower rate than in triplicate states. As a result,

the gap in crime outcomes between two sets of state groups declined substantially over the sample period of this study. For instance, the difference in violent crime decreases to 43.13 per 100,000 residents in the post-1996 period from 164.41 per 100,000 residents in the pre-1996 period. This pattern can be found in Figure 1. Across all crime types, Figure 1 shows that the level of crime rates is lower in non-triplicate states than that of triplicate states. However, triplicate states experience reductions in crime rates at a steeper rate during the late-1990s than non-triplicate states, which decreases the differences in crime rates dramatically between the two groups.

Table 1.3 presents summary statistics for state-level control variables. Non-triplicate states have a lower proportion of the population whose educational attainment is low and ethnic/racial minority groups. Moreover, individuals living in non-triplicate states are less likely to live under the poverty rate than those in triplicate states.

## 5.2 Difference-in-Differences

I first present the DID estimates that capture the effects of the introduction of OxyContin on crime using Equation (1). In Table 2.1, I report estimates of equation (1) for each crime outcome with and without state-specific time trends. Column 1 shows that non-triplicate experiences increase in total crime by 13% relative to triplicate states since OxyContin entered the market. This increase is reflected in both crime types. Column 3 shows that property crime rises by 12% relative to triplicate states and it is statistically significant at the 5% level. Column 5-6 presents that non-triplicate states experiences 25% increase in violent crime (13% when state-specific linear trend is added) relative to their counterpart states.

To uncover which type of specific crime drives such results in property and violent crime, I present estimates from the same DID equation with each crime type being an dependent variable. As can be seen in Panel A of Table 2.2, every type of



violent crime shows relative increase in non-triplicate states except for rape; estimate for rape is statistically significant at the 10% level with the clustered-robust standard errors, but the statistical significance disappear with the wild cluster bootstrap p-value. Among violent crimes, aggravated assault climbed the most, by 24%, relative to triplicate states. In Panel B, the property crime with the most increase is burglary crime which rises by 13% relative to triplicate states. The table shows that larceny also grows by about 11% in non-triplicate states relative to triplicate states. These results are in line with other studies that show the causal link between policies that affect substance use and crime (Wen, Hockenberry, and Cummings 2017; Doleac and Mukherjee 2019; Packham 2019; Dave et al. 2020).<sup>17</sup>

Table 3 presents the heterogeneous effects of OxyContin on crime across ethnic/racial groups. I reproduce the Equation (1) using the UCR Arrest data. One of the benefits of using the Arrest data is that it contains demographic information on offenders. However, there is a trade-off, which is the shrink in sample size.<sup>18</sup> Nevertheless, Table 3 provides some evidence on the existence of heterogeneity in crime outcomes by ethnic/racial groups. Across all types of crime, non-triplicate states experienced relative increases in the number of White arrestees by less than 10%, but these estimates are not statistically different from zero. On the other hand,

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17. Wen et al. (2017) presents that the Medicaid expansion resulted in a reduction in the rates of robbery, assault, and larceny through increasing substance use disorder (SUD) treatment. Doleac and Mukherjee (2019) find that states with naloxone access laws experiences increase in the opioid-related theft and arrests for possessions and sale of opioid by 30%, 17% and 27%, respectively. Packham (2019) suggests that drug-related arrests (by 16%) and local rates of theft (by 24%) rises after opening syringe exchange programs which was created with intent to reduce HIV. In a recent working paper, Dave et al. (2020) shows that having PDMPs (especially mandatory access one) are associated with decline in total crime by 7-8%. In terms of specific types of offenses, They find that mandatory-access PDMPs have significant negative effects on assault and burglary by about 10–11%.

18. In addition to decline in sample size, nineteen states were removed from sample including Idaho. To make sure whether my results are valid even when the removed states are included, I run the same analysis with unrestricted version of the Arrest data. In the unrestricted sample, there are 12,888 agencies. The main difference in results between unrestricted and restricted versions is drug-related arrests among African Americans. The estimated result indicates that drug-related arrest increases in non-triplicate states by 25% relative to triplicate states. Except for drug-related arrest, the remaining results are broadly consistent with the ones with the restricted sample. However, sizes of coefficients are a bit larger in the analysis of unrestricted version.

Panel B indicates that non-triplicate states experienced increases in both property- and violent-related arrests among Black relative to their counterpart states. For drug-related arrests, non-triplicate states experienced 17% rise among Black offenders. However, the coefficient is not statistically significant. I find 3% decrease among White offenders, but again it is not statistically significant.

### 5.3 Event Study Analysis

In this section, I examine the dynamics of the effects of OxyContin’s introduction on crime by using Equation (2). I plot the estimated coefficients obtained from the event-study model with 95% confidence intervals: five lead years (1990–1994) and twenty-one lag years (1996–2016). I normalize the coefficient in 1995, the year before OxyContin was introduced to the market, to zero. Overall, each panel of Figure 2 shows that non-triplicate states experienced a relative rise in all types of crime rates since 1996, though the effects appear to be lagged; for both crime types, the effects began rising after 1997. These delayed effects are plausible considering that it took time for drugs users to get addicted to and misuse OxyContin, thus engage in illegal activities. However, the pattern after 1997 diverges between property and violent crimes over years.<sup>19</sup>

Panel B of Figure 2 shows that the largest effects for property crime are concentrated on first five years except for 1997. Although the effects for property crime are not consistently rising over time, they remain above zero. Looking at the estimates of the lead years, Panel B suggests that there might exist some upward pre-trends in property crimes, though they are close to zero; the coefficients for years 1990–1994 are statistically significant.

On the other hand, Panel C of Figure 2 suggests that non-triplicate states experienced persistent and large increases in violent crime before decreasing in the last

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19. Total crime is the sum of property and violent crimes. As shown in Table 1.1, property crime accounts for 90% of total crime. Hence, the pattern of total crime resembles with that of property crime.

three years. The pre-OxyContin effects are near-zero and statistically insignificant for violent crime. This may indicate that there is no pre-existing trend in violent crime.

#### 5.4 Synthetic Control Analysis

Next, I conduct the synthetic control analysis to employ the data-driven approach in the selection of the comparison group following Abadie et al. (2010). As shown in Figure 1, the raw mean trends prior to 1996 slightly differed between triplicate and non-triplicate states. In addition, Figure 2 suggests that there might exist some upward pre-trends in total and property crimes in non-triplicate states before OxyContin was introduced to the market. These evidence imply that triplicate states might not provide a suitable control group for non-triplicate states (and vice versa). To overcome the arbitrary choice of the comparison group, I run the synthetic control estimation. In this analysis, I use the five triplicate states as the treatment group and non-triplicate states as potential donor states. The data-driven analysis is often used to discover the causal effects when there is only one treatment unit. As I have five triplicate states, I aggregate them into a single treatment unit to conduct the synthetic control estimation.

This data-driven procedure method creates a suitable control group, which is the weighted average of non-triplicate states to resemble the characteristics of triplicate states before 1996. Under this framework, any divergence in crime rates between triplicate states and the synthetic triplicate states is due to the introduction of OxyContin. Table 4.1 displays how similar the pre-trends in crime outcomes of the synthetic triplicate states are with that of triplicate states before 1996. It shows that the synthetic triplicate states are much closer to the actual triplicate states in all types of crime than non-triplicate states. Table 4.2 - 4.4 present the calculated weights that are assigned to each non-triplicate states among donor pools for constructing the synthetic triplicate states. Given that not every state is assigned

weights, it may indicate that using the synthetic triplicate states is more proper to study the impacts of OxyContin on crime rather than using the whole non-triplicate states as the control group.

Figure 3 presents the evolution of crime rates for 1990-2016 between triplicate states and the synthetic triplicate states. Triplicate states and the synthetic triplicate states behave very similarly up to 1995. From 1996 when OxyContin entered the market, trends start to diverge between the two groups. In general, crime outcomes in triplicate states decreased more rapidly than the synthetic control group did, particularly for violent crime. It indicates that the baseline results are not driven by the pre-trends in crime outcomes and my main results are robust.

### 5.5 Robustness Checks

In this section, I present a number of sensitivity checks to verify whether my main results are robust to alternate specifications. In addition, I conduct permutation tests following Fisher's (1935) to see whether my main results are large and unique.

The effects of OxyContin's introduction on crime may vary across states. Moreover, it could be plausible that some states drive crime outcomes up while other states do not experience relative rise in crime. To test this possibility, I replicate Table 2.1 dropping each state at a time. Table 5.1-5.2 present the effects of individual states on crime outcomes. The results are qualitatively similar with the main analysis across this exercise, though some states have stronger effects on the estimates in either directions. For instance, magnitudes of the estimates for both property and violent crime are smaller than the main estimates when California is excluded: by 2 percentage points for both property and violent crimes, respectively. On the other hand, dropping Texas or New Jersey raises magnitudes of the coefficients on all types of crime.

I also consider the possible issue that the U.S. went through several economic downturns during the sample period, so it might affect criminal behavior across

states through different economic and labor market conditions.<sup>20</sup> To address this issue, I add quadratic trends to the main analysis following the suggestion of Neumark et al. (2014). Column 3 of Table 5.3 indicates that magnitudes of the coefficients are smaller than the main results when adding quadratic state-specific trends. For property crime, the coefficient is almost same with the main analysis (12%), while the estimated effects on violent crime shrinks to 7.1% from 25%. These estimates remain statistically significant at 1% and 5% level, respectively. However, the coefficient on violent crime is statistically significant at 10% level with wild bootstrap p-value.

Another potential issue is that triplicate states might experience systematically different patterns in drug overdose and its related problems (in this study, crime outcomes) since they have large population and major urban cities within them. To address this concern, I reproduce the main DID analysis by selecting the four largest states among non-triplicate states in terms of 1990 population size as control states following Alpert et al. (2019): FL, PA, OH, and MI.<sup>21</sup> In column 4 of Table 5.3, the size of estimates is qualitatively similar with that of main results across all types of crime. But, some of the estimates, such as property and total crime, are no longer statistically significant due to the increased noisy caused by the reduced sample size. The test results may imply that the relative decline in crime outcomes in triplicate states are not driven by their large population size. For violent crime, the coefficient is no longer statistically significant when using the wild bootstrap p-value.

In the main DID estimations, the standard errors are clustered at the state level. As discussed in Abadie et al. (2010) and Buchmueller et al. (2011), however, it

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20. There are three main recessionary periods. First two recession periods are early in 1990s and early in 2000s, respectively. The last one is the Great Recession.

21. I followed the way of selecting the largest states as control group as in Alpert et al. (2019). They selected the four largest states in terms of 1990 population size, which are FL, PA, OH, and MI. They excluded ID for its small population size. Accordingly, I excluded ID from this exercise, but the results are similar when ID was included.

might not be the most conservative way for statistical inference when having a small number of clustering units or treated/untreated groups. Even though there are more than one treatment group in my research setting, comparing 5 triplicate states (as a control group) with all others could be problematic as well.<sup>22</sup> I address this concern by implementing an alternative inference method: Fisher’s (1935) permutation test. The goal of this test is to investigate whether the main results are large or abnormal relative to the distribution of the fake treatment effects assigned to non-triplicate states sample. In this test, I assign fake-treatment status to randomly chosen agencies in non-triplicate states and re-estimate Equation (1). I repeat this exercise 1,000 times. Figure 4 shows the distribution of the estimated coefficients of fake-treatment under the null hypothesis that the introduction of OxyContin does not affect crime rates regardless of triplicate status. I plot 5th and 95th percentile values of the estimated fake-treatment effects with a dashed lines and my main estimates with a solid line in Figure 4. According to the test, it is statistically rare to observe the main estimates inside of the distribution of the fake-treatment effects.

I present unweighted regression results in the Appendix Table 2 to check the sensitivity of the main results to weighting. I calculate weights to account for the population that each agency covered. The estimated coefficients and the patterns of results are broadly similar with the main results, though the sizes of coefficients across all types of crime are a bit larger in results of unweighted regression. I also conduct an event-study analysis with unweighted models and figures are presented in Appendix Figure 1. Again, trends from unweighted models are similar with the ones from weighted models.

Finally, I also perform the sensitivity check for the event-study analyses using a novel estimation approach developed by Rambachan and Roth (2020). Under

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22. Buchmueller et al. (2011) had a single state as a treatment group to study the effect of an employer health insurance mandate on labor demand in Hawaii. Abadie et al. (2010) also had a single state, which is California, in studying the causal effects of tobacco policy on tobacco consumption.

different assumptions on how informative pre-treatment difference in trends are of counterfactual post-treatment difference in trends, I found that my results are robust to the certain degree of violation of the parallel trend assumption. For space, I discuss the details of how I conduct this sensitivity check using the technique of Rambachan and Roth in Appendix B.

## 6 Suggestive Evidence on Mechanisms

In this section, I explore potential mechanisms through which the differential exposure to OxyContin between triplicate and non-triplicate states could be linked to crime outcomes.

The presence of the triplicate prescription program during the introduction of OxyContin does not necessarily mean that individuals in non-triplicate states were more exposed to OxyContin and its side-effects such as crime outcomes. Using SDUD data, I explore whether the distribution of OxyContin prescriptions truly differed between triplicate and non-triplicate states. Panel A of Appendix Figure 2 presents the distribution of OxyContin between the two groups.<sup>23</sup> The data reveals that non-triplicate states experienced higher rates of OxyContin prescriptions per 1,000 Medicaid beneficiaries since 1996 than triplicate states. According to the graph, the number of OxyContin prescriptions in non-triplicate states grew rapidly in the first few years after its introduction. In contrast, the distribution of OxyContin in triplicate states remained relatively flat over time, though an upward trend can be observed in the latter years.

I also investigate the possible spillover effects of OxyContin on the usage of other prescription opioids. Specifically, I examine the pattern of oxycodone prescriptions among the Medicaid population before and after 1996.<sup>24</sup> If the introduction of

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23. I replicated the graph following Alpert et al. (2019). There is a minor difference in the data corresponding with last year between the authors and my figures.

24. Oxycodone is another pain killer drug that contains opioid and is also classified as Schedule II.

OxyContin made physicians comfortable with prescribing oxycodone combination drugs, the disparity in the distribution of oxycodone may appear since 1996. Indeed, Panel B of Appendix Figure 2 indicates that the distribution of oxycodone is remarkably similar between the two groups before 1996; however, it increased exponentially in non-triplicate states since 1996, while remaining flat in triplicate states until the last year. This pattern is consistent with the distribution of oxycodone shown in Alpert et al. (2019).<sup>25</sup>

Similarly, I plot the distribution of hydrocodone to examine whether the usage of other prescription drugs that are not covered by the triplicate prescription program, changed after the introduction of OxyContin. If there exists a systematic difference in the usage of prescription drugs other than OxyContin (as well as oxycodone) between the groups, it is difficult to argue that OxyContin is the main cause of the opioid crisis and its related issues. Hydrocodone is used to treat acute pain and also contains opioid, but it was classified as a Schedule III drug at the time that OxyContin was introduced. Hence, hydrocodone drugs were not subject to the triplicate prescription program.<sup>26</sup> Panel C of Appendix Figure 2 does not present any change in the pattern of hydrocodone prescription among the Medicaid population across the two sets of state groups. The figure shows that hydrocodone was similarly distributed across the two groups during the sample period.<sup>27</sup> Although this figure is obtained from the Medicaid population, it indicates that the Purdue Pharma’s drug and corresponding promotion affected doctors’ propensity to prescribe oxycodone combination drugs, including OxyContin.

Appendix Table 3 presents the results using IV estimation to facilitate the com-

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25. Alpert et al. (2019) used Automation of Reports and Consolidated Orders System (ARCOS) from DEA to observe the difference in the supply trends of opioids. They suggested that the growth of oxycodone prescriptions could be a possible spillover effect of OxyContin’s promotion of the use of other opioid drugs to expand their market not only for cancer pain but non-cancer-related pain.

26. Hydrocodone was rescheduled to a Schedule II drug on October, 2014.

27. Using ARCOS data, Alpert et al. (2019) also illustrate that trends in hydrocodone distribution were almost indistinguishable between triplicate and non-triplicate states.



prenshion of the figures and structural effects of OxyContin on crime. In this exercise, I instrument the number of prescription opioid drugs per 1,000 Medicaid beneficiaries with an interaction between triplicate status and a post 1996 indicator.<sup>28</sup> Column 1 of Appendix Table 2 presents the first stage estimate of triplicate status on the distribution of opioid-drug prescriptions per 1,000 Medicaid beneficiaries. It is revealed that non-triplicate states record more prescription opioid drugs by 44 per 1,000 beneficiaries than triplicate states; it is statistically significant. Columns 2-4 present IV estimates on crime outcomes. Coefficient of prescription opioid drugs shows that property crime increased by 0.3% with an additional increase in opioids per 1,000 Medicaid beneficiaries. This means that property crime in non-triplicate states increased by 13.2% relative to property crime in triplicate states. Column 4 presents that the number of prescription opioid drugs is associated with rise in violent crime by 0.5% and it is statistically significant. Thus, non-triplicate states experienced about 22% increase in violent crime compared to their counterpart states since the introduction of OxyContin in 1996. These IV estimates correspond with the DID estimates for both property and violent crime, respectively.

In addition to the deterrence effects of triplicate prescription program and its impacts on crime, I suggest three potential channels through which the widespread prescription opioid drugs might affect crime indirectly. First, OxyContin itself could negatively affect mental health of those who take the pill regularly. Second, alcohol consumption might have increased among those who were addicted to opioids, making them binge drinkers. Third, the accessibility and availability of opioid drugs might have instigated a criminal motive.

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28. I create a variable called “prescription opioid drugs” by combining the number of prescriptions per 1,000 Medicaid beneficiaries of both oxycodone and OxyContin. I used this variable for IV estimation rather than just using OxyContin prescriptions per state because OxyContin was available only since 1996. Plus, Appendix Figure 2 indicates that there was no systematic difference in the distribution in oxycodone between triplicate and non-triplicate states until 1996 and patterns of the distribution of oxycodone after 1996 were fairly similar like that of OxyContin.

First, OxyContin might have negatively affected the mental health of individuals who took prescription opioids regularly, and thus opioid addicts could be more prone to illegal behaviors. Although violence is not commonly considered as a side-effect of opioids abuse,<sup>29</sup> one cannot ignore cases where opioid addicts might display violent tendencies, particularly during withdrawal.<sup>30</sup> Roth (1994) suggested that withdrawal from opioids could intensify aggressive and defensive responses to provocative situation. Other papers have shown that individuals may experience agitation, aggression, hyperalgesia, anxiety, as well as physical pain (Jaffe and Jaffe 1995). Hence, it is possible that opioid abuse affects mental health in ways that are associated with violent behavior. For example, Fazel et al. (2006) suggested that individuals with severe mental health problem have a higher probability of committing violent crimes compared to the general population. Moreover, several studies have shown that there is positive association between substance abuse and violence. Markowitz (2000, 2005) found that decriminalizing marijuana increases the incidence of assault and robbery. Moore et al. (2010) showed that oxycodone is significantly associated with violence-related adverse drug events. In addition, Moore et al. (2011) suggested that fathers who are addicted to opioid are more likely to use intimidating behaviors toward their partners. In the context of mental health and crime, Cuellar et al. (2004) revealed that improvement in mental health through substance abuse treatment lowers the probability of detention for any offense among juveniles. For the general population, Marcotte and Markowitz (2011) found that improved mental health through psychiatric drugs is associated with reduction only in violent crime rates.

To understand the effects of OxyContin on individuals' mental health, I conducted an event-study analysis of the mental health trend across triplicate and

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29. Well known effects of opioid use are the production of analgesia, altered mood (often euphoria), decreased anxiety, and respiratory depression (Boles and Miotto, 2003).

30. Kleber (1995) suggests that withdrawal from opioids can start even 8–12 hours after the last doses.

non-triplicate states using Behavioral Risk Factor Surveillance System (BRFSS) data.<sup>31</sup> Panel A of Appendix Figure 3 reveals the average number of days that individuals experienced mental health problems during the last 30 days prior to the survey. After 1996, this number increased in non-triplicate states. However, the differences are not statistically significant until the last three years of the sample. This indicates that chronic exposure to OxyContin (or prescription opioids) harms individuals' mental health. However, OxyContin cannot be proved as the sole culprit of deteriorating mental health across the two groups. Nevertheless, the mental health trend provides suggestive information about the effectiveness of the triplicate prescription programs in protecting the mental health of people from the opioid epidemic. Combining this suggestive evidence with existing empirical results, it is safe to say that OxyContin might have affected crime through mental health.

Second, individuals addicted to prescription opioids may consume alcohol more frequently and heavily than non-addicted individuals. Esser et al. (2019) found that people who misused prescription opioids are more likely to be binge drinkers, who in turn were more prone to the abuse of opioids compared to non-drinkers. While it is not clear whether opioid increases individuals' alcohol consumption or vice versa, extant evidence suggests that opioid and alcohol are commonly used together (Hickman et al., 2008). In addition, the link between alcohol consumption and violent behavior is well-documented in the literature. Markowitz (2000, 2005) used beer tax to discover the causal relation between alcohol and violent crimes. The author found that the probability of assault and drug- or alcohol- related assault decreased with higher beer tax. Anderson et al. (2017) showed that increase in drinking establishments is positively associated with violent and property crimes. Other papers have also suggested a positive relationship between alcohol consumption and violent behavior (Carpenter and Dobkin 2008; Heaton, 2012; Hansen and Waddell, 2018).

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31. I used the BRFSS data for 1993-2016 as the survey enquired about mental health in 1993.

Using data from the National Institute on Alcohol Abuse and Alcoholism (NIAAA), I explore whether a disparity exists in the patterns of alcohol consumption across triplicate and non-triplicate states over the sample years.<sup>32</sup> As shown in Panel B of Appendix Figure 3, alcohol consumption increased in non-triplicate states immediately since 1996. Although point estimates are plotted with large noisy since 1999, they remain above zero up to the last year of data. More importantly, there is no pre-trend in alcohol consumption between triplicate and non-triplicate states. Combining these results with previous studies of the causal link between alcohol and violent behaviors, it is plausible that the states with greater exposure to OxyContin experienced alcohol-related problems more, adversely affecting the crime rate than their counterparts.

Finally, the introduction of OxyContin itself might have instigated criminal behavior. For instance, increased demand for prescription opioid or other illicit drugs might create the illegal drug market. It is possible that individuals who are addicted to prescription opioid drugs seek other illegal drugs such as heroin or cocaine, usually in the underground market. Prior works have documented the possible causality between exposure to OxyContin and transition to heroin. Alpert et al. (2018) revealed that heroin-related death drastically increased in states with the highest initial rate of OxyContin misuse when OxyContin was reformulated as an abuse-deterrent version. Corresponding with this, another paper shows the supply-side intervention through PDMPs is associated with an increase in illegal drug deaths (Meinhofer, 2018). The transition from prescription opioids to illegal drugs inevitably impacts the crime rate. Mallatt (2018) studied the impact of the supply-side intervention (PDMPs) on heroin-related crimes. The author found that

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32. NIAAA contains the per capita consumption of alcoholic beverages (in gallons) for each state, including Washington D.C. It was originally constructed by Haughwout and Slater. I downloaded this data from ICPSR. BRFSS data also includes information on alcohol consumption, but it was not in the core questionnaire until 2012 and every state did not report alcohol consumption before 2012. Thus, I used NIAAA data to explore alcohol consumption patterns according to triplicate status.

heroin-related crimes increased (notably within the most opioid-dense counties) after the state implemented PDMP. Furthermore, the drug market is associated with an increase in violent crime such as murders and non-fatal shootings with handguns (Maher and Dixon, 2001; Miron, 1999; Levitt and Rubio, 2005; Sullivan and Elkus, 2008). Another possibility is that opioid addiction can instigate violent behaviors to generate income to sustain the addiction. Lavine (1997) suggested that opioid addicts might engage in illegitimate and/or violent behaviors to secure income to purchase drugs.

## 7 Conclusion

Due to the aggressive marketing and promotion of OxyContin by Purdue Pharma, and lax prescription regulations in the 1990s, the market for prescription opioid drugs expanded dramatically after OxyContin's launch in 1996. This caused an inevitable opioid crisis in the U.S. However, due to the application of stringent prescription monitoring policies, such as the triplicate prescription program, five states (California, Idaho, Illinois, New York, and Texas) were able to regulate the availability of OxyContin. In comparison, states without such policies experienced a substantial increase in the consumption of opioids from the late 1990s. This has negatively impacted health-related outcomes such as drug-overdose, and a broad range of social outcomes, such as crime.

Overall, non-triplicate states experienced a relative increase in both property and violent crimes by 12% and 25%, respectively. Specifically, violent crime increased constantly in non-triplicate states after OxyContin entered the market. The main results imply that non-triplicate states could have experienced less violent crimes: 63 offenses per 100,000 on average from 1996 to 2016; and less property crimes: 318.15 offenses during the same period. Looking at specific crime types, aggravated assaults increased the most (24%) among violent crimes and burglary

occurred the most among property crimes (13%).

I also explore the heterogeneity of OxyContin's impacts on crime by racial/ethnic groups. The findings suggest that both property and violent-related arrests increased in the case of Black Americans in non-triplicate states compared to triplicate states. However, the same in case of White Americans was not determined. Additionally, states without triplicate prescription program recorded an increasing number of prescriptions for opioid drugs. The results from IV approach indicate that the number of prescription opioids is positively associated with overall crime rates.

Since violent crimes are more devastating economically than property crimes, I evaluate the amount that could have saved if non-triplicate states would have implemented the triplicate prescription program. In this cost analysis, I combined the main results with the estimates of economic costs of crime provided by Chalfin (2015). Throughout the sample period, approximately 70% of the population resides in non-triplicate states.<sup>33</sup> If these states applied the triplicate program during the introduction of OxyContin, regulating its availability, 25% of violent crimes could have been prevented. This would lead to 17.5% decline in violent crime. Given that there were 1,248,185 violent crimes in 2016 according to the FBI report, the U.S. would have about 218,432 less violent crimes. Taken together, the hypothetically reduced number of violent crime alone would have saved \$33 billion in 2016.<sup>34</sup>

I acknowledge that my findings should be interpreted with caution because estimated results are obtained from data that provides information only on crimes committed in the U.S. Therefore, I cannot actually ascertain whether criminals have a history of consuming prescribed opioid drugs (or at least a history of sub-

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33. According to the Census, the number of population in triplicate states accounted for about 30% throughout the sample period.

34. Chalfin (2015) provides the economic costs of each crime type that take into account both the tangible and intangible costs. I find the expected costs of a violent crime by using the estimates and the shares of each violent crime type, which is \$152,417 (in 2016 dollar). Using the same approach, the expected costs of a property crime is \$2,651.28 (in 2016 dollar).

stance abuse) prior to committing a crime. Similarly, I cannot ascertain whether prescription opioid drugs are effectively involved with the crime observed in my sample. Nevertheless, the results indicate the importance of implementing a stringent prescription policy. The findings provide empirical evidence on the fact that the supply shock of opioids combined with loose prescription policies could have caused an unintended and negative effect on non-health outcomes, such as crime.

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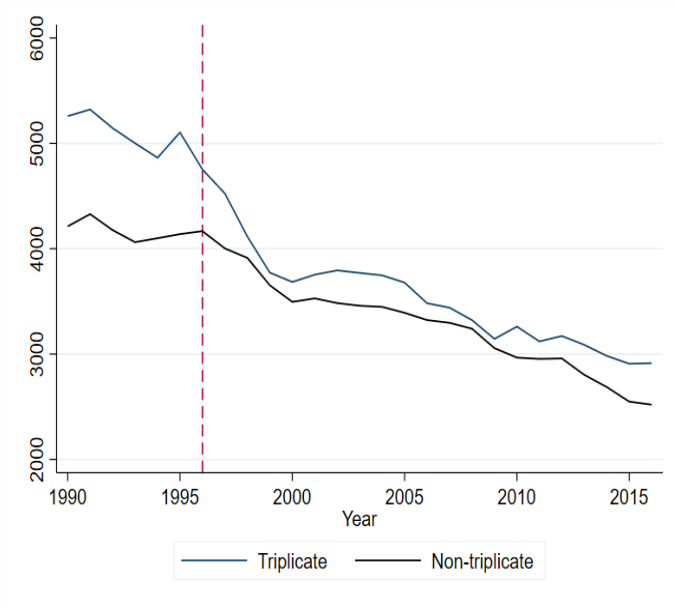
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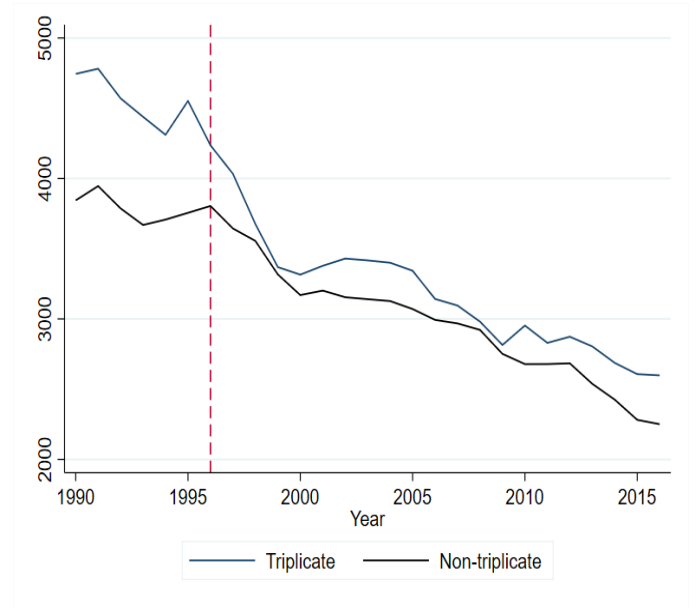
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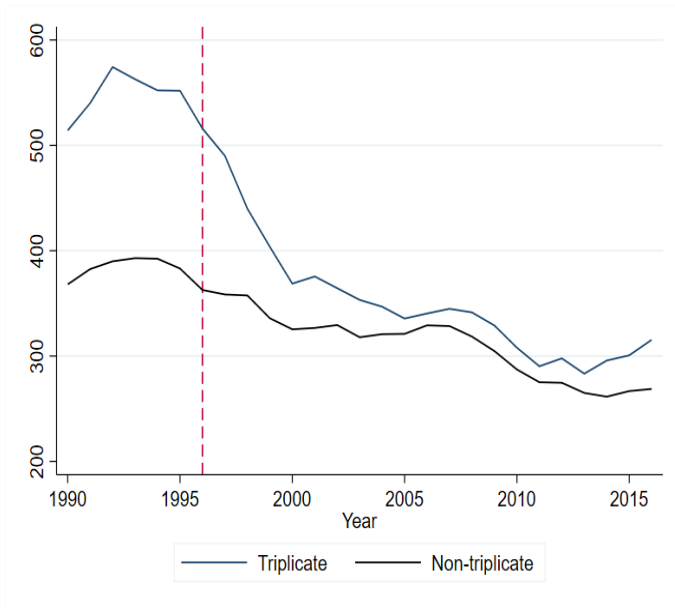
Figure 1: The Trends of Crime Rates



(a) Total Crime



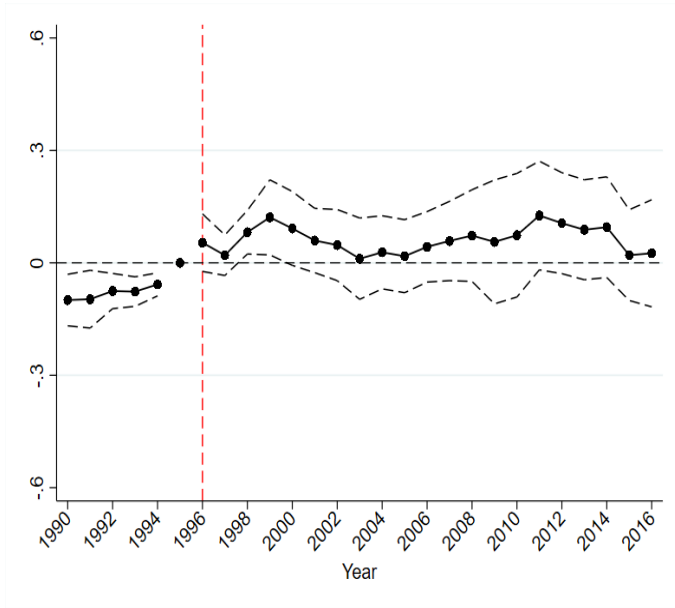
(b) Property Crime



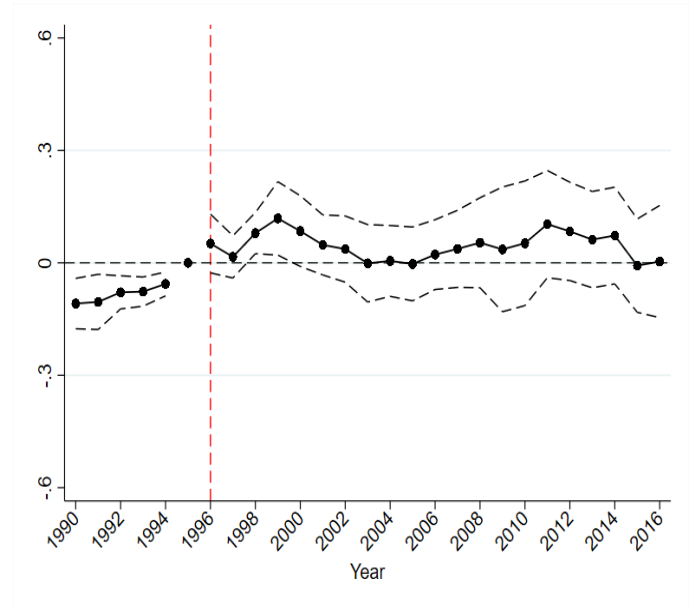
(c) Violent Crime

Data Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.

Figure 2: Event-Study Estimates



(a) Total Crime



(b) Property Crime

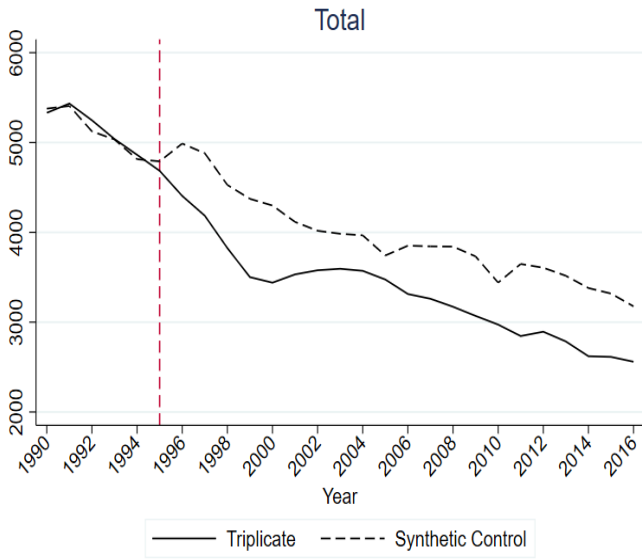


(c) Violent Crime

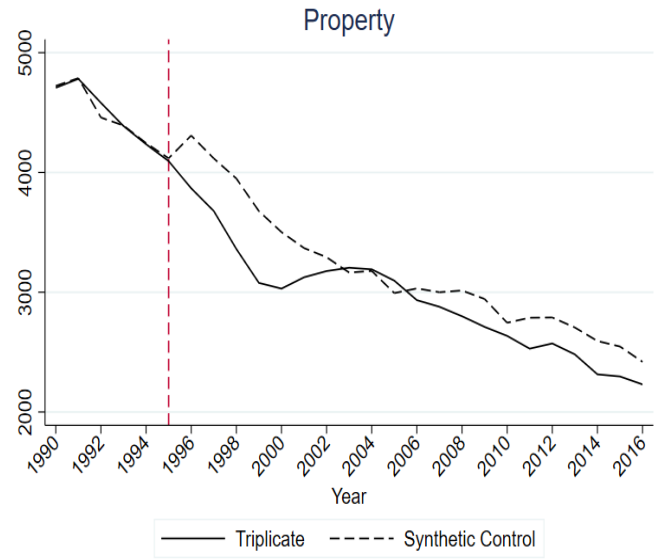
Data Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.



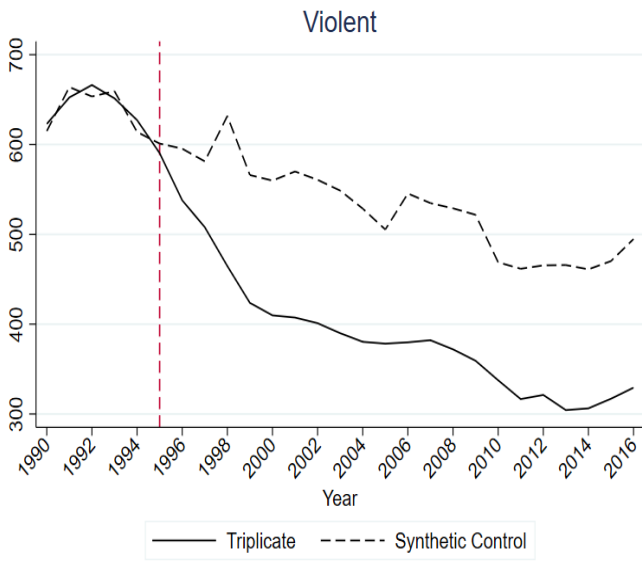
Figure 3: Synthetic Control Estimations



(a) Total Crime



(b) Property Crime

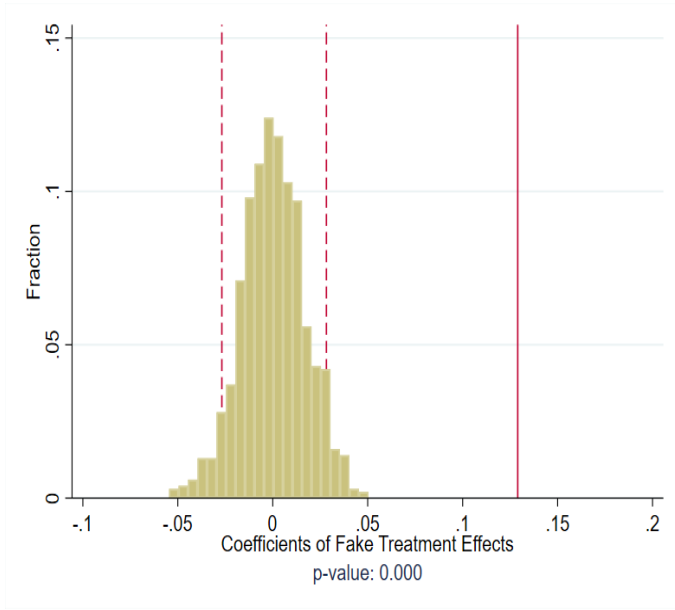


(c) Violent Crime

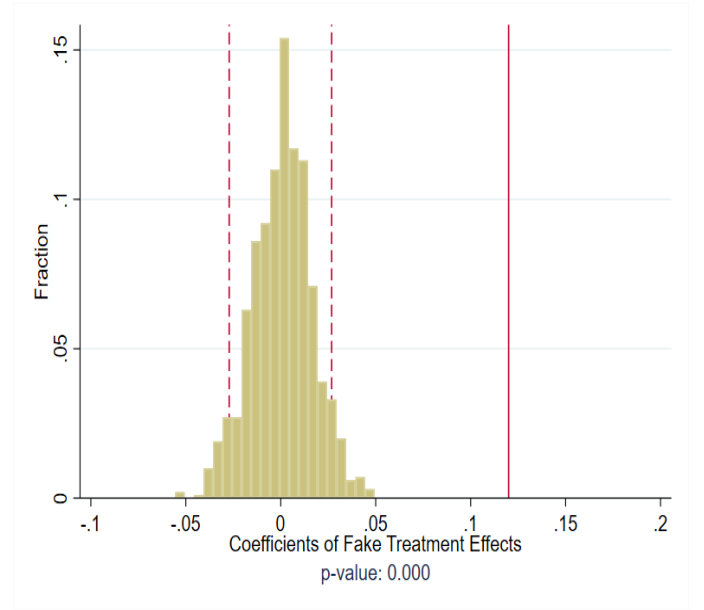
Note: I aggregate triplicate states to one treatment group to conduct the synthetic control estimation.

Data Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.

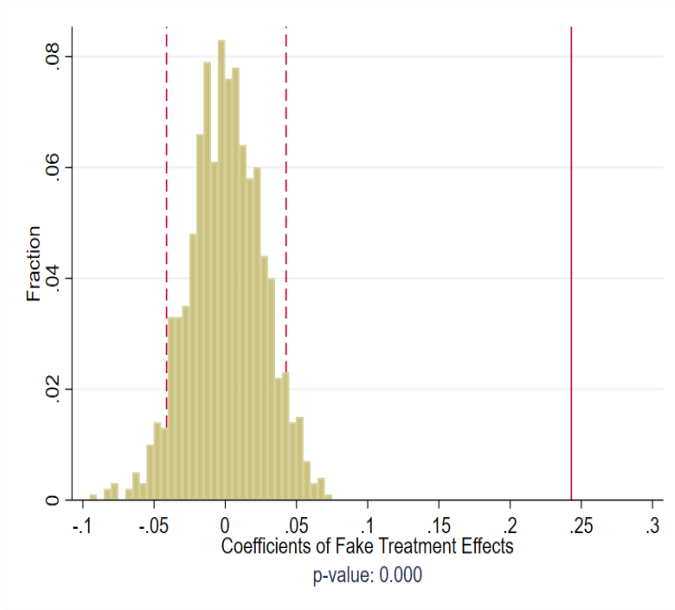
Figure 4: Permutation Tests



(a) Total Crime



(b) Property Crime



(c) Violent Crime

Note: I restrict the sample to non-triplicate states only and estimate the permutation test using Equation (1). All models are population-weighted. The dashed vertical lines indicate 5th and 95th percentile values of the "fake" treatment effects. A solid line indicates the main estimate from Table 2.1.

Data Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.

Table 1.1: Summary Statistics: Part I Crime Type

	Entire Sample	Triplicate	Non-Triplicate
Total Crime	3587.3 (10077.9)	3893.5 (18535.8)	3478.7 (3956.6)
Property Crime	3241.3 (9119.1)	3495.9 (16730.0)	3151.0 (3650.7)
Violent Crime	346.0 (1054.2)	397.7 (1899.3)	327.7 (473.8)
Murder	3.897 (16.69)	4.396 (27.34)	3.720 (10.60)
Rape	27.22 (45.50)	27.20 (56.15)	27.22 (41.07)
Robbery	70.89 (482.0)	93.74 (914.6)	62.78 (133.0)
Assault	244.2 (607.7)	272.5 (1007.3)	234.2 (374.2)
Burglary	683.4 (1228.8)	745.9 (2075.9)	661.3 (717.5)
Larceny	2296.4 (5600.0)	2352.3 (9835.8)	2276.5 (2856.8)
MV Theft	261.5 (2701.3)	397.6 (5204.4)	213.2 (516.8)
Agencies	7325	2048	5277
Observations	170914	44740	126174

Note: Triplicate states include CA, ID, IL, NY, and TX. I restrict sample to agencies that reported all 12 months in every year in the sample period. Each crime is crime per 100,000 residents in a given agency. Standard deviations are in parentheses.

Source: UCR Offenses Known and Clearances by Arrests, 1990 - 2016.

Table 1.2: Summary Statistics: Differences Between Pre-1996 and Post-1996

	<u>Pre-1996</u>			<u>Post-1996</u>		
	Triplicate	Non-Triplicate	Difference	Triplicate	Non-Triplicate	Difference
Total Crime	5115.36 (22782.98)	4169.55 (5089.57)	945.81 [145.36]	3544.15 (17112.39)	3280.68 (3541.60)	263.47 [57.83]
Property Crime	4566.16 (20659.01)	3784.74 (4687.63)	781.42 [132.01]	3189.83 (15409.59)	2969.43 (3271.41)	220.40 [52.22]
Violent Crime	549.23 (2190.24)	384.82 (611.72)	164.41 [14.45]	354.35 (1805.12)	311.23 (424.70)	43.13 [6.2]

Notes: Each crime is crime per 100,000 per residents in a given agency. Standard deviations are in parentheses, and standard errors are in bracket.

Source: UCR Offenses Known and Clearances by Arrests, 1990 - 2016.

Table 1.3: Summary Statistics: Demographic Characteristics

	Total	Triplicate	Non-Triplicate
Per capita Income (\$)	19517.9 (2811.5)	17644.3 (2425.8)	20245.0 (2607.2)
% Male	0.482 (0.010)	0.480 (0.010)	0.483 (0.010)
% Minority	0.336 (0.160)	0.476 (0.149)	0.281 (0.129)
% Age 18 - 25	0.101 (0.010)	0.109 (0.008)	0.0976 (0.009)
% Age 18 - 25 (Male)	0.0493 (0.006)	0.0529 (0.003)	0.0479 (0.006)
% Less than HS	0.164 (0.039)	0.211 (0.033)	0.145 (0.022)
% HS degree	0.226 (0.033)	0.209 (0.034)	0.233 (0.030)
% Some college	0.192 (0.024)	0.182 (0.016)	0.195 (0.025)
% College	0.176 (0.040)	0.147 (0.033)	0.187 (0.036)
Poverty rate	0.134 (0.037)	0.165 (0.034)	0.122 (0.029)
Officer per 100,000	236.9 (58.13)	225.7 (38.29)	241.3 (63.66)

Sources: CPS segment of IPUMS and LEOKA for sworn police officer per 100,000 residents for 1990 - 2016.

Table 2.1: The Effects of OxyContin's launch on Crime - Index Crime

	Total		Property		Violent	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Triplicate	0.129*** (0.036)	0.142*** (0.040)	0.119*** (0.036)	0.145*** (0.043)	0.246*** (0.047)	0.131*** (0.033)
Cluster-robust p	0.000	0.001	0.000	0.001	0.000	0.000
Wild bootstrap p	0.021	0.012	0.023	0.010	0.004	0.016
R-squared	0.793	0.798	0.779	0.785	0.708	0.714
Linear Trends		YES		YES		YES
Observations	170911	170911	170911	170911	170911	170911

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . I report p-values obtained from using wild cluster bootstrap with a 6-point weight distribution suggested by Webb (2014). Dependent variable is logarithmically transformed. Non-Triplicate is a binary variable that indicates whether a state had triplicate prescription program at the time of OxyContin launch in 1996. All specifications include control variables: income per capita, share of minority, individual aged between 18 and 25, males, males aged between 18 and 25, and residents whose highest educational attainment is a college degree, some college, high school, and less than high school. I also include unemployment rate, minimum wage, poverty rate, the number of sworn officers, TANF/SNAP availability for drug-related felonies, PDMPs, medical marijuana laws, and beer tax. All models include agency and year fixed effects, and are weighted by the relevant agency population size.

Table 2.2: The Effects of OxyContin's launch on Crime - By Crime Type

<b>A. Violent Crime</b>								
	Robbery		Assault		Rape		Murder	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-Triplicate	0.190*** (0.064)	0.152*** (0.046)	0.244*** (0.048)	0.119*** (0.037)	0.141* (0.081)	0.152** (0.070)	0.173*** (0.039)	0.0765*** (0.037)
Cluster-robust p	0.005	0.002	0.000	0.002	0.087	0.035	0.000	0.041
Wild bootstrap p	0.022	0.033	0.004	0.032	0.144	0.037	0.002	0.151
<b>B. Property Crime</b>								
	Burglary		Larceny		MV Theft			
	(1)	(2)	(3)	(4)	(5)	(6)		
Non-Triplicate	0.133*** (0.041)	0.124*** (0.044)	0.105*** (0.039)	0.139*** (0.044)	0.140* (0.083)	0.289*** (0.047)		
Cluster-robust p	0.002	0.007	0.009	0.003	0.100	0.000		
Wild bootstrap p	0.013	0.020	0.040	0.065	0.160	0.005		
Linear Trends	YES		YES		YES		YES	
Observations	170804	170804	170804	170804	170804	170804	170804	170804

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Dependent variable is logarithmically transformed. I report p-values obtained from using wild cluster bootstrap with a 6-point weight distribution suggested by Webb (2014). Non-Triplicate is a binary variable that indicates whether a state had triplicate prescription program at the time of OxyContin launch in 1996. All specifications include control variables: income per capita, share of minority, individual aged between 18 and 25, males, males aged between 18 and 25, and residents whose highest educational attainment is a college degree, some college, high school, and less than high school. I also include unemployment rate, minimum wage, poverty rate, the number of sworn officers, TANF/SNAP availability for drug-related felonies, PDMPs, medical marijuana laws, and beer tax. All models include agency and year fixed effects

Table 3: Heterogeneous Effects of OxyContin on Crime by Race

	Total		Property		Violent		Drug	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>A. White</b>								
Non-Triplicate	0.030 (0.051)	0.045 (0.045)	0.082 (0.062)	0.109* (0.056)	0.000 (0.069)	-0.003 (0.059)	-0.030 (0.072)	-0.093 (0.076)
Cluster-robust p	0.565	0.323	0.198	0.061	0.995	0.965	0.684	0.232
Wild bootstrap p	0.592	0.384	0.283	0.132	0.994	0.974	0.729	0.334
<b>B. Black</b>								
Non-Triplicate	0.174* (0.088)	0.174*** (0.063)	0.165** (0.080)	0.197*** (0.053)	0.188** (0.076)	0.126* (0.066)	0.171 (0.111)	0.127 (0.111)
Cluster-robust p	0.056	0.010	0.047	0.001	0.019	0.066	0.135	0.264
Wild bootstrap p	0.095	0.037	0.095	0.023	0.055	0.152	0.193	0.355
Linear Trends		YES		YES		YES		YES
Observations	26026	26026	26026	26026	26026	26026	26026	26026

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Dependent variable is logarithmically transformed. I report p-values obtained from using wild cluster bootstrap with a 6-point weight distribution suggested by Webb (2014). Drug-related arrest consists of drug possessing and drug selling. All specifications include control variables: income per capita, share of minority, individual aged between 18 and 25, males, males aged between 18 and 25, and residents whose highest educational attainment is a college degree, some college, high school, and less than high school. I also include unemployment rate, minimum wage, poverty rate, the number of sworn officers, TANF/SNAP availability for drug-related felonies, PDMPs, medical marijuana laws, and beer tax. All models include agency and year fixed effects, and are weighted by the relevant agency population size

Source: UCR the Arrest, 1990 - 2016.



Table 4.1: Synthetic Control: Pre-trend Comparison in Crime

<b><u>Total Crime</u></b>			
Year	Triplicate	Synthetic	Non-Triplicate
1990	5330.318	5377.283	4212.119
1991	5433.906	5407.286	4328.43
1992	5247.541	5124.213	4176.771
1993	5038.967	5033.96	4061.808
1994	4863.085	4814.27	4099.713
1995	4685.125	4790.393	4138.481
<b><u>Property Crime</u></b>			
Year	Triplicate	Synthetic	Non-Triplicate
1990	4707.426	4722.582	3844.081
1991	4781.534	4787.341	3945.881
1992	4581.281	4458.643	3786.866
1993	4387.879	4391.749	3668.962
1994	4235.899	4245.352	3707.308
1995	4094.653	4118.371	3755.386
<b><u>Violent Crime</u></b>			
Year	Triplicate	Synthetic	Non-Triplicate
1990	622.892	614.955	368.039
1991	652.373	663.919	382.549
1992	666.260	653.412	389.906
1993	651.335	659.387	392.934
1994	627.186	613.945	392.406
1995	590.471	601.061	383.095

Notes: Triplicate states include CA, ID, IL, NY, and TX. Synthetic indicates the synthetic triplicate state. Non-triplicate means the rest of states that has never had triplicate prescription programs. Year ends in 1995 since OxyContin was introduced to the market in 1996.

Table 4.2: Synthetic Control: State Weights - Total Crime

State	Synthetic Triplicate	State	Synthetic Triplicate
Alabama	0.286	Nebraska	0
Alaska	0	Nevada	0
Arizona	0	New Hampshire	0.112
Arkansas	0	New Jersey	0
Colorado	0	New Mexico	0
Connecticut	0	North Carolina	0
Delaware	0.043	North Dakota	0.114
District of Columbia	0	Ohio	0
Florida	0	Oklahoma	0
Georgia	0	Oregon	0
Hawaii	0	Pennsylvania	0
Indiana	0	Rhode Island	0.132
Kansas	0	South Carolina	0
Kentucky	0	South Dakota	0
Louisiana	0.223	Tennessee	0
Maine	0	Utah	0
Maryland	0	Virginia	0
Massachusetts	0.058	Washington	0
Michigan	0	West Virginia	0.032
Minnesota	0	Wisconsin	0
Mississippi	0	Wyoming	0
Missouri	0		

Note: I aggregated triplicate states into the one treated state to conduct the synthetic control exercise.

Table 4.3: Synthetic Control: State Weights - Property Crime

State	Synthetic Triplicate	State	Synthetic Triplicate
Alabama	0	Nebraska	0
Alaska	0	Nevada	0
Arizona	0	New Hampshire	0.063
Arkansas	0	New Jersey	0.105
Colorado	0	New Mexico	0.329
Connecticut	0	North Carolina	0
Delaware	0.041	North Dakota	0.116
District of Columbia	0	Ohio	0
Florida	0.213	Oklahoma	0
Georgia	0	Oregon	0
Hawaii	0	Pennsylvania	0.022
Indiana	0	Rhode Island	0.12
Kansas	0	South Carolina	0
Kentucky	0	South Dakota	0
Louisiana	0.187	Tennessee	0
Maine	0	Utah	0
Maryland	0	Virginia	0
Massachusetts	0	Washington	0
Michigan	0	West Virginia	0.239
Minnesota	0	Wisconsin	0
Mississippi	0	Wyoming	0
Missouri	0		

Note: I aggregated triplicate states into the one treated state to conduct the synthetic control exercise.

Table 4.4: Synthetic Control: State Weights - Violent Crime

State	Synthetic Triplicate	State	Synthetic Triplicate
Alabama	0.078	Nebraska	0
Alaska	0	Nevada	0
Arizona	0	New Hampshire	0
Arkansas	0	New Jersey	0.105
Colorado	0	New Mexico	0.329
Connecticut	0	North Carolina	0
Delaware	0.135	North Dakota	0.108
District of Columbia	0	Ohio	0
Florida	0	Oklahoma	0
Georgia	0	Oregon	0
Hawaii	0	Pennsylvania	0.017
Indiana	0	Rhode Island	0
Kansas	0	South Carolina	0
Kentucky	0	South Dakota	0
Louisiana	0.159	Tennessee	0
Maine	0	Utah	0.007
Maryland	0	Virginia	0
Massachusetts	0	Washington	0
Michigan	0	West Virginia	0
Minnesota	0	Wisconsin	0
Mississippi	0.062	Wyoming	0
Missouri	0		

Note: I aggregated triplicate states into the one treated state to conduct the synthetic control exercise.

Table 5-1: Robustness Check - Individual State Effects

Drop	AL	AK	AZ	AR	CA	CO	CT
Total	0.127*** (0.037)	0.129*** (0.036)	0.131*** (0.037)	0.125*** (0.037)	0.104*** (0.035)	0.131*** (0.037)	0.138*** (0.036)
Property	0.117*** (0.036)	0.120*** (0.036)	0.122*** (0.036)	0.117*** (0.037)	0.0969*** (0.035)	0.122*** (0.038)	0.129*** (0.036)
Violent	0.250*** (0.047)	0.245*** (0.048)	0.249*** (0.048)	0.235*** (0.049)	0.209*** (0.047)	0.247*** (0.048)	0.249*** (0.049)
N	169699	170508	169564	167619	158440	168133	168465

Drop	DE	DC	FL	GA	HI	ID	IL
Total	0.123*** (0.035)	0.129*** (0.036)	0.131*** (0.036)	0.124*** (0.039)	0.130*** (0.036)	0.124*** (0.036)	0.127*** (0.036)
Property	0.114*** (0.035)	0.120*** (0.036)	0.122*** (0.036)	0.114*** (0.039)	0.121*** (0.036)	0.113*** (0.035)	0.118*** (0.036)
Violent	0.240*** (0.047)	0.247*** (0.048)	0.249*** (0.048)	0.239*** (0.051)	0.245*** (0.048)	0.255*** (0.049)	0.243*** (0.047)
N	170076	170887	169077	164482	170833	168672	170805

Drop	IN	KS	KY	LA	ME	MD	MA
Total	0.129*** (0.036)	0.129*** (0.036)	0.129*** (0.036)	0.129*** (0.036)	0.128*** (0.035)	0.126*** (0.036)	0.135*** (0.036)
Property	0.119*** (0.036)	0.120*** (0.036)	0.119*** (0.036)	0.119*** (0.036)	0.119*** (0.035)	0.117*** (0.036)	0.125*** (0.037)
Violent	0.247*** (0.047)	0.246*** (0.047)	0.246*** (0.047)	0.249*** (0.048)	0.246*** (0.047)	0.243*** (0.048)	0.251*** (0.048)
N	169348	170887	170764	169509	167809	168551	167350

Drop	MI	MN	MS	MO	NE	NV	NH
Total	0.138*** (0.038)	0.128*** (0.037)	0.129*** (0.036)	0.125*** (0.03)	0.131*** (0.036)	0.129*** (0.036)	0.129*** (0.036)
Property	0.130*** (0.038)	0.119*** (0.037)	0.119*** (0.036)	0.115*** (0.037)	0.122*** (0.0363)	0.119*** (0.036)	0.120*** (0.036)
Violent	0.242*** (0.05)	0.240*** (0.047)	0.248*** (0.047)	0.243*** (0.049)	0.239*** (0.047)	0.245*** (0.048)	0.246*** (0.047)
N	162669	164792	170158	166515	168512	170401	170887

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Dependent variable is logarithmically transformed of total, property, and violent crimes, respectively. Results are obtained by using Equation (1). Coefficients indicate the impacts of OxyContin on each index crime. P-values from wild cluster bootstrap are not included in this table for space.

Table 5-2: Robustness Check - Individual State Effects

Drop	NJ	NM	NY	NC	ND	OH	OK
Total	0.152*** (0.030)	0.128*** (0.036)	0.134*** (0.038)	0.126*** (0.036)	0.131*** (0.036)	0.128*** (0.037)	0.127*** (0.037)
Property	0.142*** (0.031)	0.119*** (0.036)	0.122*** (0.038)	0.117*** (0.036)	0.121*** (0.036)	0.119*** (0.036)	0.117*** (0.037)
Violent	0.278*** (0.041)	0.244*** (0.047)	0.254*** (0.053)	0.244*** (0.047)	0.247*** (0.0476)	0.250*** (0.048)	0.245*** (0.049)
N	157479	170212	161814	164109	170076	167268	164244

Drop	OR	PA	RI	SC	SD	TN	TX
Total	0.124*** (0.037)	0.119*** (0.037)	0.130*** (0.036)	0.127*** (0.037)	0.131*** (0.036)	0.134*** (0.035)	0.153*** (0.038)
Property	0.114*** (0.037)	0.112*** (0.038)	0.120*** (0.036)	0.117*** (0.037)	0.121*** (0.036)	0.124*** (0.035)	0.142*** (0.038)
Violent	0.242*** (0.048)	0.224*** (0.047)	0.247*** (0.048)	0.247*** (0.047)	0.247*** (0.0472)	0.250*** (0.046)	0.272*** (0.049)
N	168052	157162	169914	166377	170482	167758	151554

Drop	UT	VA	WA	WV	WI	WY
Total	0.129*** (0.036)	0.126*** (0.037)	0.124*** (0.036)	0.125*** (0.036)	0.129*** (0.036)	0.128*** (0.036)
Property	0.120*** (0.036)	0.117*** (0.037)	0.114*** (0.036)	0.117*** (0.036)	0.120*** (0.036)	0.118*** (0.036)
Violent	0.246*** (0.047)	0.244*** (0.049)	0.248*** (0.049)	0.231*** (0.047)	0.246*** (0.048)	0.245*** (0.048)
N	168970	165568	167026	169096	170833	169536

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Dependent variable is logarithmically transformed of total, property, and violent crimes, respectively. Results are obtained by using Equation (1). Coefficients indicate the impacts of OxyContin on each index crime. P-values from wild cluster bootstrap are not included in this table for space.

Table 5.3: Robustness Check - Other Specifications

	Total			
	(1)	(2)	(3)	(4)
Non-Triplicate	0.129*** (0.036)	0.142*** (0.04)	0.101*** (0.026)	0.117 (0.063)
Cluster-robust p	0.000	0.001	0.000	0.108
Wild bootstrap p	0.021	0.012	0.017	0.290
	Property			
	(1)	(2)	(3)	(4)
Non-Triplicate	0.119*** (0.036)	0.145*** (0.043)	0.105*** (0.029)	0.104 (0.061)
Cluster-robust p	0.023	0.010	0.001	0.134
Wild bootstrap p	0.021	0.012	0.021	0.327
	Violent			
	(1)	(2)	(3)	(4)
Non-Triplicate	0.246*** (0.047)	0.131*** (0.033)	0.071** (0.029)	0.242** (0.096)
Cluster-robust p	0.000	0.000	0.020	0.040
Wild bootstrap p	0.004	0.016	0.056	0.165
Linear Trends		YES	YES	
Quadratic Trends			YES	
Large States Only				YES
90's Covs x Trend				
Observations	170911	170911	170911	68517

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Dependent variable is logarithmically transformed. Statistical significance denoted by \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . I report p-values obtained from using wild cluster bootstrap with a 6-point weight distribution suggested by Webb (2014). Non-Triplicate is a binary variable that indicates whether a state had triplicate prescription program at the time of OxyContin launch in 1996. All specifications include the same control variables and fixed effects used in the main analysis. The subsample for large states only exercise includes FL, PA, OH, and MI along with the four triplicate states except ID.

# Appendix A

## A. Appendix Figures

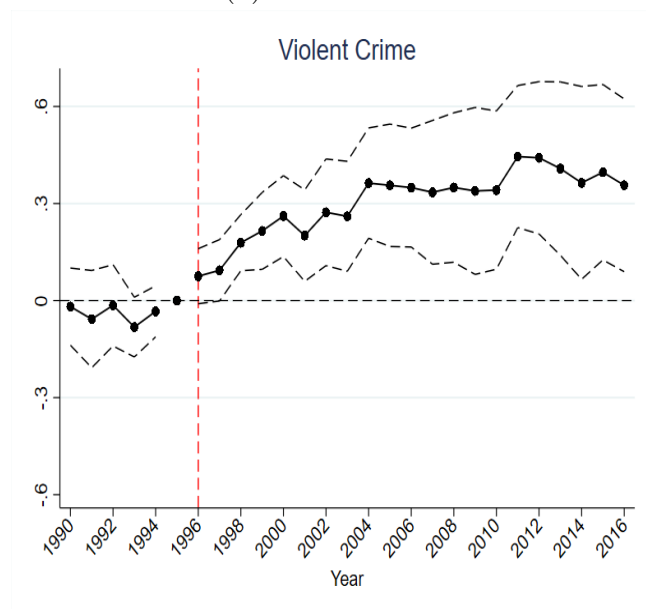
Figure A.1: Event-Study Estimates: Unweighted Version



(a) Total Crime



(b) Property Crime

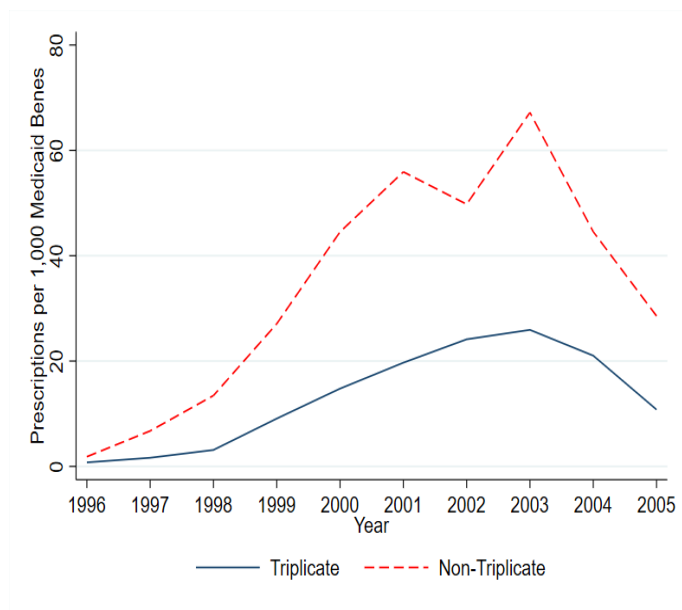


(c) Violent Crime

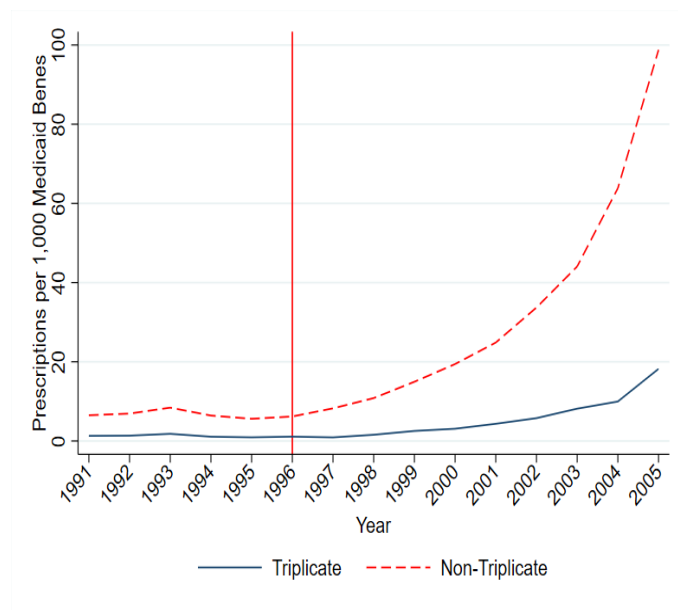
Data Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.



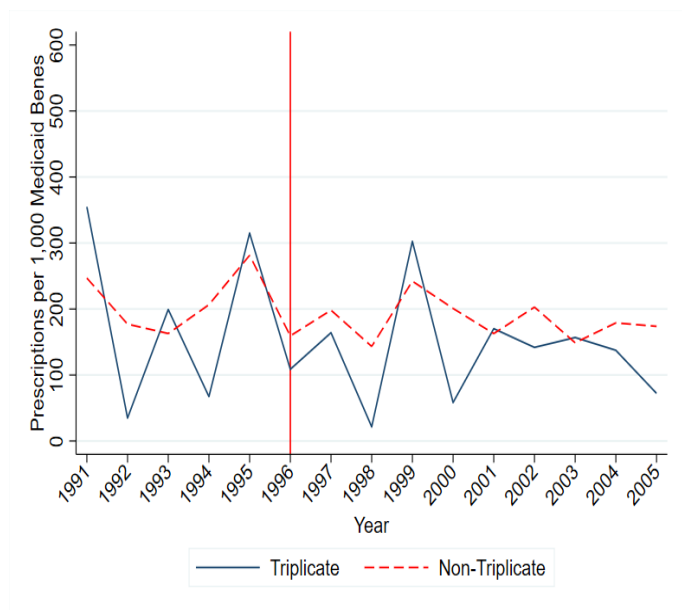
Figure A.2: Potential Mechanisms - the Distribution of Opioid Drugs



(a) OxyContin



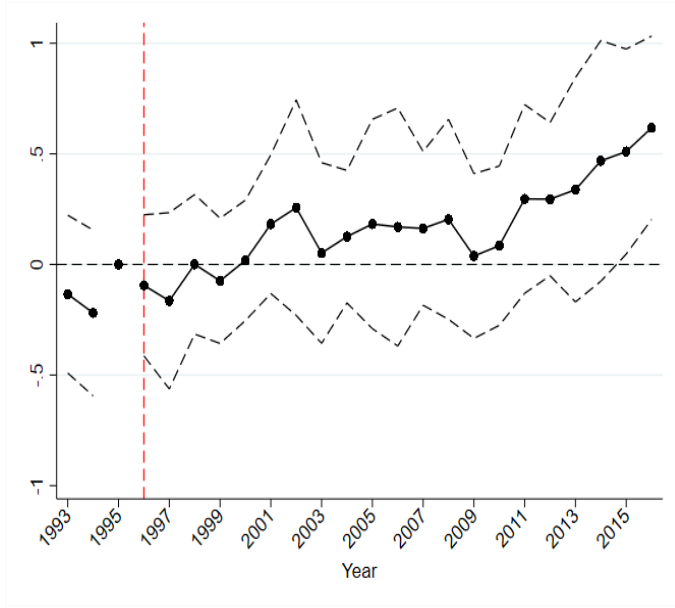
(b) Oxycodone



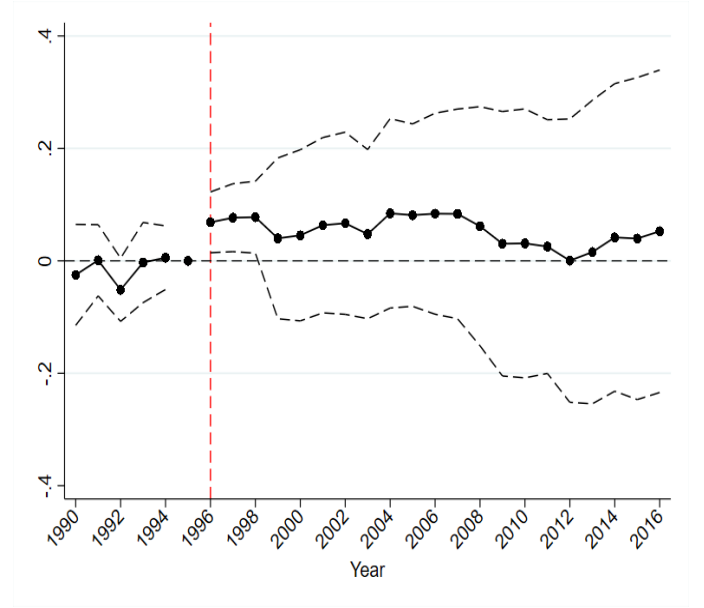
(c) Hydrocodone

Notes: I report the number of prescriptions per 1,000 Medicaid beneficiaries using SDUD and UKCPR for 1991-2005. Data Sources: State Drug Utilization Data (SDUD) and University of Kentucky Center for Poverty Research (UKCPR) from 1991 - 2005.

Figure A.3: Potential Mechanisms - Event Study Analysis



(a) Mental Health



(b) Alcohol Consumption

Notes: Both panels present the event-study estimates using the Equation (2), which include state and year fixed effects, state demographic characteristics, macro economic variables, and beer tax. Panel (a) shows trend in the average number of days that respondents did have mental illness during 30 days prior to the interview. Panel (b) presents trend in alcohol consumption per capita. The sample year in the left panel starts from 1993 instead of 1990 because it is only available back to 1993.

Data Sources: For panel a, Behavioral Risk Factor Surveillance System (BRFSS). For panel b, the National Institute on Alcohol Abuse and Alcoholism (NIAAA) downloaded from ICPSR.

## A. Appendix Tables

Table A.1: Drug Scheduling

Schedule	Drugs Name
I	heroin, lysergic acid diethylamide (LSD), marijuana (cannabis), methylenedioxymethamphetamine (ecstasy), methaqualone, peyote
II	oxycodone (OxyContin), cocaine, methadone, methamphetamine, hydromorphone (Dilaudid), meperidine (Demerol), fentanyl, hydrocodone combination products (Vicodin)
III	anabolic steroids, testosterone, ketamine, Tylenol (with less than 90 milligrams of codeine per dosage unit)
IV	Xanax, Soma, Ativan, Talwin, Ambien, Tramadol, Valium, Darvocet
V	Lomotil, Motofen, Lyrica, Parepectolin, Robitussin AC (with less than 200 milligrams of codeine)

Note that hydrocodone combination drugs were a schedule III drug at the time of OxyContin's introduction. They were reclassified as schedule II drugs in 2014. To see other names of controlled substances not stated in this table, go to <https://www.deadiversion.usdoj.gov/schedules/index.html>

Source: The U.S. Drug Enforcement Administration.

Table A.2: The Main Analysis - Unweighted Version

	Total		Property		Violent	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Triplicate	0.139*** (0.035)	0.144*** (0.029)	0.128*** (0.036)	0.145*** (0.030)	0.302*** (0.058)	0.163*** (0.043)
Cluster-robust p	0.000	0.000	0.001	0.000	0.000	0.001
Wild bootstrap p	0.019	0.013	0.022	0.014	0.005	0.028
R-squared	0.142	0.148	0.132	0.138	0.147	0.153
Linear Trends		YES		YES		YES
Observations	170911	170911	170911	170911	170911	170911

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Dependent variable is logarithmically transformed. Statistical significance denoted by \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . I report p-values obtained from using wild cluster bootstrap with a 6-point weight distribution suggested by Webb (2014). Non-Triplicate is a binary variable that indicates whether a state had triplicate prescription program at the time of OxyContin launch in 1996. All specifications include control variables: income per capita, share of minority, individual aged between 18 and 25, males, males aged between 18 and 25, and residents whose highest educational attainment is a college degree, some college, high school, and less than high school. I also include unemployment rate, minimum wage, poverty rate, the number of sworn officers, TANF/SNAP availability for drug-related felonies, PDMPs, medical marijuana laws, and beer tax. All models include agency and year fixed effects.

Table A.3: Potential Mechanisms - IV Estimation

	(1) First Stage	(2) IV	(3) IV	(4) IV
Non-Triplicate	44.376*** (9.579)	—	—	—
Opioid Prescription	—	0.003*** (0.001)	0.003*** (0.001)	0.005*** (0.002)
F statistic first stage	21.46	—	—	—
R-squared (overall)	—	0.113	0.104	0.135
Observations	91939	91939	91939	91939

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Outcome variable for Column 1 is the number of prescription opioid drugs per 1,000 Medicaid beneficiaries. For Columns 2-4, outcome variables are Total, Property, and Violent crimes, respectively and they are logarithmically transformed. Non-Triplicate is a binary variable that indicates whether a state had triplicate prescription program at the time of OxyContin launch in 1996. All specifications include the same control variables and fixed effects used in the main analysis. Column 1 present the first stage estimate of triplicate status on the distribution of prescription opioid drugs per 1,000 Medicaid beneficiaries. Columns 2-4 indicates the IV estimates on total, property, and violent crime outcomes where opioid prescription per state is instrumented with triplicate status. The baseline sample period is from 1991 to 2005 due to the data availability on drug prescriptions.

## B. Sensitivity Check for the Event-Study Analysis

In this section, I conduct the sensitivity check for the event-study analysis under different assumptions on how informative pre-treatment difference in trends predict counterfactual post-treatment difference in trends. I conducted an event-study analysis to assess the parallel trend between triplicate- and non-triplicate states under the assumption that pre-treatment difference in trends can predict counterfactual post-treatment difference in trends. However, pre-treatment difference in trends may not serve as an accurate indicator of post-treatment difference in trends. For instance, after 1996, some shocks may affect the crime rate in non-triplicate or triplicate states, creating different crime trends. Consequently, the main DD estimates should be cautiously interpreted as a causal effect even though the event-study analysis reveals no pre-existing trends. To overcome this possible issue, I exploited a novel estimation approach to provide robust confidence sets of the DD estimate developed by Rambachan and Roth (2020). Their methodology allows the researcher to obtain a valid confidence interval for the causal effect even if the parallel trend assumption does not hold exactly. The implication of this approach is to test how robust the DD estimate is to the violation of the parallel trend assumption.<sup>35</sup> For example, pre-treatment difference in trend can be assumed to persist over the time horizon. Consequently, the difference in trends can be linearly extrapolated for the post-period counterfactual difference in trends. Furthermore, we can even assume that the slope of differential trends after treatment may evolve non-linearly over consecutive time-periods as long as the degree of deviation from the linearity is not too much.

Appendix Figure B1 depicts sensitivity checks for the treatment effects on violent and property crimes three years after OxyContin was introduced. The original DD estimate, with the 95% confidence intervals (CI), is in blue (from Equation (2)). Following Rambachan and Roth (2020),

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<sup>35</sup> Rambachan and Roth (2020) decomposed the DD estimate as causal effects of interest and difference in trends between the two groups that would exist absent treatment. They suggested that the researcher needs to impose certain possible restrictions on the difference in trends between consecutive periods to conduct sensitivity analysis for DD and event-study designs. Following are the proposed restrictions on differential trends: smoothness, shape, sign, and polyhedral restrictions. They claimed that uniformly valid inference can be obtained when such restrictions are satisfied.

I plot the robust confidence intervals in red. ‘M’ is the degree of non-linearity of the slope representing the differential trend over consecutive time-periods.<sup>36</sup> Panel A shows that when the slope of difference in trends is approximately linear (at  $M = 0$ ), the robust confidence sets (or robust CIs) for violent crime are similar to the original OLS CIs. However, the robust CIs widens with increasing non-linearity; they begin to include zero when  $M$  exceeds 0.004.<sup>37</sup> This indicates that the main estimate is statistically significant if we assume that the degree of change of the slope representing differential trends does not exceed 0.4% between consecutive periods. Following Rambachan and Roth (2020), I construct a 95% CI for the largest change in slopes of differential trends between consecutive periods using pre-periods to evaluate the breakdown value of  $M$ . The CI for the largest change in slope of differential trends in the pre-periods is  $[0, 0.154]$ . Therefore, if we are willing to assume that the slope of differential trends in the post-treatment periods cannot change by more than the largest value of change observed in the pre-period, we can reject a null effect in 1998.

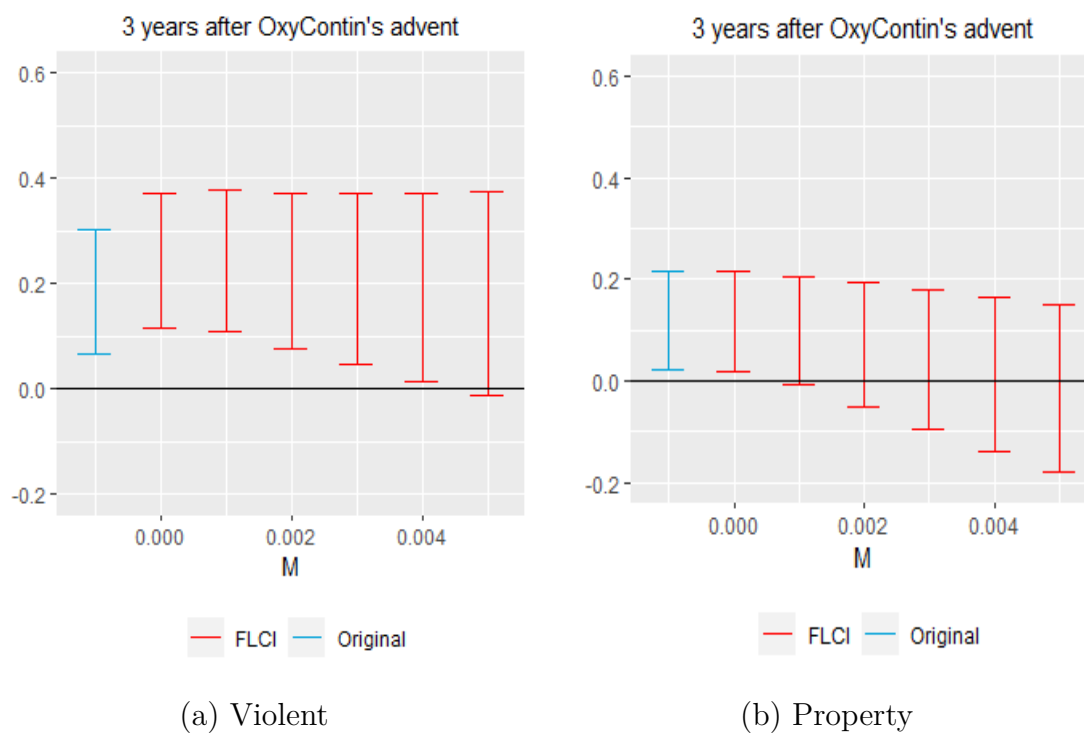
The robust confidence sets are similar to the original CIs at  $M = 0$  for property crime (Panel B). The figure depicts that we can reject the null treatment effect in 1998 if we restrict the alteration of the slope of the difference in trends by no more than 0.001. A 95% CI for the largest change in slope of differential trends between consecutive periods using the pre-periods is  $[0, 0.081]$ . Therefore, a null effect in 1998 can be rejected unless we are willing to assume that the slope of differential trends after treatment (OxyContin’s introduction) is greater than the largest change in slopes between periods before treatment. In the main text, the event-study analysis shows that there is pre-trend for property crime. Nevertheless, this sensitivity check suggests that the DD estimate in 1998 for property crime can be valid for the causal effect if the true difference in post-treatment trends is less than the value of 0.001.

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36. In Rambachan and Roth (2020),  $M$  is defined as an upper bound on the degree of change of the slope of difference in trends between consecutive periods can change.

37. In Rambachan and Roth (2020), the largest value of  $M$  such that the main effect is still statistically significant is called the “breakdown” value of  $M$ .

Figure B.1: Sensitivity Check for the Event-Study Analysis



Notes: Confidence intervals in blue for both violent and property crimes are from Figure 2.

Data Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.