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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. 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 mood instability, alcohol abuse, and illegal drug markets.

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. 2022). 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 (Alpert, 2022; Arteaga and Victoria, 2021; Ruhm, 2018), 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; Evans et al. 2022). 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., 2021). 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. (2022) 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

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

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Table 1
Summary Statistics: Demographic Characteristics.

	Entire Sample	Triplicate	Non-Triplicate
A. Crime Type			
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)
Agencies	7325	2048	5277
Observations	170,914	44,740	126,174
B. Demographics			
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)

Notes: 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. For panel A, 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.
Sources: For panel A, UCR Offenses Known and Clearances by Arrests, 1990–2016. For panel B, CPS segment of IPUMS and LEOKA for sworn police officer per 100,000 residents for 1990–2016.

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 (2018) 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 violent (25%) crimes and property crime (12%) compared to states with the triplicate pre- scription policy (triplicate states). Non-triplicate states experienced a persistent rise in violent crime before declining in 2014–2016, though effects for these years are elevated. The largest effects for property crime are concentrated among the first five years after OxyContin entered the market (until 2000). Among violent crimes and property crimes, aggravated assault and burglary increased the most, respectively. While I do not find evidence of pre-treatment differential trends between the two groups for violent crimes, differential pre-trends appear for property crimes. Consequently, I cannot draw a strong conclusion about the causal relationship between the introduction of OxyContin and property crimes with this study's identification strategy.

To shed light on the structural effects of OxyContin on crime, I instrument for the number of opioid (OxyContin and oxycodone) prescriptions per 1000 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 over-prescribing opioid drugs (Berina et al. 1985; Alpert et al. 2022), I find that opioid drugs were prescribed more often in non-triplicate states by 44 per 1000 Medicaid beneficiaries after the intro- duction of OxyContin. The triplicate-status-based IV estimates show that both property and violent crimes increase with an additional opioid prescription per 1000 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 (especially violent 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 (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 mood instability, such as impulsive behavior and/or violent tendencies (Roth, 1994; Jaffe and Jaffe, 1999; Fazel et al., 2006; Moore et al. 2010; Moore et al. 2011), finding suggestive evidence on the fact that individuals in non-triplicate states, suffered from mood instability 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, 2010; Markowitz, 2005; Heaton, 2012; Cook and Durrance 2013; Anderson et al. 2018; 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 in- creased 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 (Buchmueller and Carey 2018; Mallat 2018;

[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

Grecu et al. 2019; Wen et al. 2019; Dave et al., 2021; Evans et al. 2022).[3] My findings demonstrate 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. (2022), 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 the drug 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 con- sume 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 overpromotion.[5]

Convinced by Purdue Pharma's campaign and promotion, physicians began prescrib- ing 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, 2018). Among other opioid drugs, OxyContin prescriptions increased approximately tenfold be- tween 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 Oxy- Contin. 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., 2022).

Doctors in states with triplicate prescription program were required to make three copies of the prescription using serially numbered state-issued prescription forms for pre- scribing 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 due to the fear of the state government's monitoring of their prescribing practice. Citing Purdue's internal document, Alpert et al. (2022) 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 Tompkins, 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. (2022) 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. (2022), 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., 2022).

3. Data

I use data from the Uniform Crime Reporting (UCR) from 1990 to 2016 to under- stand 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

[3] These papers studied the effects of more recent prescription drug monitoring programs known as PDMPs 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 1996–767 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 1067 in 2002 after including sales representatives from Abbott Laboratories

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

[6] Alpert et al. (2022) obtained Purdue Pharma's internal documents from recently unsealed court documents in multiple lawsuits against the pharmaceutical firm

every year of the sample periods following Maltz and Targonski (2002). This yields a total of 7325 agencies in 48 states over 27 years.[7] For the analysis of OxyContin's launch on crime, crimes are modeled per 100, 000 residents in a given agency's jurisdiction.[8]

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.[9] In addition, I collected information on the unemployment rate and minimum wage from the Bureau of Labor Statistics (BLS) and Vaghul and Zipperer (2016), respectively, to control for economic conditions that may affect crime.[10] 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.[11] 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.[12]

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. (2022). 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:

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

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.[13] 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 value of one for year greater than or equal to 1996. The coefficient of primary interest, β_1, represents the causal effect of OxyContin's introduction on crime rate in the U.S.

X_{st} is a vector of control variables that account for characteristics of each state to which agencies are belong. To control for unobserved and time-invariant agency-specific heterogeneity, I include agency-fixed effects, γ_a. In addition, year fixed effects, δ_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 con- founding factors that other control variables cannot capture across states. ϵ_{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 note- worthy 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 (2013).

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 2008). To test the parallel trend assumption, I conduct the event-study exercise by using the following model:

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

where Triplicate status is interacted with a full set of year dummies. I normalize β_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 Eq. (1) over all years.

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 Eq. (1), and repeat this procedure 1000 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 demo- graphic 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 presents the descriptive statistics for Part I crime rates (Panel A) and demo- graphic characteristics (Panel B). 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 Appendix Table A.2, among property crimes, 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 et al. 2014). Fig. 1, however, reveals crime

[7] In total, 48 states including D.C. are included in the sample. States that are excluded from the sample are Montana and Vermont.

[8] Note that not all policing agencies are recorded as having a population, though they provide crimereports. 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.

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

[10] The minimum wage dataset contains information on federal, state and substate level. For more details, see https://github.com/equitablegrowth/VZ historicalminwage/release.

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

[12] I used the Prescription Drug Abuse Policy System (PDAPS) website to obtain information on when (date) PDMPs were implemented by a state. I collected information on beer tax rates from the Urban- Brookings Tax Policy Center. I referenced Yang (2017) for SNAP/TANF availability for drug-related felonies. For marijuana laws, I refer to https://norml.org/laws/decriminalization/.

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

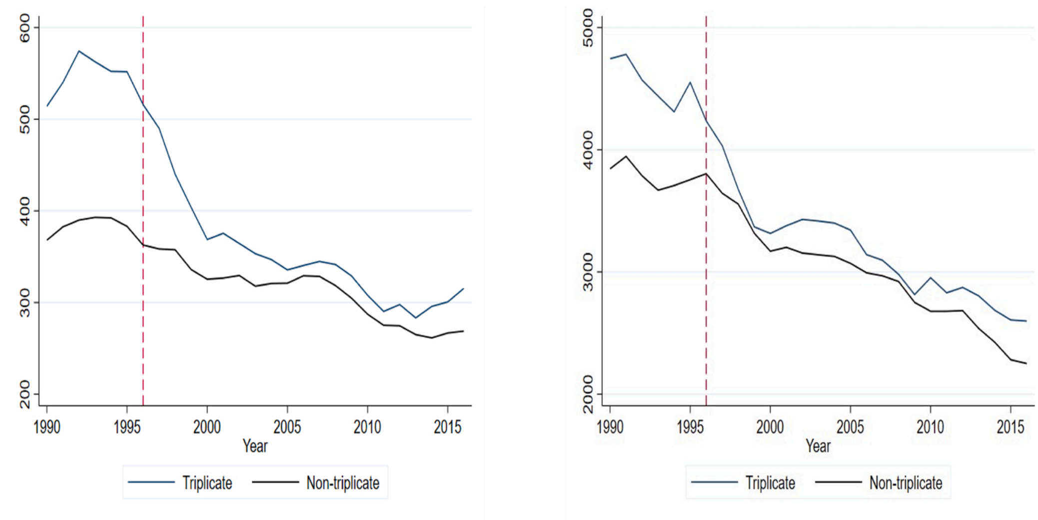


Fig. 1. The Trends of Crime Rates. Note: Annual average crime rate is reported by crime types.
Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990–2016.

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 both crime types, Fig. 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.

Panel B of Table 1 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 Oxy- Contin on crime using Eq. (1). In Table 2.1, I report estimates of Eq. (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 ex-periences 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.

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 (2013). 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.

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 et al. 2017; Doleac and Mukherjee, 2019; Packham, 2019; Dave et al., 2021).[14]

When it comes to the opioid crisis, CDC defines three waves of the opioid overdose epidemic: 1996–2000 for increase in opioid prescription that is corresponding to the introduction of OxyContin, 2001–2010

[14] 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 recently published paper, Dave et al. (2021) 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%.

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 (2013). 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.

for the first wave of the opioid epidemic, and 2011–2016 for second and third waves of the epidemic.[15] Following Alpert et al. (2022), I extend the baseline model in a way that it can capture the dynamics of the main DID estimates, β_1 in Eq. (1), by splitting the sample period into 4 periods: 1990–1995, 1996–2000, 2001–2010, and 2011–2016. Table 3 presents estimates that are corresponding to each time interval. Relative to the baseline time interval 1990–1995, I find that property crime increased by 14% in the introduction period relative to triplicate states (in Column 1–2). For the later periods, non-triplicate states experienced relative declines in property crime. Effects are robust to the model specification with state-specific linear time trend. While the size of estimates of property crime remains similar across sub-periods, the point estimates of violent crime shows that the effect gets larger over time: 17% increase in 1996–2000–36% increase in 2011–2016. The estimated effects for violent crime are also robust to model specifications, though the size of coefficients in column 4 are a bit smaller.

Table 3
Main Results by Sub-Period.

	Property		Violent	
	(1)	(2)	(3)	(4)
1996 – 2000	0.140***	0.147***	0.170***	0.129***
	(0.027)	(0.035)	(0.031)	(0.029)
2001 – 2010	0.100**	0.110*	0.276***	0.185***
	(0.045)	(0.060)	(0.059)	(0.0505)
2011 – 2016	0.123**	0.109**	0.360***	0.188***
	(0.049)	(0.048)	(0.071)	(0.054)
R-squared	0.779	0.785	0.708	0.714
Linear Trends		YES		YES
Observations	170911	170911	170911	170911

Note: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p< 0.1, * * p< 0.05, * ** p< 0.01. Dependent variable is logarithmically transformed. 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 population covered by the agency.

5.3. Event study analysis

In this section, I examine the dynamics of the effects of OxyContin's introduction on crime by using Eq. (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 Fig. 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 Fig. 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 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 may not be interpreted as a causal effect.

I also perform the event-study analysis with each crime type being a dependent variable to explore if the parallel trends assumption holds for the DID estimates reported in Table 2.2. As presented in Appendix Figure A.1, non-triplicate states experienced a persistent rise in all types

[15] According to CDC, the first decade of 21st century is defined the first wave of the opioid cri-sis when most opioid-related deaths are attributed to prescription schedule-II drugs, such as Oxy- Contin. Likewise, the second and the third waves represent the period when deaths from heroin and illicitly-manufactured fentanyl became more prominent. For more detailed information, go to https://www.cdc.gov/drugoverdose/opioids/prescribed.html.

Fig. 2. Event-Study Analysis.
Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990–2016.

of violent crime, but point estimates are noisy, particularly for rape crime. More importantly, the pre-OxyContin effects are close to zero and statistically insignificant for all violent crime types, implying that no pre-trends in each crime type are detected. For property crime types, the most significant effects are found in the first five years, and the effects are transitory except for burglary-type crime. Point estimates for the lead years show that the parallel trend assumptions do not hold for all property crime types, implying that any positive effects from the DID approach are spurious.

5.4. Robustness checks

5.4.1. Testing parallel trend assumption

First, I perform the sensitivity check for the event-study analysis under different assumptions on pre-treatment difference in trends following Rambachan and Roth (2020). I conducted an event-study analysis to assess the parallel trend in crime rates 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 differential trends may not serve as an accurate indicator of post-treatment differential trends. For example, 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 exploit the "honest DID" 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.[16] 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.

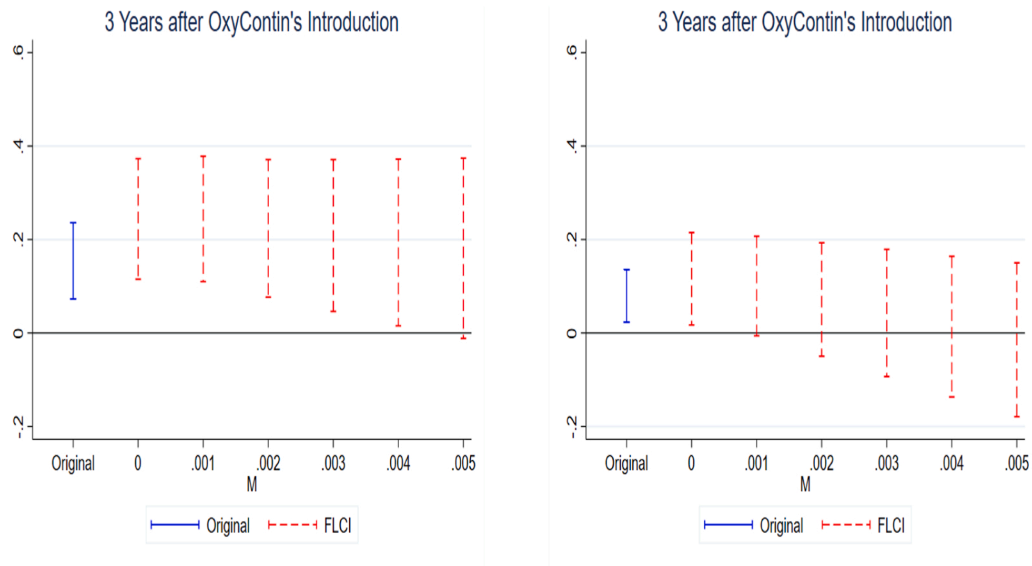
Fig. 3 depicts sensitivity checks for the treatment effects on violent and property crimes three years after OxyContin was introduced. The original DID estimate, with the 95% confidence intervals (CI), is in blue solid line (from Eq. (2)). Following Rambachan and Roth (2020), I plot the robust confidence intervals in red dashed lines. 'M′ is the degree of non-linearity of the slope representing the differential trend over consecutive time-periods.[17] 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.[18] This indicates that the main estimate is statistically significant if we assume that the degree of change of the slope representing differential trends does not exceed 0.4% between consecutive periods. Following Rambachan and Roth (2020), I construct a 95% CI for the largest change in slopes of differential trends between consecutive periods using pre-periods to evaluate the breakdown value of M. The CI for the largest change in the slope of differential trends in the pre-periods is [0, 0.154]. Based on the CI for the pre-periods M, we cannot rule out that the maximum pre-treatment non-linearity is greater than 0.004. However, by definition of the breakdown value of M, we cannot reject the null effect if the linear violation of parallel trends assumption is greater than 0.004. One possible explanation for such a result is that the event-study point estimates prior to 1996 moved around a lot from period to period, resulting in a larger maximal degree of the pretreatment non-linearity.

The robust confidence sets are similar to the original CIs at $M = 0$ for property crime (Panel B). This may imply that we can still obtain robust CIs at $M = 0$ as long as the slope of the pre-trend is not too large. The breakdown value of M indicates that we can reject the null treatment

[16] 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.

[17] 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.

[18] 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.



(a) Violent (b) Property

Fig. 3. Sensitivity Check for the Event-Study Analysis - HonestDiD Approach. Notes: Confidence intervals in blue solid lines ('Original') for both violent and property crimes are from Fig. 2. FLCI stands for fixed length confidence interval.
Source: The Offenses Known and Clearances by Arrest segment of UCR, 1990–2016.

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]. The CI for the pre-periods M suggests that the maximal change in slopes in pre-treatment periods is 0.081. However, any value of M greater than 0.001 yields the null effect by the definition of the breakdown value of M. Overall, the test shows that the result for property crime in 1998 is robust to linear violation of parallel trends, though the degree of non-linearity is very small.

5.4.2. Additional robustness checks

I conducted a number of sensitivity checks that are discussed in detail in the Appendix. This subsection briefly describes my findings. First, the synthetic control estimation provides evidence that crime rates in triplicate states reduced at a faster rate than the synthetic control group, especially for violent crime (See Appendix B). 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. Second, I employed instrumental variable approach to better understand the structural effects of OxyContin on crime (See Appendix C). The first stage shows that non-triplicate states recorded more prescription opioid drugs than triplicate states since OxyContin's introduction. An additional prescription for opioid drugs is associated with rises in both violent and property crimes. Third, I implemented an alternative inference method: Fisher's (1935) permutation test (See Appendix D). This placebo-type test shows that it is statistically rare to observe the main estimates inside of the distribution of the fake-treatment effects.

In addition to these three main robustness tests, I performed numerous sensitivity checks that are discussed in Appendix E. My results are robust to dropping one state at a time and comparing states with large population only. My estimates remain broadly similar if I remove agencies whose coverage might overlap with others from the sample. Furthermore, the estimates are statistically significant if I add quadratic linear trends to take into account economic downturns during the sample period. Finally, my results are not sensitive to weighting.

6. Suggestive evidence on mechanisms

In this section, I explore three potential channels through which the widespread pre- scription 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 Oxy- Contin 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 ad- diction 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 Oxy- Contin might have impacted crime. First, OxyContin might have negatively affected the mood of individuals who took prescription opioids regularly, and thus individuals addicted to opioids could be more prone to illegal behaviors. Although violence is not commonly considered as a side-effect of opioids abuse,[19] one cannot ignore cases where opioid addicts might display violent tendencies, particularly during withdrawal.[20] 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, 1999). Hence, it is possible that opioid abuse affects individuals' mood in ways that are associated with violent behavior. 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.

To understand the effects of OxyContin on individuals' moods, I conducted an event-study analysis of the mood state trend across triplicate and non-triplicate states using Behavioral Risk Factor Surveillance System (BRFSS) data.[21] In this exercise, I used the variable mental health as a proxy for an individual's mood. Panel A of Fig. 4 reveals the average number of days that individuals experienced mental health problems, such as stress, depression, or emotional difficulties, 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' mood states. However, OxyContin cannot be proved as the sole culprit of deteriorating mood stability across the two groups. Nevertheless, the mood state trend provides suggestive information about the effectiveness of 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 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. (2018) 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, 2010; 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.[22] As shown in Panel B of Fig 4, 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 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 violent crimes by 25%. Effects constantly increased in non-triplicate states after OxyContin entered the market. The main results imply that non-triplicate states could have experienced fewer violent crimes: 63 offenses per 100,000 on average from 1996 to 2016. Looking at specific crime types, aggravated assaults increased the most (24%) among violent crimes, followed by robbery crime (19%).

I also explore the heterogeneity of OxyContin's impacts on crime by sub-periods representing each wave of the opioid epidemic. Relative to triplicate states, non-triplicate states experienced a persistent rise in violent crime, while the effects on property crime became smaller in recent periods. Additionally, states without triplicate prescription program recorded an increasing number of prescriptions for opioid drugs. The results from the 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 eval- uate 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.[23]

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 im- portance of implementing a

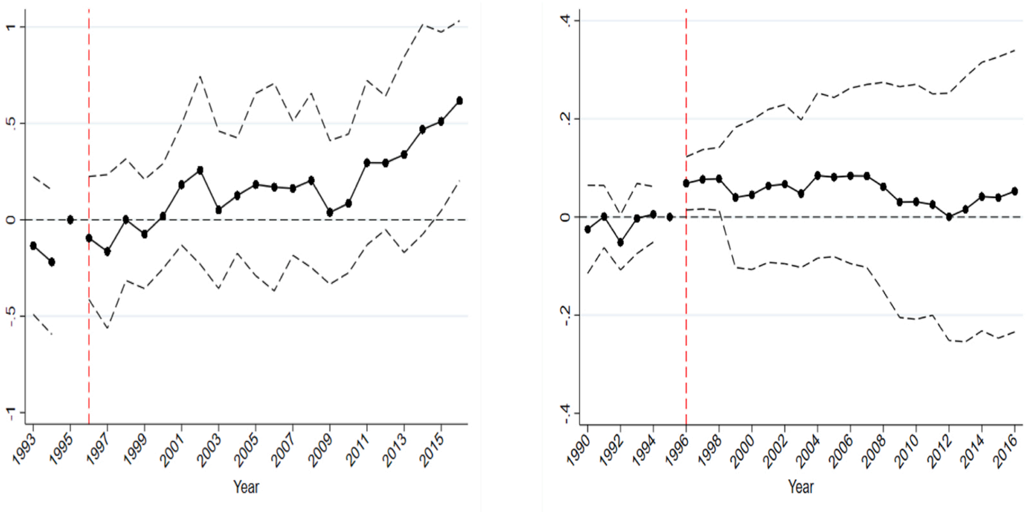
[19] Well known effects of opioid use are the production of analgesia, altered mood (often euphoria), de- creased anxiety, and respiratory depression (Boles and Miotto, 2003).

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

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

[22] 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.

[23] 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 $2651.28 (in 2016 dollar).



(a) Mood Instability (b) Alcohol Consumption

Fig. 4. Potential Mechanisms - Event Study Analysis. Notes: Both panels present the event-study estimates using the Eq. (2), which include state and year fixed effects, state demographic characteristics, macro economic variables, and beer tax. Panel (a) shows the trend in the average number of days that respondents felt their mental health was not good during 30 days prior to the interview. Panel (b) presents the 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) downloaded from ICPSR.

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.

CRediT authorship contribution statement

Yongbo Sim: Conception and design of study, Acquisition of data, Analysis and/or interpretation of data, Writing – original draft, Writing – review & editing.

Declaration of interest

None.

Data Availability

Data will be made available on request.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.irle.2023.106136.

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