# The Effect of Opioids on Crime: Evidence from the Introduction Of OxyContin

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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 increased use of opioid drugs. This study estimates the effects of the opioid epidemic on crime relying for identification on geographic variation in the distribution of OxyContin, which in turn was driven by initial state drug prescription policies. Using Uniform Crime Reports (UCR) data, I find that compared to states with stringent prescription policies, states more exposed to OxyContin had 25% higher violent crime rates and 12% higher property crime rates. Thus, the supply shock of opioids combined with loose policies on prescription drugs created unintended and negative consequences beyond health and mortality. This conclusion is supported by suggestive evidence on mechanisms of mental health conditions, alcohol abuse, and illegal drug markets.

**Keywords:** Crime, Oxycontin, Opioids.
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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 in 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)—a more than 200% increase over 16 years. Recent studies have suggested that the epidemic was facilitated by a combination of liberalized medical practices dealing with patients' pain in the 1990s and aggressive marketing by a pharmaceutical firm, Purdue Pharma. Convinced by Purdue and other manufacturers that pain had not been treated sufficiently in the past and encouraged by marketing incentives, physicians started aggressively prescribing opium-based drugs. This led to 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 to expand 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; Arteaga and Barone 2021), labor market outcomes (Krueger 2017; Aliprantis et al. 2019; Harris et al. 2020; Park and Powell 2021), and child well-being (Buckles et al. 2020). However, the consequences of this epidemic on crime remain relatively unknown with two working papers considering prescription drug monitoring policies (Mallat 2018; Dave et al. 2020). Because of the high social costs of crime, especially violent crime, this is a crucial omission in the literature.

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. I follow Alpert et al. (2019) in relying on a state-level prescription policy called the triplicate prescription program to identify the cross-state variation in the supply of OxyContin. "Triplicate" programs were intended to prevent the diversion of controlled substances such as opioid drugs by requiring multiple copies when prescribing Schedule II drugs,[1] one of which was filed with the state to allow monitoring of prescribing behavior. 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 cross-state variation in the degree of exposure to OxyContin. Additionally, over time the gap between triplicate and non-triplicate states grew as Purdue targeted marketing promotions to less regulated jurisdictions. 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 non-triplicate states at the time of OxyContin's introduction experienced a relative rise in both property (12%) and violent (25%) crimes compared to states with the triplicate prescription policy (triplicate states). The largest effects for property crime are concentrated among the first five years after OxyContin entered the market (until 2000). Non-triplicate states experienced a persistent rise in violent crime 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). Alpert et al. (2019) presented that non-triplicate states have a higher proportion of whites than that of triplicate states. Furthermore, using reformulation of OxyContin as an abuse-deterrent version in 2010, Alpert et al. (2018) found that drug-related (heroin) death rates are concentrated among white Americans in states more

[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.

exposed to initial rate of OxyContin misuse. Race information is not available for Offenses Known, but it is available in the UCR arrest data. With the knowledge that these two series have different biases, I investigate racial patterns. I do not find differential patterns in crime arrests committed of 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 OxyContin on crime, I instrument for the number of opioid (OxyContin and oxycodone) 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 prescribed more often in non-triplicate states by 44 per 1,000 Medicaid beneficiaries after the introduction of OxyContin. 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 DID estimates.

To investigate these findings further, I conduct a series of checks of the sensitivity of results to alternative samples and placebo-type tests. Because of pre-trend differences across states, I also estimate synthetic control models. In addition, I perform the event-study analysis under different assumptions on pre-treatment difference in trends using a recently developed econometric technique by Rambachan and Roth (2020). Together, these alternative specifications provide confidence that the interpretation of a significant divergence in crime trends occurred due to the introduction of OxyContin.

The existing studies have shown that chronic drug use can affect crime through various channels. For instance, the demand for OxyContin itself could have become a motive of criminal behavior. Felson and Staff (2017) revealed that 30% of property offenders and 27% of drug offenders committed property crime to generate income to purchase drugs.[2] In addition to the financial motive, the expanding market for the prescription opioid drugs could have generated the illegal drug market, driving up the prevalence of violent crimes. Empirical evidence suggests that drug users may consume illegal drugs such as heroin as a substitute for prescription opioid drugs (Alpert et al. 2018; Mallat 2018). Further, it is known that gangs are systematically involved with the illegal drug distribution (Block and Block 1993; Levitt and Venkatesh 2000) and that the nature of the illegal market with the existence of gangs is associated with a rise in violence (Miron 1999; Levitt and Rubio 2005). I consider two additional 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), finding 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 (Markowitz and Grossman 2000; Carpenter and Dobkin 2008; Markowitz 2005; Heaton 2012; Cook and Durrance 2013; Anderson et al. 2017; Hansen and Waddell 2018). The evidence on the increase in the consumption of alcohol in non-triplicate states after the introduction of OxyContin in 1996 is plausible, but the data are very noisy.

Note that 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. Rather, this paper adds empirical evidence to the extant literature on the effects of stringent prescription monitoring programs on opioid misuse and other social outcomes (Ali et al. 2017; Buchmueller and Carey 2018; Mallat 2018; Grecu et al. 2019; Wen et al. 2019; Dave, Deza, and Horn 2020).[3] My findings demonstrate

[2] Although this paper focused on illegal drugs such as heroin and cocaine, the relationship can be extended to prescription opioid drugs given that heroine itself is an opioid made from morphine and has similar effects to opioids.

[3] These papers studied the effects of more recent prescription drug monitoring programs known as PDMPs

that OxyContin's introduction played a role in increasing crime rates in states without stringent policies on prescription drugs.

## 2 Background

In 1996, Purdue Pharma introduced a new product to the market—OxyContin, 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 to aggressively market and promote their new product.[4] 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 was less than one percent and it was not subject to abuse because of its 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 states such as Kentucky, Maine, Ohio, Pennsylvania, Virginia, and West Virginia (GAO 2003). Several local and state governments filed lawsuits against Purdue Pharma for the false advertisement and

on social outcomes. "Triplicate" programs have much in common with PDMPs in the sense that it was intended to track and monitor controlled substance prescriptions.

[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.

overpromotion.[5]

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 had demonstrated a willingness to prescribe OxyContin. Purdue Pharma targeted 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 lobbied to eliminate the prescription regulation but their primary focus was to promote OxyContin in non-triplicate states (Alpert et al., 2019).

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 one copy for their records for years, and the other two copies were given 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

[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.

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.[6].

Triplicate prescription programs were initially implemented in California in 1939 due to the increasing diversion of opioid drugs at that time (Simoni-Wastila and Tolder, 2001). Since then, several states have followed California's model, 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 were purposely exposed to a greater availability of OxyContin than were individuals in a state with triplicate program. They showed that the distribution of OxyContin was on average 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 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 as the regulatory environment set the initial conditions for the opioid epidemics. The gap in the distribution of OxyContin widened even after 2004 rather than narrowing down (Alpert et al., 2019).

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

# 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. For the primary analysis of this study, I use the Offenses Known data. This data source presents the most commonly reported (index) crimes across the country that can be divided into property-related and violent crimes. Specifically, there are seven 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 jurisdictions 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 modeled per 100,000 residents in a given agency's jurisdiction.[7]

A limitation of using the Offenses Known data is that they do not provide demographic information on offenders and victims. In addition, drug-related crimes are not collected, hindering the investigation of 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. It is widely understood that drug arrests reflect enforcement priorities, so care must be taken in

[7] Note that not all policing agencies are recorded as having a population, though they provide crime reports. According to Maltz and Targonski (2002), jurisdictions are assigned zero-population when policing jurisdictions overlap. Crime rates in this study does not include crimes reported by such jurisdictions by construction.

interpreting these results. Note also that cocaine and heroin arrests are aggregated, which prevents me from evaluating the effects of OxyContin's introduction on opioid-related arrests.[8] 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) from 1991–2005 for the number of oxycodone and OxyContin prescriptions per state.[9] 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.[10] 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.[11] Although the Medicaid population is not representative of the general population that could be affected by OxyContin, it is considered a good proxy for those who are disproportionately affected by the opioid crisis (e.g., Centers for Disease Control and Prevention 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, including the poverty rate, the share of minorities, the share of individuals aged between 18 and 25 years, males, share of males aged between 18 and 25, and share of individuals' at four levels of educational attainment.[12] In addition, I collected information on the unemployment

[8] 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.

[9] Medicaid State Drug Utilization Data is available back to 1991.

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

[11] 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.

[12] Educational attainment is categorized into: less than high-school degree, high-school graduates, some

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.[13] 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.[14] 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.[15]

## 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 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:

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

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.[16] 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. Post$_t$ is an indicator variable that turns to the

college degree, and college graduates.

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

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

[15]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/.

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

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 unoberved 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 account for 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 an over-rejection problem. To address this concern, I also report p-values from 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:

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

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.[17] In contrast, they find no clear negative results of the opioid crisis on

[17] In the work on "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.

African Americans and Hispanics. The variation in the degree of the impacts of the opioid crisis among ethnic/racial groups suggests 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 separately for whites and blacks (the only groups that have consistent data availability).

In an alternate approach to the main analysis, I estimate synthetic controls to account for the small number of states which operated the triplicate prescription program. In this practice, I aggregate triplicate states into a single treatment unit following Abadie et al. (2010).

For a final check on the robustness of the findings, I perform a permutation test suggested by 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. I, then, estimate the effects of treatment by using the random status and the model in Equation (1), and repeat this procedure 1,000 times. Then, I create a distribution of the fake treatment effects to which I can compare 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, accounting 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 fell 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. For both 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 experienced increase in property crime by 12% relative to triplicate states since OxyContin entered the market. Column 3-4 presents that non-triplicate states experiences 25% increase in violent crime (13% when the state-specific linear trend is added) relative to their counterpart states; both estimates are statistically significant at the 1% level.

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; the estimate for rape is statistically significant at the 10% level with the clustered-robust standard errors, but the statistical significance disappears 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).[18]

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.[19] Nevertheless, Table 3 pro-

[18] 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 treatment. Doleac and Mukherjee (2019) find that states with naloxone access laws experienced increases in opioid-related theft and arrests for possessions and sales of opioid by 30%, 17% and 27%, respectively. Packham (2019) suggests that drug-related arrests (by 16%) and local rates of theft (by 24%) rise after opening syringe exchange programs (created to reduce HIV transmission). In a recent working paper, Dave et al. (2020) shows that having PDMPs (especially mandatory access ones) are associated with declines in total crime of 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%.

[19] In addition to decline in sample size, nineteen states were removed from sample including Idaho. To ensure whether my results are valid even when the removed states are included, I run the same analysis with an 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

vides 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, Panel B indicates that non-triplicate states experienced increases in property- and violent-related arrests among Black relative to their counterparts. 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 diverts between property and violent crimes over years.

Panel A of Figure 2 suggests that non-triplicate states experienced persistent and significant increases in violent crime before decreasing in the last three years. The pre-OxyContin effects are near-zero and statistically insignificant for violent crime. The F-statistic for the joint hypothesis that the whole lead years have null effect on violent crime is 1.83 and corresponding p-value is 0.125. These evidence may indicate that there is no pre-existing trend in violent crime.

On the other hand, Panel B shows that the largest effects for property crime are

non-triplicate states by 25% relative to triplicate states. Except for drug-related arrests, 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.

concentrated on the 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. The F-statistic for the lead years is 4.16 and corresponding p-value is 0.003, indicating that the estimates on these years are significantly different from zero. Thus, the DID estimate for property crime should be cautiously interpreted as a causal effect.

### 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 before 1996 slightly differed between triplicate and non-triplicate states. Related to this concern, Figure 2 suggests that there might exist some upward pre-trends in property crime in non-triplicate states before OxyContin was introduced to the market. In addition, before 1996, the baseline levels of crime rates are higher in triplicate states than in non-triplicate states. All of these evidence imply that non-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. 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 and consider the non-triplicate states as potential donor states.

The synthetic control procedure creates a suitable control group by specifying a weighted average of non-triplicate states to resemble the characteristics of triplicate states before 1996. Under this framework, any subsquent divergence in crime rates between triplicate states and the synthetic triplicate states is interpreted as due to the introduction of OxyContin. Table 4.1 displays how similar the 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 the full set of non-triplicate states. Tables 4.2-4.3 present the calculated weights that are assigned to each non-triplicate states among donor pools for constructing the synthetic triplicate states. Note that not every state is assigned weights, which may indicate that the synthetic triplicate approach may provide superior estimates of the treatment effect.

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 main results are not driven by the pre-trends in crime outcomes (and the difference in crime rates at the baseline levels), and my main results are robust.

### 5.5 Structural Effects of OxyContin on Crime

Using SDUD data, I analyze the structural effects of OxyContin's introduction on crime. First, I explore whether the distribution of OxyContin prescriptions truly differed between triplicate and non-triplicate states. Panel A of Figure 4 presents the distribution of OxyContin between the two groups.[20] 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 to other prescription opioids. Specifically, I examine the pattern of oxycodone prescriptions among the Medicaid population before and after 1996.[21] If the introduction of OxyContin made physicians comfortable with prescribing oxycodone combination drugs, the disparity in the distribu-

[20] 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.

[21] Oxycodone is a pain killer medication that contains opioid and is also classified as Schedule II.

tion of oxycodone may appear since 1996.[22] Indeed, Panel B of Figure 4 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).[23]

To better understand the figures and structural effects of OxyContin on crime, I use IV estimation approach using the following equations,

$$Prescription_{st} = \pi_0 + \pi_1 Non\text{-}Triplicate_s * Post_t + \pi_2 X'_{st} + \theta_a + \delta_t + \xi_{st} \quad \text{(1st Stage)}$$

$$Crime_{ast} = \beta_0 + \beta_1 Prescriptions_{st} + \beta_2 X'_{st} + \theta_a + \delta_t + \eta_{ast} \quad \text{(2nd Stage)}$$

where $Prescriptions_{st}$ indicates the number of opioid prescriptions per 1,000 Medicaid beneficiaries. In this exercise, I use an interaction term between triplicate status and a post 1996 indicator as a instrument for $Prescriptions$.[24] Drugs that contain opioids but do not fall under schedule II drugs are not included.[25] In the second stage, I use the same dependent variable as used in the main analysis. $\pi_1$ measures the effects of OxyContin's introduction on the number of opioid prescriptions per 1,000 Medicaid beneficiaries, while $\beta_1$ represents the effect of an additional prescription for opioid drugs per 1,000 beneficiaries on crime rates.

Column 1 of Table 5 presents the first stage estimate of triplicate status on the num-

[22] OxyContin is a specific brand name for a pain reliever that contains the time-release version of oxycodone.

[23] 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.

[24] 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.

[25] One concern can be that prescribing patterns for other drugs that are not under schedule II and thus not subject to triplicate prescription programs might also differ between the two groups for the period of this study. However, Alpert et al. (2019) show that schedule III drug distribution trends, such as hydrocodone drugs, were almost identical between the two sets of groups before and after 1996 using ARCOS data. Using Medicaid data, Panel C of Figure 4 in this paper also displays the prescribing pattern for hydrocodone drugs that is in line with the corresponding figure in Alpert et al. (2019).

ber of opioid-drug prescriptions per 1,000 Medicaid beneficiaries. It is revealed that non-triplicate states at the time of OxyContin's introduction record more prescription opioid drugs by 44 per 1,000 beneficiaries than triplicate states. The estimate is statistically significant, and its F-statistic is 21.46 which is greater than the conventional number for the relevance requirement. The mean opioid prescription per 1,000 Medicaid beneficiaries is 36. Hence, an increase of 44 prescriptions in non-triplicate states represents an 122% increase over the mean prescriptions per 1,000 beneficiaries. Column 2 shows that property crime increased by 0.3% with an additional increase in opioids per 1,000 Medicaid beneficiaries. For violent crime (Column 3), an additional prescription for opioid drugs is associated with a rise in violent crime by 0.5%. Estimates for both crimes are statistically significant. Overall, combining with the first stage result, non-triplicate states experienced a 13.2% increase in property crime relative to triplicate states since the 1996 introduction of OxyContin. In addition, non-triplicate states experienced a 22% increase in violent crime compared to triplicate states. These IV estimates correspond with the DID estimates for both property and violent crime.

### 5.6 Robustness Checks

In this section, I present a number of sensitivity checks, including placebo-type analysis, to verify whether my main results are robust to alternate specifications. In addition, I conduct the sensitivity check for the event-study analysis under different assumptions on pre-treatment difference in trends following Rambachan and Roth (2020).

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 a relative rise in crime. To test this possibility, I replicate Table 2.1, dropping each state at a time. The results, shown in Tables 6.1-6.2, are qualitatively similar to the main analysis across this exercise, though some states have stronger effects on the estimates in either direction. 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. 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 prescription opioid use and criminal behavior across states through different economic and labor market conditions.[26,27] To address this issue, I add quadratic trends to the main analysis following the suggestion of Neumark et al. (2014). Column 3 of Table 6.3 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 the same as the main analysis (12%), while the estimated effects on violent crime shrink 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 the 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 a 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.[28] In column 4 of Table 6.3, the size of estimates is qualitatively similar to that of main results across all types of crime. However, the estimate for property crime is no longer statistically significant due to the increased noise caused by the reduced sample size. The test results may imply that the relative decline in crime outcomes in triplicate states is 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 addition to the different population size, one concern is that there might exist overlapping jurisdictions which could generate biased estimates. To test this alternative

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

[27]Carpenter et al. (2017) found a strong counter-cyclical relationship between economic conditions and prescription analgesics disorders including opioids.

[28]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: 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.

hypothesis, I replicate the main DID analysis by dropping all agencies within a county where at least one jurisdiction is assigned zero-population.[29] With this further restriction, twenty-seven states remain in the sample with only two of them as being triplicate states, ID and TX. Column 5 of Table 6.3 reveals that the estimates for both crime types are broadly similar with the main results. Moreover, despite the noise caused by the smaller sample size, the estimates remain statistically significant at the 5% level.

In the main DID estimations, the standard errors are clustered at the state level to account for the intra-state correlation and corresponding biased standard errors. Table 6.4 compares the estimates of standard errors obtained from three different models: the conventional ordinary least squares (OLS), clustering at the state level, and estimation from grouped means at the state level. It is revealed that coefficients for both crime types are statistically significant across the board, though standard errors from the OLS are much smaller than the other two estimates. This result shows that clustering at the state level provides more conservative standard errors, leading to robust inference. As discussed in Abadie et al. (2010) and Buchmueller et al. (2011), however, it 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.[30] 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 the 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

[29]When more than one jurisdiction covers an area in a given county, the UCR assigns a population to one of them to avoid double-counting. I use this fact as a proxy for identifying agencies whose coverage might overlap with others. To be more conservative, I also dropped any jurisdiction whose coverage is more than one county.

[30]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, California, in studying the causal effects of tobacco policy on tobacco consumption.

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 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 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 to 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 the figures are presented in Appendix Figure 1. Again, trends from unweighted models are similar to the ones from weighted models (Figure 2).

Finally, I also perform the sensitivity check for the event-study analyses using a novel estimation approach developed by Rambachan and Roth (2020). Under 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 three potential channels through which the widespread prescription opioid drugs might affect crime indirectly.

First, 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 Prescription Drug Monitoring Programs (PDMP) is associated with an increase in illegal drug deaths (Meinhofer, 2018). PDMP is a state-level policy intervention intended to curb overprescribing opioid drugs and adverse drug-related consequences; it collects database about prescription and dispensation of controlled substances. 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 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 in violent crime such as murders and non-fatal shootings with handguns (Maher and Dixon, 2001; Miron, 1999; Levitt and Rubio, 2005). Another possibility is that opioid addiction can instigate violent behaviors to generate income to sustain the addiction. Using a nationally representative sample of prison inmates, Felson and Staff (2017) suggested that heroin and cocaine addicts might engage in illegitimate behaviors to secure income to purchase drugs.

I consider two additional potential channels through which OxyContin might have impacted crime. 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,[31] one cannot ignore cases where opioid addicts might display violent tendencies, particularly during withdrawal.[32] 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.

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

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

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 non-triplicate states using Behavioral Risk Factor Surveillance System (BRFSS) data.[33] Panel A of Appendix Figure 3 reveals the average number of days that individuals experienced mental health problems during the 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 may indicate 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.

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

[33]I used the BRFSS data for 1993-2016 as the survey inquired about mental health in 1993.

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 (Markowitz and Grossman 2000; Carpenter and Dobkin 2008; Heaton, 2012; Cook and Durrance 2013; Hansen and Waddell, 2018).

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.[34] As shown in Panel B of Appendix Figure 3, alcohol consumption increased in non-triplicate states immediately since 1996. Although point estimates are very 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.

# 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

[34]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.

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. 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.[35]

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 substance 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.

[35]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).

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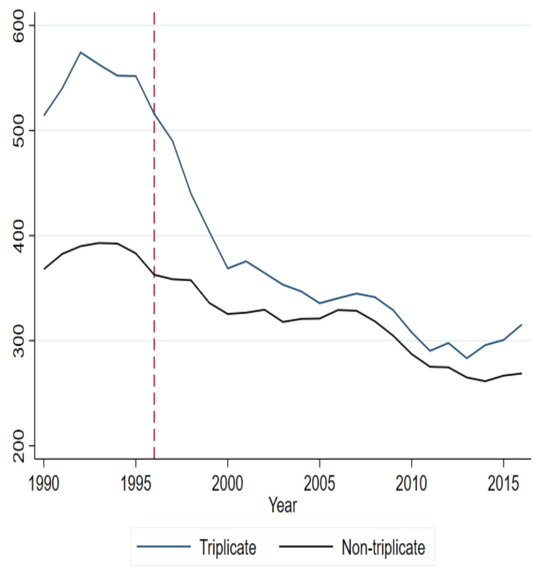
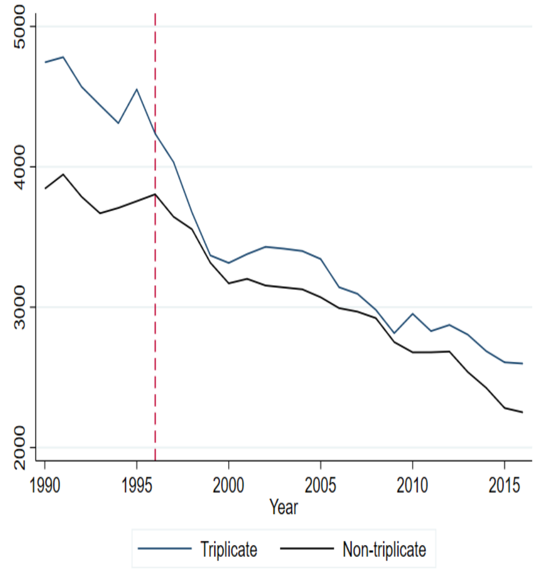
**Figure 1.** The Trends of Crime Rates




(a) Violent Crime

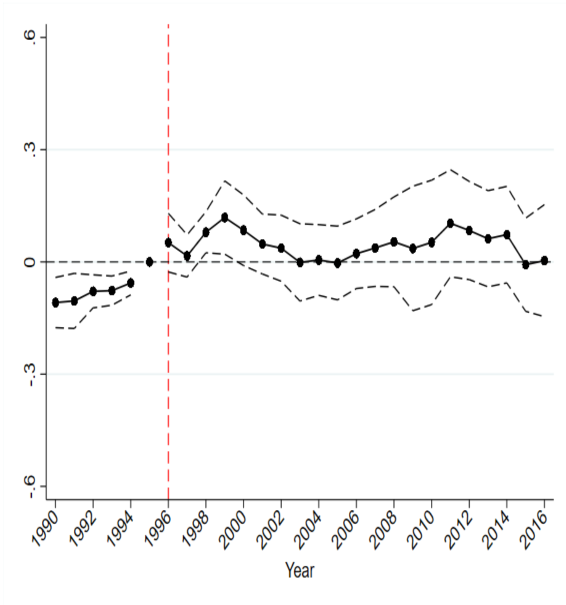



(b) Property Crime

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

**Figure 2.** Event-Study Analysis

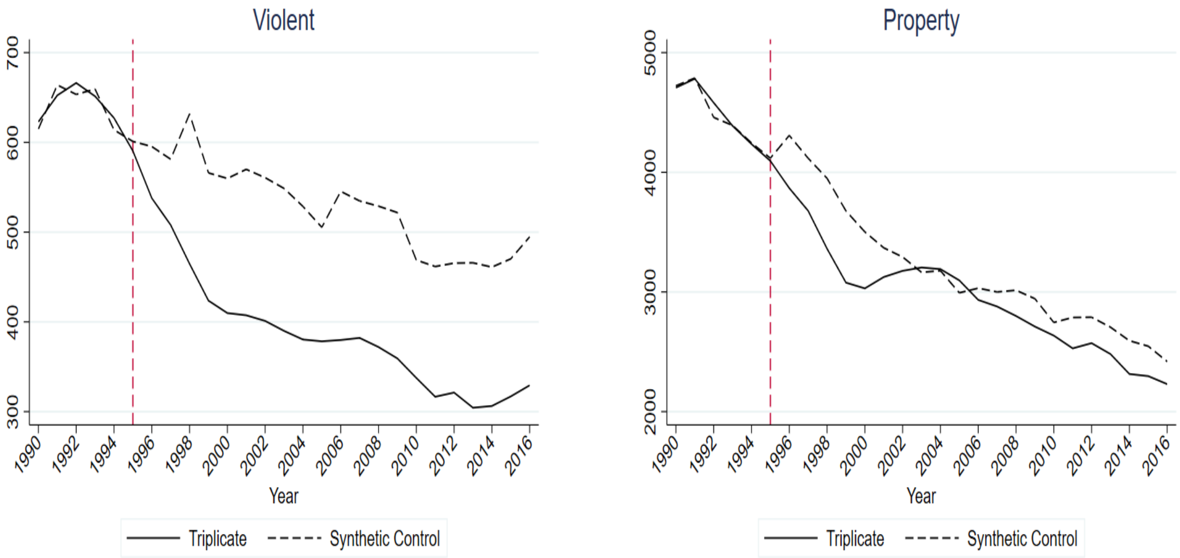

(a) Violent Crime




(b) Property Crime

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

**Figure 3.** Synthetic Control Estimations

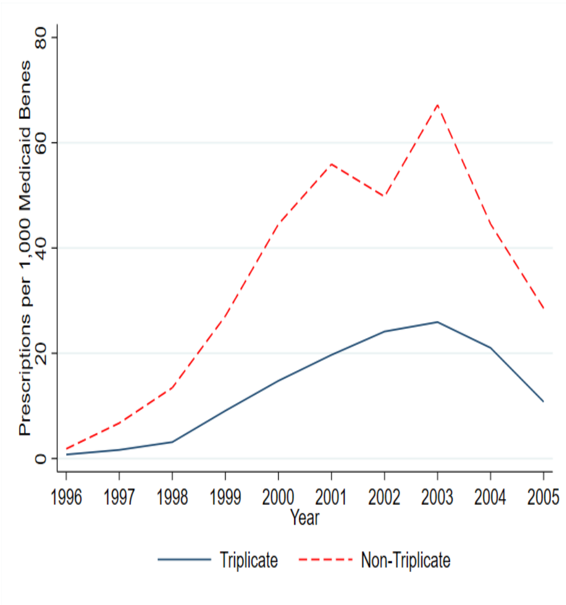



(a) Violent Crime

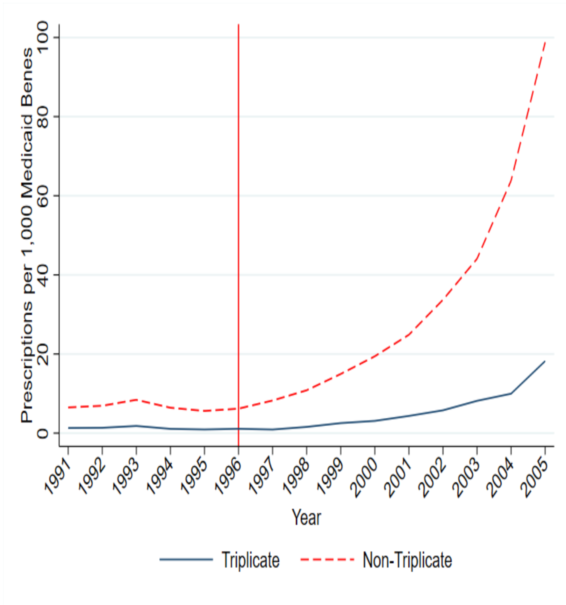
(b) Property Crime

Note: I aggregate triplicate states to one treatment group to conduct the synthetic control estimation.
Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.

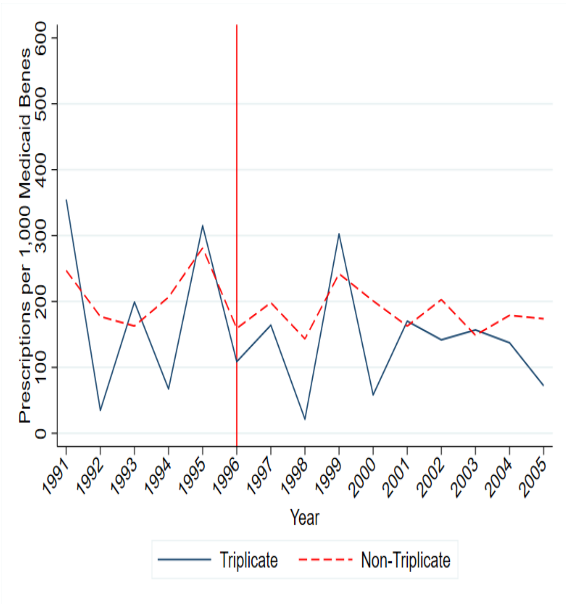
**Figure 4.** The Distribution of Opioid Drugs




(a) OxyContin



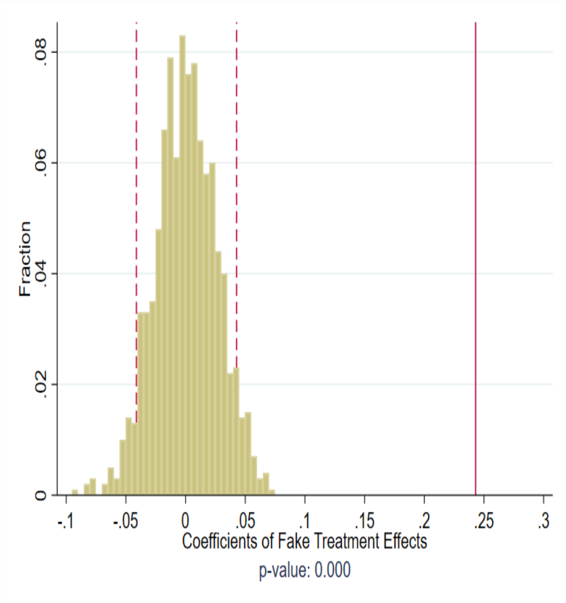

(b) Oxycodone



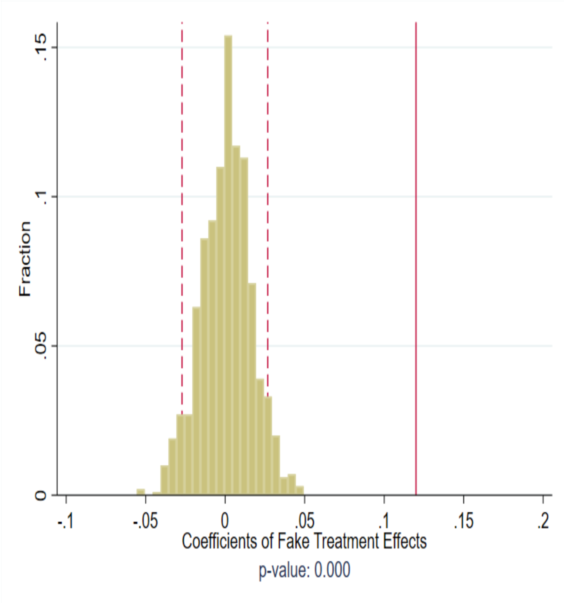

(c) Hydrocodone

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

**Figure 5.** Permutation Test




(a) Violent Crime




(b) Property 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.
Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.

Table 1.1: Summary Statistics by Crime Type

| (Per 100,000) | Entire Sample | Triplicate | Non-Triplicate |
|---|---|---|---|
| Total Crime | 3587.3 | 3893.5 | 3478.7 |
| | (10077.9) | (18535.8) | (3956.6) |
| Property Crime | 3241.3 | 3495.9 | 3151.0 |
| | (9119.1) | (16730.0) | (3650.7) |
| Violent Crime | 346.0 | 397.7 | 327.7 |
| | (1054.2) | (1899.3) | (473.8) |
| Murder | 3.897 | 4.396 | 3.720 |
| | (16.69) | (27.34) | (10.60) |
| Rape | 27.22 | 27.20 | 27.22 |
| | (45.50) | (56.15) | (41.07) |
| Robbery | 70.89 | 93.74 | 62.78 |
| | (482.0) | (914.6) | (133.0) |
| Assault | 244.2 | 272.5 | 234.2 |
| | (607.7) | (1007.3) | (374.2) |
| Burglary | 683.4 | 745.9 | 661.3 |
| | (1228.8) | (2075.9) | (717.5) |
| Larceny | 2296.4 | 2352.3 | 2276.5 |
| | (5600.0) | (9835.8) | (2856.8) |
| MV Theft | 261.5 | 397.6 | 213.2 |
| | (2701.3) | (5204.4) | (516.8) |
| Agencies | 7,325 | 2,048 | 5,277 |
| Observations | 170,914 | 44,740 | 126,174 |

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. Total crime is the sum of property and violent crimes. 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

| | Pre-1996 | | | Post-1996 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Triplicate | Non-Triplicate | Diff | Triplicate | Non-Triplicate | Diff |
| Property | 4566.16 | 3784.74 | 781.42 | 3189.83 | 2969.43 | 220.40 |
| | (20659.01) | (4687.63) | [132.01] | (15409.59) | (3271.41) | [52.22] |
| Violent | 549.23 | 384.82 | 164.41 | 354.35 | 311.23 | 43.13 |
| | (2190.24) | (611.72) | [14.45] | (1805.12) | (424.70) | [6.2] |

Notes: Each crime is crime per 100,000 per residents in a given agency. Diff stands for difference in each crime outcome between the two groups. 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 | 17644.3 | 20245.0 |
| | (2811.5) | (2425.8) | (2607.2) |
| % Male | 0.482 | 0.480 | 0.483 |
| | (0.01) | (0.01) | (0.01) |
| % Minority | 0.336 | 0.476 | 0.281 |
| | (0.16) | (0.15) | (0.13) |
| % Age 18 - 25 | 0.101 | 0.109 | 0.098 |
| | (0.01) | (0.01) | (0.01) |
| % Age 18 - 25 (Male) | 0.049 | 0.053 | 0.048 |
| | (0.01) | (0.00) | (0.01) |
| % Less than HS | 0.164 | 0.211 | 0.145 |
| | (0.04) | (0.03) | (0.02) |
| % HS degree | 0.226 | 0.209 | 0.233 |
| | (0.03) | (0.03) | (0.03) |
| % Some college | 0.192 | 0.182 | 0.195 |
| | (0.02) | (0.02) | (0.03) |
| % College | 0.176 | 0.147 | 0.187 |
| | (0.04) | (0.03) | (0.04) |
| Poverty rate | 0.134 | 0.165 | 0.122 |
| | (0.037) | (0.03) | (0.03) |
| Officer per 100,000 | 236.9 | 225.7 | 241.3 |
| | (58.13) | (38.29) | (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 Introduction on Crime

| | Property | | Violent | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Non-Triplicate | 0.119*** | 0.145*** | 0.246*** | 0.131*** |
| | (0.036) | (0.043) | (0.047) | (0.033) |
| P-value | 0.000 | 0.001 | 0.000 | 0.000 |
| Wild P-value | 0.023 | 0.010 | 0.004 | 0.016 |
| R-squared | 0.779 | 0.785 | 0.708 | 0.714 |
| Linear Trends | | YES | | YES |
| Observations | 170,911 | 170,911 | 170,911 | 170,911 |

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 cluster-robust p-values and wild cluster bootstrap p-values 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

| **A. Violent** | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Robbery | | Assault | | Rape | | Murder | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Non-Triplicate | 0.190*** | 0.152*** | 0.244*** | 0.119*** | 0.141* | 0.152** | 0.173*** | 0.0765** |
| | (0.064) | (0.046) | (0.048) | (0.037) | (0.081) | (0.070) | (0.039) | (0.037) |
| P-value | 0.005 | 0.002 | 0.000 | 0.002 | 0.087 | 0.035 | 0.000 | 0.041 |
| Wild P-value | 0.022 | 0.033 | 0.004 | 0.032 | 0.144 | 0.037 | 0.002 | 0.151 |
| **B. Property** | | | | | | | | |
| | Burglary | | Larceny | | MV Theft | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | | |
| Non-Triplicate | 0.133*** | 0.124*** | 0.105*** | 0.139*** | 0.140* | 0.289*** | | |
| | (0.041) | (0.044) | (0.039) | (0.044) | (0.083) | (0.047) | | |
| P-value | 0.002 | 0.007 | 0.009 | 0.003 | 0.100 | 0.000 | | |
| Wild P-value | 0.013 | 0.020 | 0.040 | 0.065 | 0.160 | 0.005 | | |
| Linear Trends | | YES | | YES | | YES | | YES |
| Observations | 170,804 | 170,804 | 170,804 | 170,804 | 170,804 | 170804 | 170,804 | 170,804 |

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 cluster-robust p-values and wild cluster bootstrap p-values 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 as shown in Table 2.1. All models include agency and year fixed effects.

Table 3: Heterogeneous Effects of OxyContin on Crime by Race

| | Property | | Violent | | Drug | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **A. White** | | | | | | |
| Non-Triplicate | 0.082 | 0.109* | 0.000 | -0.003 | -0.030 | -0.093 |
| | (0.062) | (0.056) | (0.069) | (0.059) | (0.072) | (0.076) |
| P-value | 0.198 | 0.061 | 0.995 | 0.965 | 0.684 | 0.232 |
| Wild p-value | 0.283 | 0.132 | 0.994 | 0.974 | 0.729 | 0.334 |
| **B. Black** | | | | | | |
| Non-Triplicate | 0.165** | 0.197*** | 0.188** | 0.126* | 0.171 | 0.127 |
| | (0.080) | (0.053) | (0.076) | (0.066) | (0.111) | (0.111) |
| P-value | 0.047 | 0.001 | 0.019 | 0.066 | 0.135 | 0.264 |
| Wild p-value | 0.095 | 0.023 | 0.055 | 0.152 | 0.193 | 0.355 |
| Linear Trends | | YES | | YES | | YES |
| Observations | 26,026 | 26,026 | 26,026 | 26,026 | 26,026 | 26,026 |

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 the same control variables as shown in Table 2.1. All models include agency and year fixed effects.

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

| **Violent Crime** | | | |
|---|---|---|---|
| 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 |
| **Property Crime** | | | |
| 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 |

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 - 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 |
| Louisana | 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 4.3: 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 |
| Louisana | 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: Potential Mechanisms - IV Estimation

| Dependent Variable | Prescription (1) [First Stage] | Property (2) [IV] | Violent (3) [IV] |
|---|---|---|---|
| Non-Triplicate | 44.376*** | – | – |
| | (9.579) | | |
| Opioid Prescription | – | 0.003*** | 0.005*** |
| | | (0.001) | (0.002) |
| Mean Opioid Prescription | 35.54 | – | – |
| F statistic first stage | 21.46 | – | – |
| R-squared (overall) | – | 0.104 | 0.135 |
| Observations | 91,939 | 91,939 | 91,939 |

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 2-3 indicates the IV estimates on property and violent crime outcomes where opioid prescription per state is instrumented with triplicate status. The sample period in this exercise is from 1991 to 2005 due to the data availability on drug prescriptions and the introduction of Medicare Part D.

Table 6.1: Robustness Check - Individual State Effects

| Drop | AL | AK | AZ | AR | CA | CO | CT |
|---|---|---|---|---|---|---|---|
| Property | 0.117*** | 0.120*** | 0.122*** | 0.117*** | 0.0969*** | 0.122*** | 0.129*** |
| | (0.036) | (0.036) | (0.036) | (0.037) | (0.035) | (0.038) | (0.036) |
| Violent | 0.250*** | 0.245*** | 0.249*** | 0.235*** | 0.209*** | 0.247*** | 0.249*** |
| | (0.047) | (0.048) | (0.048) | (0.049) | (0.047) | (0.048) | (0.049) |
| N | 169,699 | 170,508 | 169,564 | 167,619 | 158,440 | 168,133 | 168,465 |

| Drop | DE | DC | FL | GA | HI | ID | IL |
|---|---|---|---|---|---|---|---|
| Property | 0.114*** | 0.120*** | 0.122*** | 0.114*** | 0.121*** | 0.113*** | 0.118*** |
| | (0.035) | (0.036) | (0.036) | (0.039) | (0.036) | (0.035) | (0.036) |
| Violent | 0.240*** | 0.247*** | 0.249*** | 0.239*** | 0.245*** | 0.255*** | 0.243*** |
| | (0.047) | (0.048) | (0.048) | (0.051) | (0.048) | (0.049) | (0.047) |
| N | 170,076 | 170,887 | 169,077 | 164,482 | 170,833 | 168,672 | 170,805 |

| Drop | IN | KS | KY | LA | ME | MD | MA |
|---|---|---|---|---|---|---|---|
| Property | 0.119*** | 0.120*** | 0.119*** | 0.119*** | 0.119*** | 0.117*** | 0.125*** |
| | (0.036) | (0.036) | (0.036) | (0.036) | (0.035) | (0.036) | (0.037) |
| Violent | 0.247*** | 0.246*** | 0.246*** | 0.249*** | 0.246*** | 0.243*** | 0.251*** |
| | (0.047) | (0.047) | (0.047) | (0.048) | (0.047) | (0.048) | (0.048) |
| N | 169,348 | 170,887 | 170,764 | 169,509 | 167,809 | 168,551 | 167,350 |

| Drop | MI | MN | MS | MO | NE | NV | NH |
|---|---|---|---|---|---|---|---|
| Property | 0.130*** | 0.119*** | 0.119*** | 0.115*** | 0.122*** | 0.119*** | 0.120*** |
| | (0.038) | (0.037) | (0.036) | (0.037) | (0.0363) | (0.036) | (0.036) |
| Violent | 0.242*** | 0.240*** | 0.248*** | 0.243*** | 0.239*** | 0.245*** | 0.246*** |
| | (0.05) | (0.047) | (0.047) | (0.049) | (0.047) | (0.048) | (0.047) |
| N | 162,669 | 164,792 | 170,158 | 166,515 | 168,512 | 170,401 | 170,887 |

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Dependent variable is logarithmically transformed of 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 6.2: Robustness Check - Individual State Effects

| Drop | NJ | NM | NY | NC | ND | OH | OK |
|---|---|---|---|---|---|---|---|
| Property | 0.142*** | 0.119*** | 0.122*** | 0.117*** | 0.121*** | 0.119*** | 0.117*** |
| | (0.031) | (0.036) | (0.038) | (0.036) | (0.036) | (0.036) | (0.037) |
| Violent | 0.278*** | 0.244*** | 0.254*** | 0.244*** | 0.247*** | 0.250*** | 0.245*** |
| | (0.041) | (0.047) | (0.053) | (0.047) | (0.0476) | (0.048) | (0.049) |
| N | 157,479 | 170,212 | 161,814 | 164,109 | 170,076 | 167,268 | 164,244 |

| Drop | OR | PA | RI | SC | SD | TN | TX |
|---|---|---|---|---|---|---|---|
| Property | 0.114*** | 0.112*** | 0.120*** | 0.117*** | 0.121*** | 0.124*** | 0.142*** |
| | (0.037) | (0.038) | (0.036) | (0.037) | (0.036) | (0.035) | (0.038) |
| Violent | 0.242*** | 0.224*** | 0.247*** | 0.247*** | 0.247*** | 0.250*** | 0.272*** |
| | (0.048) | (0.047) | (0.048) | (0.047) | (0.0472) | (0.046) | (0.049) |
| N | 168,052 | 157,162 | 169,914 | 166,377 | 170,482 | 167,758 | 151,554 |

| Drop | UT | VA | WA | WV | WI | WY |
|---|---|---|---|---|---|---|
| Property | 0.120*** | 0.117*** | 0.114*** | 0.117*** | 0.120*** | 0.118*** |
| | (0.036) | (0.037) | (0.036) | (0.036) | (0.036) | (0.036) |
| Violent | 0.246*** | 0.244*** | 0.248*** | 0.231*** | 0.246*** | 0.245*** |
| | (0.047) | (0.049) | (0.049) | (0.047) | (0.048) | (0.048) |
| N | 168,970 | 165,568 | 167,026 | 169,096 | 170,833 | 169,536 |

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Dependent variable is logarithmically transformed of 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 6.3: Robustness Check - Other Specifications

| | Baseline Model | State Linear Trend | Quadratic Linear Trend | Large State Only | No Overlapping Jurisdictions |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **A. Violent Crime** | | | | | |
| Non-Triplicate | 0.246*** | 0.131*** | 0.071** | 0.242** | 0.203** |
| | (0.047) | (0.033) | (0.029) | (0.096) | (0.079) |
| P-value | 0.000 | 0.000 | 0.020 | 0.040 | 0.016 |
| Wild bootstrap p | 0.004 | 0.016 | 0.056 | 0.165 | 0.185 |
| **B. Property Crime** | | | | | |
| Non-Triplicate | 0.119*** | 0.145*** | 0.105*** | 0.104 | 0.139** |
| | (0.036) | (0.043) | (0.029) | (0.061) | (0.052) |
| P-value | 0.023 | 0.010 | 0.001 | 0.134 | 0.013 |
| Wild bootstrap p | 0.021 | 0.012 | 0.021 | 0.327 | 0.128 |
| Observations | 170,911 | 170,911 | 170,911 | 68,517 | 37,012 |

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. Models for Column 2 and 3 are based off of Column 1 with additions as indicated in the headers. For the last two columns, samples are restricted as indicated in the headers. The subsample for large states only exercise includes FL, PA, OH, and MI along with the four triplicate states except ID. For the analysis of non-overlapping jurisdictions, sample contains twenty-seven states with two of them (ID and TX) as a triplicate state group. States included in this analysis are as follows: AL, AK, AZ, AR, CO, CT, GA, HI, ID, LA, MN, MS, MO, NV, NH, NM, NC, ND, OH, OK, RI, SD, TN, TX, UT, WA, WY.

Table 6.4: Robustness Check - Comparing Standard Errors by Models

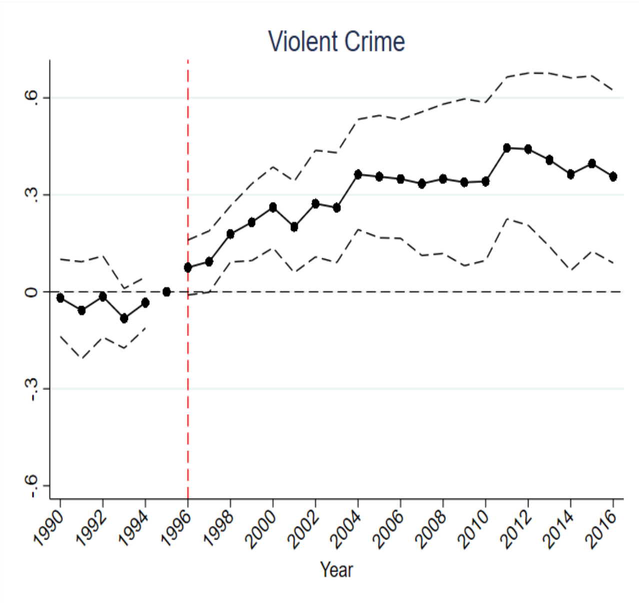
| | Property | | | Violent | | |
|---|---|---|---|---|---|---|
| | Conventional | Clustered | State | Conventional | Clustered | State |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Non-Triplicate | 0.119*** | 0.119*** | 0.144*** | 0.246*** | 0.246*** | 0.302*** |
| | (0.006) | (0.036) | (0.023) | (0.011) | (0.047) | (0.039) |
| P-value | 0.000 | 0.000 | 0.001 | 0.000 | 0.000 | 0.000 |
| R-squared | 0.779 | 0.779 | 0.928 | 0.708 | 0.708 | 0.921 |
| Observations | 170,911 | 170,911 | 1,296 | 170,911 | 170,911 | 1,296 |

Note: Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Conventional indicates that standard errors are estimated using the conventional OLS (Columns 1 and 4). The group level for clustering is the state (Columns 2 and 5). Lastly, Columns 3 and 6 report standard errors from estimation using state means. All models of each crime type use the Equation (1), but the state-level estimations control for state fixed effects rather than agency fixed effects. All specifications include the same control variables used in the main model.

# Appendix A

## A. Appendix Figures

**Figure A.1.** Event-Study Estimate: Unweighted Version

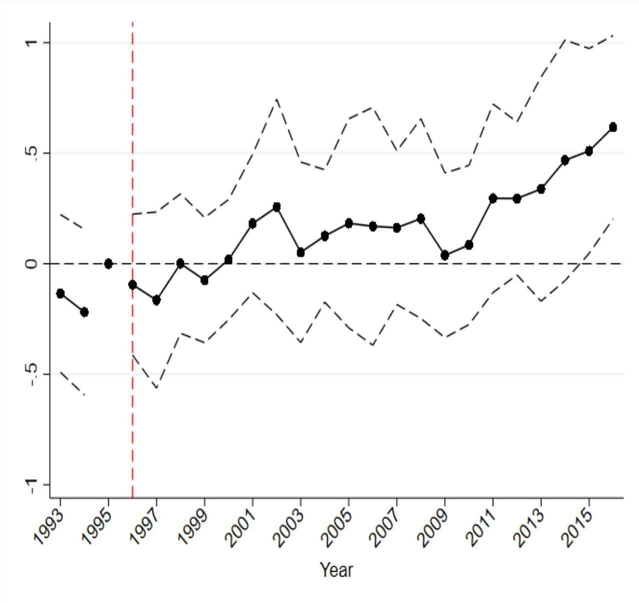



(a) Violent Crime

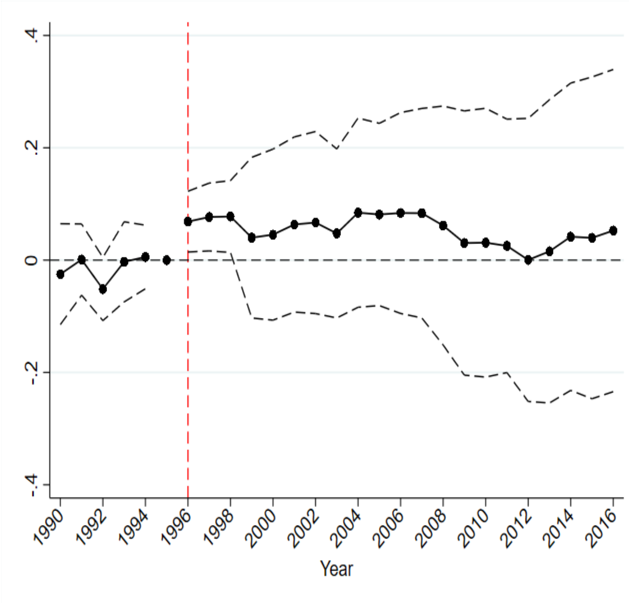

(b) Property Crime

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

**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.
Sources: For panel (a), Behavioral Risk Factor Surveillance System (BRFSS). For panel (b), the National Institute on Alcohol Abuse and Alcoholism (NIAAA) downnloaded 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*** | 0.144*** | 0.128*** | 0.145*** | 0.302*** | 0.163*** |
| | (0.035) | (0.029) | (0.036) | (0.030) | (0.058) | (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 | 170,911 | 170,911 | 170,911 | 170,911 | 170,911 | 170,911 |

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.

# 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 DID 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 DID 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 DID estimate is to the violation of the parallel trend assumption.[36] 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 DID estimate, with the 95% confidence intervals (CI), is in blue (from Equation (2)). Following

[36] Rambachan and Roth (2020) decomposed the DID 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 DID 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.

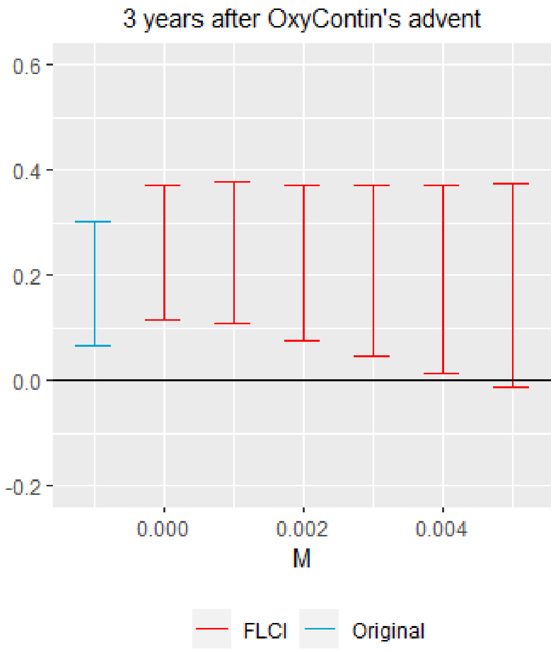
Rambachan and Roth (2020), I plot the robust confidence intervals in red. 'M' is the degree of non-linearity of the slope representing the differential trend over consecutive time-periods.[37] Panel A shows that when the slope of difference in trends is approximately linear (at M = 0), the robust confidence 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.[38] 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. Follwoing 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 DID 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.

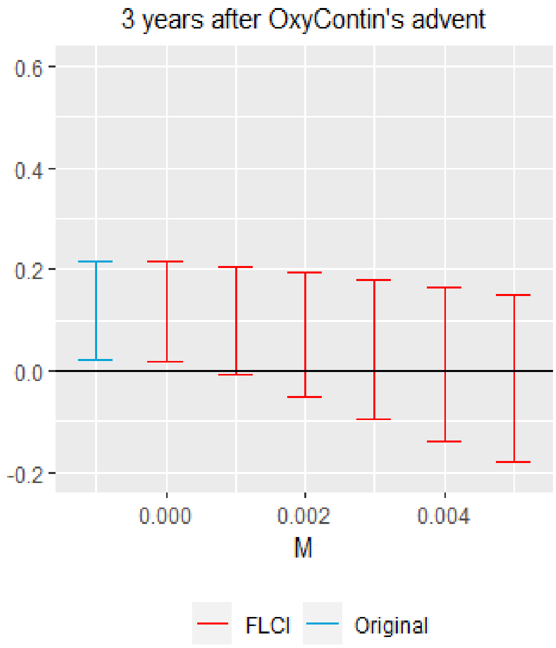
[37] In Rambachan and Roth (2020), M is defined as an upper bound on the degree of change of the slope of difference in trends between consecutive periods can change.

[38] 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.

## **B.1.** Sensitivity Check for the Event-Study Analysis




(a) Violent




(b) Property

Notes: Confidence intervals in blue for both violent and property crimes are from Figure 2.
Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990-2016.