NBER WORKING PAPER SERIES

FROM ADDICTION TO AGGRESSION:
THE SPILLOVER EFFECTS OF OPIOID POLICIES ON INTIMATE PARTNER VIOLENCE

Dhaval M. Dave
Bilge Erten
Pinar Keskin
Shuo Zhang

Working Paper 31609
http://www.nber.org/papers/w31609

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2023, Revised April 2025

We are grateful for funding support for this research from the National Institute on Drug Abuse (NIDA), National Institutes of Health (NIH) (Grant number R21DA058849). The authors have no relevant or material financial interests that relate to the research described in this paper. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

From Addiction to Aggression: The Spillover Effects of Opioid Policies on Intimate Partner Violence
Dhaval M. Dave, Bilge Erten, Pinar Keskin, and Shuo Zhang
NBER Working Paper No. 31609
August 2023, Revised April 2025
JEL No. H0, I12, I18, K0

ABSTRACT

Substance abuse is a major risk factor for intimate partner violence (IPV). We investigate how a key supply-side intervention – the abuse-deterrent reformulation of a widely-diverted opioid, OxyContin – affected IPV. Our results indicate counties with greater baseline rates of prescription opioid usage experienced relatively larger declines in IPV after OxyContin's reformulation. The reformulation reduced IPV only in states with small illicit drug markets, while states with large illicit drug markets experienced increased heroin-involved IPV due to substitution towards illicit opioids. Our results underscore the importance of identifying populations at high risk of substitution to illicit opioids and moderating this risk with evidence-based policies.

Dhaval M. Dave
Bentley University
Department of Economics
175 Forest Street, AAC 195
Waltham, MA 02452-4705
and IZA
and also NBER
ddave@bentley.edu

Bilge Erten
Northeastern University & IZA
Economics Department
Boston, MA 02115
b.erten@northeastern.edu
erten.bilge@gmail.com

Pinar Keskin
Department of Economics
Wellesley College & IZA
106 Central Street
Wellesley, MA 02481
pkeskin@wellesley.edu
pkeskin@wellesley.edu

Shuo Zhang
Department of Economics
Northeastern University
43 Leon Street, 312A Lake Hall
Boston, MA 02115
s.zhang@northeastern.edu

1 Introduction

Intimate partner violence (IPV) is a significant public health problem, being the most common form of violence experienced by women and imposing adverse consequences for the health of the victims and their children (World Health Organization 2013). According to the National Intimate Partner and Sexual Violence Survey (NISVS), 6.6 percent of women in the United States report experiencing IPV in the past 12 months, reaching a lifetime prevalence of 37 percent. An important risk factor associated with IPV perpetration is substance abuse, which can trigger aggressive behavior and worsen impulse control problems (Castilla and Murphy 2022; Chalfin et al. 2021; Angelucci and Heath 2020). With the U.S. facing an epidemic of opioid overdose, public health experts raised concerns about the role that opioid misuse plays in facilitating IPV (Warshaw et al. 2014; Packard and Warshaw 2018).[1] While increasing trends in opioid misuse are causing a serious public health crisis across the U.S., their consequences for IPV have not been explored systematically.

This paper examines the effects of opioid misuse on IPV by studying the reformulation of the main legal opiate—OxyContin—into an abuse-deterrent form in 2010, a major supply-side intervention implemented in the U.S. to curb excessive prescription of opioids and reduce their addictive potential. We provide the first study on the spillover effects of the OxyContin reformulation on domestic violence by intimate partners, and inform how a supply-side shock that disrupted access for one particular, albeit important, segment of the opioid market, generated downstream impacts on interpersonal violence and women's well-being. We combine IPV data from the National Incident Based Reporting System (NIBRS) from 2006 to 2019, which includes incident-based reports to law enforcement agencies, with county-level opioid prescriptions prior to 2010, the year in which OxyContin was reformulated. We capitalize on the baseline spatial variation in treatment exposure within a difference-in-differences (DID) framework to examine whether areas that were more ex-

[1] A recent review finds that among men using opioids, the prevalence of IPV perpetration ranged from 15% (past year physical IPV) to 58% (lifetime prevalence of any IPV); opioid use also raises the risk of being a victim of IPV, with 32-75% of women, who had used opioids, reporting victimization in the past year (Stone and Rothman 2019).

posed to prescription opioids prior to reformulation experienced differential changes in IPV outcomes after the reformulation.

We find that the reformulation of OxyContin into an abuse-deterrent form led to a significant relative decline in the rate of IPV experienced by women in counties with greater exposure to prescription opioids prior to the reformulation. We show that these declines occur after the policy change, and they are driven primarily by non-Hispanic Whites. The coefficient estimates imply that a one standard deviation increase in pre-reformulation exposure yields a relative decrease of 6.2 percent annually in the IPV rate following OxyContin's reformulation. We also document corollary declines in injuries and arrests related to IPV (6.6 percent and 4.7 percent, respectively), indicating that the effects are reflective of an actual decline in the incidence of IPV rather than a shift in reporting behaviors. The overall decline in IPV, however, masks a significant uptick in IPV incidents where the perpetrator was suspected of using heroin, particularly in more urban areas. These findings highlight the importance of identifying populations at a higher risk of substitution to illicit opioids post-reformulation and mitigating this risk with evidence-based policies.

Exploring potential channels, we document that the reformulation reduced the IPV rate primarily in states with less-developed illicit opioid markets, where options for substitution towards illicit drugs were much more limited ex ante. In states with larger and more developed illicit opioid markets, we find no evidence of a decline in the IPV rate; conversely, these locations which would offer greater substitution possibilities towards illicit drugs actually experienced an increased rate of heroin-involved IPV following the reformulation. Moreover, if the primary mechanism for reducing IPV prevalence is the decline in prescription opioid misuse, we would expect to see larger reductions in IPV among demographic groups and locations that initially had higher rates of prescription opioid misuse and thus benefited more from the reformulation. Our pattern of results is consistent with this mechanism: sub-populations (non-Hispanic Whites; younger adults) and localities (lower-educated; high-poverty) which experienced higher rates of opioid prescribing and misuse at baseline, accrued the largest benefits in terms of lower IPV rates. Finally, to the extent that the OxyContin reformulation resulted in a decline in employment and labor force participation rates of both men and women at similar rates (Cho et al. 2021; Harris et al. 2020; Aliprantis et al. 2023), we would expect this channel to increase IPV risk through worsened financial distress at the household level. Our baseline specification adds controls for local area unemployment and labor force participation rates, which only marginally impacts our estimates. Hence, we largely rule out shifts in labor market outcomes as a key channel underlying our main results.

We make several contributions to the literature. First, despite the well-known associations, most of the previous studies that document the relationship between opioid misuse and IPV are based on small sample sizes and fail to account for selection bias and reverse causality (Hughes et al. 2019; Stone and Rothman 2019; Pryor et al. 2021). Our empirical setup allows us to estimate the effects of an exogenous supply-side intervention targeting opioid misuse on the risk of IPV victimization.

Second, our study contributes to the literature on the broader repercussions of the opioid crisis on families. Gihleb et al. (2022) find that must-access Prescription Drug Monitoring Programs (PDMPs), a similar supply-side shock that constrained access to prescription opioids for misuse purposes, reduced entry into foster care. Emerging evidence (Dave et al. 2024; Barbos and Sun 2021) linking must-access PDMPs to IPV points to results consistent with ours, that controlling the supply of Rx opioids (albeit via a different lever and margin, by targeting prescribers) has led to a net decline in women's exposure to domestic violence. A closely related paper to ours is Evans et al. (2022), which finds that counties with greater initial rates of prescription opioid usage experienced an increase in child maltreatment after OxyContin reformulation. It is important to note that the rates of IPV and child maltreatment within a county are almost orthogonal to each other.[2] One explanation could be the differences in reporting and censoring. Child maltreatment, often reported by teachers, typically requires more concrete evidence, resulting in only

[2]The within county correlation of the rates of alleged child abuse or neglect and intimate partner violence reported to the police is -0.05 for the overlapping counties from 2006 and 2016.

the more severe cases being documented, while IPV is more frequently reported by third parties, capturing a broader range of incidents. Additionally, child maltreatment data censors counties with fewer than 1,000 total cases, focusing on counties with relatively high rates of abuse. Moreover, the factors that increase the likelihood of partner abuse differ significantly from those of child abuse, making it uncertain whether an event that influences child maltreatment will have a similar impact on IPV.

Finally, we contribute to the growing literature on factors affecting IPV prevalence, ranging from the effects of cash transfers (Bobonis et al. 2013), labor market shocks (Aizer 2010), education (Erten and Keskin 2018), divorce laws (Stevenson and Wolfers 2006) and trade shocks (Erten and Keskin 2021). Evidence on the effects of substance use on IPV is rare, and focuses on alcohol use (Castilla et al. 2022; Markowitz 2000). Using a randomized control trial in rural Kenya, Castilla et al. (2022) find that the reduction in alcohol use lowers sexual violence.

2 Background and Data

2.1 Introduction of OxyContin Reformulation

In the US, the number of opioid prescriptions nearly quadrupled from 76 million in 1991 to over 250 million in 2010 (Volkow 2014). During this period, Purdue Pharma–the company that released OxyContin in 1996–invested heavily in advertising campaigns to increase the use of opioids for treating chronic non-cancer pain (Boudreau et al. 2009; Alpert et al. 2022). However, OxyContin was highly addictive due to its formulation as a potent opioid containing oxycodone, which directly interacted with the brain's opioid receptors, leading to feelings of euphoria and pain relief. Moreover, if the pill was tampered with by crushing or dissolving, it could release a large dose of oxycodone all at once, increasing the risk of addiction (Van Zee 2009).

In order to address the misuse of OxyContin and its diversion to illicit markets, Purdue Pharma developed an abuse deterrent formulation that was designed to be harder to crush or dissolve. This version received approval from the Food and Drug Administration (FDA) in April 2010. Purdue Pharma began distributing the new formulation while discontinuing the shipment of the previous formulation in August 2010. The reformulation successfully reduced prescription opioid abuse involving OxyContin (Butler et al. 2013). As Figure 1 shows, annual opioid prescribing rates leveled off from 2010 to 2012 and then declined subsequently. These trends in opioid prescriptions were also highlighted by prior studies (Guy Jr et al. 2017; Powell and Pacula 2021), with the rate of annual opioid prescriptions increasing from 0.72 to 0.81 per person from 2006 to 2010, remaining constant from 2010 to 2012, and then decreasing steadily to 0.46 in 2019.

Several factors explain why opioid prescriptions remained stable for one to two years before decreasing after the reformulation of OxyContin. First, although the reformulated drug ceased shipments to retail pharmacies in August 2010, stockpiled original versions, that were easier to misuse, remained available, consequently delaying the reformulation's full impact.[3] Second, the reformulation reduced demand for new and existing users. New users had fewer chances of misuse with the abuse-deterrent version, leading to fewer prescriptions. For those already addicted and misusing OxyContin, substitution towards other illicit opioids has been found to be more gradual as illicit markets expanded in response to the reformulation (Powell and Pacula 2021); hence, this would be expected to lead to a somewhat gradual decline in their reliance on prescription opioids.

While the reformulation has been found to be effective in reducing prescription opioid prescribing and misuse/overdose related to prescription opioids (Hwang et al. 2015; Evans et al. 2019; Coplan et al. 2016), several studies also find evidence of substitution from licit prescription opioids into illicit opioids such as heroin and fentanyl, leading to an increase in overdoses related to these drugs (Evans et al. 2019; Powell and Pacula 2021).[4] A recent study has also documented an increase in child physical abuse and neglect after OxyContin's reformulation in impacted counties (Evans et al. 2022). In this study, we

[3]The FDA gave the reformulated drug an "abuse-deterrent" designation in April 2013.

[4]For a comprehensive review of the literature on the effects of opioid policies on health and crime outcomes, see Maclean et al. (2022). Several studies also examined the effects of mandatory access PDMPs on heroin use, crime, and mortality (Meinhofer 2018; Mallatt 2018; Dave et al. 2021; Kim 2021).

further widen the lens and examine how the reformulation of OxyContin has affected IPV prevalence in affected counties, thereby informing broader impacts on women's and their children's well-being.

2.2 IPV data

Our empirical analysis leverages police-reported intimate partner violence (IPV) incidents recorded in the National Incident-Based Reporting System (NIBRS) from 2006 to 2019. NIBRS is a system that U.S. law enforcement agencies had voluntarily used to report incident-based crime data. Each report in the NIBRS contains information about the characteristics of the victim (age, gender, race, ethnicity, and relationship to the offender), the offender (age, gender, race, ethnicity, and whether they were suspected of using substances, including heroin), and the incident itself (date/time, injuries, arrests). This dataset offers a significant improvement over survey data as it is less reliant on self-reports, was consistently collected over a prolonged period, and permits us to identify if an offender was suspected of using opioids.

We examine IPV experienced by female victims, including relationships that consist of spouses, common-law spouses, boyfriends/girlfriends, homosexual partners, ex-spouses, and ex-boyfriends/girlfriends. The incidents considered are aggravated assaults, simple assaults, forced sex, and intimidation. Our primary indicator is the annual IPV rate per 1,000 population at the county level. We use a balanced panel of county-level data from 2006-2019.

The average county had an IPV incident rate of 2.65 per 1,000 population annually, with 50 percent of these incidents resulting in injuries and 55 percent of them ending with the perpetrator being arrested (Appendix Table A1). Figure 1 illustrates that the annual IPV rate in the US followed a declining trend from 2.83 per 1,000 in 2006 to 2.36 per 1,000 in 2019.

2.3 Data on opioid prescriptions and county-level covariates

Our primary explanatory variable—the pre-reformulation exposure to prescription opioids—is measured by the population-weighted average number of Schedule II opioid prescriptions per capita by county from 2006 to 2009 following Evans et al. (2022). These data are reported by the Centers for Disease Control (CDC).

We use multiple data sources to account for time-varying county characteristics that could influence IPV outcomes. Demographic data, including gender, racial and age composition, come from the Surveillance, Epidemiology, and End Results (SEER) Program. We also use the cancer death rates per 100,000 individuals reported by the CDC and the annual average unemployment and labor force participation rates reported by the Bureau of Labor Statistics to account for socioeconomic conditions at the county level.

We also control for baseline (2006) values of the following county characteristics, interacting them with year fixed effects to account for time-varying spatial shocks. First, we include the share of the population without any college education, from the American Community Survey, to account for counties more exposed to labor-saving technical changes and associated deaths of despair (Case and Deaton 2017, 2020). Second, we add the share of employment in mining, reported by the BLS, to control for higher rates of injury in underground mining, which increase opioid consumption and mortality rates (Monnat 2018; Metcalf and Wang 2019).

Furthermore, we control for indicators for whether the state has a PDMP of any form and whether it has a medical marijuana law in our baseline analysis. We incorporate further state policies in robustness checks. Appendix A provides descriptions of data sources. Appendix Table A1 provides summary statistics.

3 Empirical Analysis

3.1 Identification

We focus on the effects of OxyContin reformulation on the IPV outcomes by employing event study and difference-in-differences methodologies, leveraging spatial variation in the intensity of the treatment driven by the variation in pre-intervention exposure to prescription opioids across counties. The reformulation of OxyContin serves as an exogenous shock since it occurred unexpectedly in 2010 and affected all counties in our sample to varying degrees based on pre-reformulation exposure. Following Evans et al. (2022), we measure the pre-intervention exposure at the county level by the population-weighted average number of opioid prescriptions per capita using CDC data from 2006 to 2009. For ease of interpretation, we standardize this exposure measure to have a mean of 0 and a standard deviation of 1.

Using an event study analysis, we disentangle dynamics in the causal relationship between the reformulation and our outcomes by interacting indicators for single years and the county-specific pre-intervention measure of exposure using the following specification:

$$\begin{aligned} Y_{ct} = &\sum_t \gamma_t 1\{year = t\} \times Exposure_c + \beta X_{ct} \\ &+ \sum_t \theta_t 1\{year = t\} \times X_c + \delta_c + \delta_t + \epsilon_{ct}, \end{aligned} \tag{1}$$

where Y_{ct} represents the outcome of IPV rate per 1,000 in county c and year t. We consider related outcomes, including the heroin-involved IPV rate, injury rate, and arrest rate associated with an IPV incident. The first terms on the right-hand side are the difference-in-differences (DID) terms, interactions of a full set of year dummies (excluding 2010, the year in which OxyContin was reformulated) with the (time-invariant) county-level pre-intervention exposure to prescription opioids, $Exposure_c$, as described above. The coefficients of interest are γ_t event year coefficients, which reveal the differences in IPV rates between counties with higher and lower pre-intervention exposure in year t, relative to 2010, the year in which OxyContin was reformulated. The term X_{ct} represents a vector of covariates that vary across counties and over time. These include county-level covariates—percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population in different age groups; unemployment and labor force participation rates—and state-level policies including indicators for a PDMP of any form and a medical marijuana law. The term X_c represents initial (2006) county characteristics as discussed in Section 2.3: share of population without any college education and the share of employment in mining. Including interactions of these characteristics with the full set of year dummies allows their relationship with IPV rates to differ before and after the reformulation of OxyContin. The county fixed effects δ_c absorb time-invariant differences across counties that contribute to disparities in the IPV rate, while the year fixed effects δ_t account for any time-varying national shocks affecting all counties identically in a particular year. Regressions are weighted by 2006 county population. Standard errors are clustered at the county level to account for serial correlation in the error term within a county.

To facilitate a more standard interpretation of the average treatment effect of the reformulation on IPV over the entire post-reformulation period, we employ a generalized DID specification:

$$Y_{ct} = \gamma_1 Post_t \times Exposure_c + \beta X_{ct} + \sum_t \theta_t 1\{year = t\} \times X_c + \delta_c + \delta_t + \epsilon_{ct}, \qquad (2)$$

where the indicator variable $Post_t$ takes a value of 1 for the post-reformulation period, which is from 2010 to 2019. The other terms are defined as in Eq. (1).

An attractive feature of this DID identification strategy is its ability to isolate the effects of OxyContin reformulation on IPV outcomes. While counties with high and low pre-intervention exposure are not identical, comparing outcomes within counties over time isolates the differential impact of the OxyContin reformulation.

3.2 Primary Results

We begin by graphically illustrating that the pre-reformulation exposure to prescription opioids in a county strongly predicts differential changes in prescription opioid misuse after the OxyContin reformulation in 2010. Appendix Figure A1 plots the "first-stage" relationship between the population-weighted average per capita Schedule II opioid prescriptions in the county in the pre-reformulation exposure period from 2006 to 2009 and the state-level change in the OxyContin misuse rate from 2008 to 2012 using data from Alpert et al. (2018) (Panel A) and the county-level change in per capita Schedule II opioids from 2008 to 2019 using the CDC data (Panel B). Categorizing counties into quartiles based on their pre-reformulation exposure to prescription opioids, we observe that counties with higher pre-reformulation exposure experience greater reductions in both the OxyContin misuse rate between 2008 and 2012 and Schedule II opioid prescriptions per capita between 2008 and 2019.

We start by exploring the dynamic effects of OxyContin reformulation on IPV outcomes. Figure 2 shows event-study plots. In Panel A, we find that in the period prior to the intervention in 2010, the coefficient estimates are statistically indistinguishable from zero. This lack of a pre-existing differential trend in counties that are more exposed to OxyContin reformulation provides a validation of the common trends assumption, supporting our DID strategy.[5] By contrast, after the reformulation, the coefficient estimates shift down noticeably and become statistically different from zero after two years, a lag which is consistent with the delayed reduction in OxyContin misuse as the original versions remained accessible due to stockpiling and availability on the street markets as explained in Section

[5]Table A2 presents the pre-trend evaluation, following the approaches outlined by Roth (2022). For all the outcomes depicted in Figure 2, we observe that none of the individual pretreatment coefficients are statistically significant, with all t-values being less than 1.96. A joint significance test of all pre-period coefficients indicates that we cannot reject the null hypothesis that all pre-period coefficients are zero (p-values > 5%). The slopes of the fitted lines of pretreatment coefficients are all insignificant. In addition, we conducted a pre-trend test to assess whether there are linear violations of parallel trends that conventional pretests would detect 50% or 80% of the time ($\gamma 0.5$ and $\gamma 0.8$). We found that all slopes are less than 0.05 for more than 50% or 80% of the time.

2.1.[6], [7] These findings indicate that counties that had high opioid prescribing rates at baseline and thus were more exposed to the OxyContin reformulation experience significantly larger declines in IPV relative to those that are less affected.

Table 1 shows the estimated average treatment effects realized over the entire post-intervention period, and their robustness to progressively adding more covariates, with column (5) representing the fully saturated model. In the first row, the coefficient estimate implies that a one standard deviation increase in pre-reformulation exposure yields about a 6.2 percent annual decline in the IPV rate on average following OxyContin's reformulation.[8]

In Panel B of Figure 2, the event study plot for the heroin-involved IPV rate shows that following the reformulation, there is a gradual increase in the rate of heroin-involved IPV incidents (i.e., those where the police suspected the offender of having used heroin), while the estimates are for the most part imprecisely estimated for the full sample. The delayed effects may be due to the gradual shift of highly addicted individuals from OxyContin to heroin and the expansion of illicit drug markets to meet the increasing demand (Powell and Pacula 2021). Despite the delay, the magnitude of the pooled coefficient estimate is quite substantial. The average treatment effects reported in the second row of Table 1 imply that a one standard deviation increase in pre-reformulation exposure is associated with a tripling of the rate of heroin-involved IPV rate per 1,000 population in the post-reformulation period. This increase in heroin-involved IPV rate is consistent with some opioid-dependent individuals substituting into heroin use once it became difficult to access and abuse OxyContin (Alpert et al. 2022), and heroin use being associated with a greater risk of IPV perpetration (El-Bassel et al. 2007; Tran et al. 2014). However, since heroin-involved IPV is a small fraction of the total IPV incidents (less than 1%), the large increase in IPV incidents driven by heroin consumption of highly addicted individuals does not

[6]These findings are also in line with Figure 1, illustrating a flattening of the trend in per capita prescription opioid consumption from 2010–2012, with a steady decline star starting in 2013.

[7]Similar effects operating with some delay and compounding over time are not uncommon in studies of the OxyContin reformulation and of policies (i.e., PDMPs) that aim to restrict access to prescription opioids (Beheshti and Kim 2022; Gihleb et al. 2022; Powell and Pacula 2021; Park and Powell 2021; Dave et al. 2021).

[8]This reduction represents a decline in IPV of 0.1762 incidents per 1,000 population as share of the pre-reformulation outcome mean of 2.8319 (-0.1762/2.8319*100).

offset the decline in total IPV incidents observed overall in affected counties.

The event study results for the injury rate and arrest rate per 1,000 population are consistent with the results observed for the IPV rate (Panels C and D of Figure 2). Specifically, the affected counties experience a significant relative decline in the rates of injury due to IPV as well as a decline in the arrest rates for IPV incidents after the reformulation. Table 1 shows that a one standard deviation increase in pre-reformulation exposure yields about a 6.6 percent annual decline in the injury rate and a 4.7 percent annual decline in the arrest rate for IPV cases after the reformulation. The corollary declines for injuries and arrests related to IPV imply that the overall decline in reported IPV incidents to law enforcement agencies (Panel A of Figure 2) reflects an actual decline in incidence in more affected counties rather than just a shift in reporting behaviors.

To place our effects in context, Evans et al. (2019) report an 11 percent short-term decline in non-heroin opioid-related mortality due to the reformulation, compared to our 6 percent annual decline in the IPV rate. Evans et al. (2019) and Alpert et al. (2018) find that each prevented non-heroin opioid death from the reformulation is approximately replaced by a heroin death, resulting in no significant impact on the combined opioid death rate. While we also find a substantial increase in heroin-involved IPV rate, there are several reasons for why this increase in heroin-involved IPV is not large enough to offset the decline in non-heroin related IPV. First, the one-to-one offset in opioid versus heroin deaths does not imply that the same number of opioid abusers switch to heroin. In fact, Evans et al. (2019) impute that less than 10 percent of recreational pain medication users transition to heroin annually. However, because heroin is much more potent and has a higher overdose risk, even a less than one-to-one substitution results in a larger offset in overall mortality. In the case of IPV perpetration, although heroin abusers have a higher risk of perpetration than prescription opioid abusers, this increased probability is not sufficient to fully offset the decline in non-heroin IPV incidents resulting from the reformulation-induced reduction in prescription opioid misuse for the full sample. However, in areas with greater substitution into heroin given the presence of (and subsequent expansion of) illicit opioid markets, offset effects may be much stronger, which we explore in the next section to elucidate potential mechanisms underlying our results.

3.3 Potential Channels

Our findings highlight a distinct "reduced form" effect of OxyContin reformulation on intimate partner violence. This evidence suggests that a major supply-side intervention that made the main legal opiate abuse-deterrent can contribute to substantial mitigation of violence perpetrated against women. While several causal mechanisms may underlie these effects, many of which we are not able to definitively test due to data limitations, in this section we examine some key potential channels that could account for these findings. We do so by drawing primarily on existing evidence on potential mediators and indirect evidence through supplementary analyses.

First, although the reformulation of OxyContin reduced prescription opioid misuse, it also induced addicted individuals to turn to more potent illicit opioids (Evans et al. 2019; Powell and Pacula 2021). This shift was particularly pronounced in areas with a pre-existing large illicit drug market (Unick et al. 2014; Gupta and Mazumder 2023). Given that illicit opioid use is an important risk factor for IPV perpetration (El-Bassel et al. 2007), one would expect the decline in IPV to be stronger in regions with a small illicit drug market, limiting the substitution towards illicit opioids. In Figure 3, we assess whether the effects of OxyContin reformulation vary by the size of the illicit opioid market activity. We proxy the size of illicit opioid market activity by the ratio of deaths due to illicit and synthetic opioids to deaths from prescription opioids in a state over the period 2006 to 2009, prior to reformulation in 2010.[9] We then bifurcate the sample into two groups based on the median level of illicit opioid market activity. The estimates in Panel A indicate that the OxyContin reformulation reduced the IPV rate only in states with small illicit opioid markets. In states with larger and more developed illicit opioid markets, we find no evidence of any

[9] We follow Gupta and Mazumder (2023) in using this proxy for the size of illicit market activity at the state level. We derive this information from CDC WONDER from Multiple Cause of Death (MCOD) vital statistics data made available by the National Center for Health Statistics (NCHS) https://wonder.cdc.gov/mcd.html. For states with very low illicit mortality, the data are suppressed and we recode these values as zero.

significant impact on IPV rates. Furthermore, Panel B estimates show that the OxyContin reformulation increased heroin-involved IPV rate in states with large illicit opioid markets, with no significant changes in states with small illicit markets. These findings provide support to the hypothesis that the reduction in addiction stemming from the reformulation policy drive a decline in IPV risk, but only in locations with limited substitution towards illicit drugs.[10] In locations with strong options for substitution into other illicit opioids, the results show no evidence of a decline in IPV rate, and in fact, these counties experience a heightened risk of heroin-involved IPV incidents.

Second, if the primary mechanism for reducing IPV prevalence is the decrease in prescription opioid misuse due to the reformulation, one could expect to see larger reductions in IPV among demographic groups that initially had high rates of prescription opioid misuse and benefited more from the reformulation. As shown in Appendix Figure A1, OxyContin misuse declined differentially more in counties with initially higher rates of prescription opioid misuse at baseline. We examine whether OxyContin reformulation had heterogeneous treatment effects on IPV outcomes by victim characteristics by constructing the incident rate for each specific population subgroup (e.g., non-Hispanic White/Black, Hispanic, younger than 30, etc.). The left side of Panel A in Figure 4 shows that the decline in the IPV rate from the reformulation is strongest among the non-Hispanic White population, with little to no effects observed for non-Hispanic Black, Hispanic, or other racial/ethnic groups. This finding is consistent with the misuse of prescription opioids being highest among non-Hispanic White individuals in the first wave of the opioid crisis in late 1990s and 2000s in the US (National Academies of Sciences, Engineering, and Medicine and others 2017; Humphreys et al. 2022). Moreover, the magnitude of the estimated reductions in the IPV rate is larger among younger adults (ages 30 and below) compared to those older adults, while we cannot reject that they are statistically significantly different.[11] However, the relatively larger reduction in IPV risk for younger women is consistent with

[10] Appendix Figure A4 shows that the changes in injury and arrest rates in more exposed counties followed similar patterns with respect to heterogeneous impacts.

[11] Appendix Figure A2 reports that the reductions in injury and arrest rates in more affected counties largely followed similar patterns in heterogeneity by victim characteristics.

the evidence that younger individuals had a higher risk of consuming prescription opioids before 2010 and thus would be impacted more intensively from the lack of access to abuse-prone opioids (Palmer et al. 2015). The right side of Panel A in Figure 4 also shows that the non-Hispanic White population experienced the largest increase in the risk of exposure to heroin-involved IPV, in the post-reformulation period in more affected counties.

Third, another source of heterogeneity that could shed light on the underlying mechanisms is the location characteristics. One could, for example, expect the impacts on IPV outcomes to be larger in areas more severely affected by the first wave of the opioid epidemic due to greater reductions in prescription opioid misuse post-reformulation. Consistent with this hypothesis, the left side of Panel B in Figure 4 shows that the reformulation of OxyContin had stronger impacts in terms of IPV reduction in counties with lower levels of education (e.g., counties below the median share of high school graduates), which were more adversely impacted by the opioid epidemic during the first wave. We also find some suggestive evidence that IPV reduction was larger in poorer counties (e.g., counties below the median share of families at or below the poverty threshold), while the difference from less poor counties is not statistically significant. Similarly, we find no statistically significant difference in the estimated IPV reduction estimates between more metropolitan (e.g., counties with higher than median share of metropolitan population) or micropolitan and noncore counties. The right side of Panel B further illustrates that the post-reformulation increase in the heroin-involved IPV rate is higher among more metropolitan counties, consistent with urban regions having more developed illicit drug markets that can facilitate substitution into illicit opioids.[12] Appendix Figure A3 shows similar heterogeneity patterns for our other IPV-related injury and arrest rate outcomes.

Fourth, the OxyContin reformulation could also affect labor market outcomes through reductions in prescription misuse and potential substitution into illicit opioid consumption, with potentially differential effects by gender. Cho et al. (2021) find that more exposed states experienced a decline in employment and labor force participation rates for both men and

[12] These effects also seem larger in relatively more educated counties with higher poverty rates on average.

women, with no statistically significant differences between these two groups.[13] The decline in employment and labor force participation at the household level could increase the risk of IPV through increased financial distress in affected households. To the extent that this channel is operating, it would bias our estimates in the opposite direction. Moreover, controlling for unemployment and labor force participation rates at the county level makes no meaningful change to our estimates (i.e., the difference in column 1 and 2 estimates in Table 1 is marginal). Hence, we largely rule out the labor market channel as a key driver of our main results.

3.4 Robustness Checks

We conduct several sensitivity analyses. In Appendix Table A3, we incorporate additional state policies and regulations that could affect opioid use and IPV prevalence. We control for harm reduction policies, such as Good Samaritan Laws and Naloxone access laws, as well as policies related to recreational marijuana legalization and decriminalization. Additionally, we include controls for physical exam requirement (PER) laws, Medicaid expansion under the Affordable Care Act (ACA), and the Earned Income Tax Credit (EITC) coverage. Remarkably, these additional controls yield consistent estimates similar to those in Table 1.

In Appendix Table A4, we assess the sensitivity of our estimates to alternate specifications, samples, and covariates. First, we cluster standard errors at the state level to account for spatially and temporally correlated errors across localities and over time within the same state. Second, we add interactions of census division fixed effects with a post-reformulation period indicator to account for unobserved regional shocks. Third, we control for differences in police deployment by including the number of officers per capita. Fourth, to ensure data quality, we control for the number of agencies reporting IPV incidents within each county and year, and we exclude counties with inadequate IPV data reporting following

[13]Other studies also find mostly negative labor market effects (Harris et al. 2020; Aliprantis et al. 2023), with the exception of Currie et al. (2019), which finds small positive effects for women.

prior studies (Freedman and Owens 2011; Thomas and Shihadeh 2013; Fone et al. 2023).[14] These various checks yield findings consistent with our baseline estimates in Table 1.

Moreover, we incorporate an OxyContin-specific measure of pre-intervention opioid misuse to better assess OxyContin abuse, following Evans et al. (2022) and using the population-weighted rate of OxyContin misuse at the state level from 2004 to 2009 introduced by Alpert et al. (2018). The event-study estimates in Appendix Figure A5 align with our main estimates in Figure 2. Finally, we control for the housing market index (HHI) and 90-day mortgage delinquency rates at the county and state levels to account for the potential impact of the 2007–2009 housing bust and the Great Recession. Our estimates reported in Appendix Table A5 are robust to accounting for housing market fluctuations.

4 Conclusion

In this study, we provide the first evidence of the spillover effects of a key supply-side intervention targeting opioid misuse – the abuse-deterrent reformulation of OxyContin – on IPV. Using administrative data on incidents by female victims to law enforcement agencies combined with a difference-in-differences design, our results show that the reformulation led to a significant decline in exposure of women to IPV. Our estimates indicate that a one standard deviation increase in exposure to OxyContin reformulation resulted in a 6.2 percent decline in the IPV rate among women. The economic burden of IPV is staggering, amounting to over \$4.1 trillion (inflated to 2022\$), including \$2.4 trillion in medical costs and \$1.5 trillion in productivity losses (Peterson et al. 2018); almost 40% of the economic burden is borne by the public sector. The annual economic burden is estimated to reach \$714 billion (inflated to 2022\$) (Peterson et al. 2018). Monetizing the estimated decline in IPV observed for a one standard deviation shift for exposed counties, our results suggest that the OxyContin reformulation generated additional cost-savings on the order of \$42 billion annually.

[14] We follow Fone et al. (2023) and use 65% coverage rate, although results are similar for alternative cutoffs. See Appendix A for the definition of coverage indicator.

Our findings are consistent with the decline in IPV being driven by a reduction in prescription opioid misuse in locations where substitution to illicit drugs was limited. Specifically, we find that the reformulation reduced the IPV rate only in states with relatively low levels of illicit opioid market activity. In states with larger and more developed illicit opioid markets, there is no decline in the IPV rate; instead, our findings point to a substantial increase in heroin-involved IPV rate in these areas. Such unintended consequences underscore the importance of identifying populations and areas where the risk of substitution to illicit opioids is high, and targeting evidence-based policies that can counteract this risk.

References

Aizer, Anna, "The Gender Wage Gap and Domestic Violence," *American Economic Review*, 2010, *100* (4), 1847–1859.

Aliprantis, Dionissi, Kyle Fee, and Mark E Schweitzer, "Opioids and the labor market," *Labour Economics*, 2023, *85*, 102446.

Alpert, A., W. N. Evans, E. M. J. Lieber, and D. Powell, "Origins of the opioid crisis and its enduring impacts," *The Quarterly Journal of Economics*, 2022, *137*, 1139–1179.

Alpert, Abby, David Powell, and Rosalie Liccardo Pacula, "Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids," *American Economic Journal: Economic Policy*, 2018, *10* (4), 1–35.

Angelucci, M. and R. Heath, "Women Empowerment Programs and Intimate Partner Violence," *AEA papers and proceedings*, 2020, *110*, 610–614.

Barbos, Andrei and Minglu Sun, "Opioid Control Policies Can Also Reduce Domestic Violence," *Available at SSRN 4747347*, 2021.

Beheshti, David and Bokyung Kim, "Beyond Opioids: The Effect of Prescription Drug Monitoring Programs on Non-Opioid Drug Prescribing," *Available at SSRN 4098995*, 2022.

Bobonis, Gustavo J., Melissa Gonzalez-Brenes, and Roberto Castro, "Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control," *American Economic Journal: Economic Policy*, 2013, *5* (1), 179–205.

Boudreau, D., M. Von Korff, C. M. Rutter, K. Saunders, G. T. Ray, M. D. Sullivan, C. I. Campbell, J. O. Merrill, M. J. Silverberg, C. Banta-Green et al., "Trends in long-term opioid therapy for chronic non-cancer pain," *Pharmacoepidemiology and drug safety*, 2009, *18* (12), 1166–1175.

Butler, S. F., T. A. Cassidy, H. Chilcoat, R. A. Black, C. Landau, S. H. Budman, and P. M. Coplan, "Abuse rates and routes of administration of reformulated extended-release oxycodone: initial findings from a sentinel surveillance sample of individuals assessed for substance abuse treatment," *The Journal of Pain*, 2013, *14* (4), 351–358.

Case, Anne and Angus Deaton, "Mortality and morbidity in the 21st century," *Brookings papers on economic activity*, 2017, *2017*, 397.

_ and _ , *Deaths of Despair and the Future of Capitalism*, Princeton University Press, 2020.

Castilla, C. and D. M. A. Murphy, "Bidirectional Intimate Partner Violence: Evidence from a List Experiment in Kenya," 2022. Working Paper.

_ , F. Aqeel, and D. M. A. Murphy, "Tipples and Quarrels in the Household: The Effect of an Alcohol Mitigation Intervention on Intimate Partner Violence," 2022. Working Paper.

Chalfin, A., S. Danagoulian, and M. Deza, "COVID-19 Has Strengthened the Relationship Between Alcohol Consumption and Domestic Violence," Technical Report, National Bureau of Economic Research 2021.

Cho, David, Daniel I. Garcia, Joshua Montes, and Alison Weingarden, "Labor Market Effects of the Oxycodone-Heroin Epidemic," Technical Report 2021-025, Board of Governors of the Federal Reserve System, Washington 2021.

Coplan, P. M., H. D. Chilcoat, S. F. Butler, E. M. Sellers, A. Kadakia, V. Harikrishnan, J. D. Haddox, and R. C. Dart, "The effect of an abuse-deterrent opioid formulation (OxyContin) on opioid abuse-related outcomes in the postmarketing setting," *Clinical Pharmacology and Therapeutics*, 2016, *100*, 275–286.

Currie, Janet, Jonas Jin, and Molly Schnell, "US Employment and Opioids: Is There a Connection?," in "Health and labor markets," Emerald Publishing Limited, 2019, pp. 253–280.

Dave, Dhaval M, Bilge Erten, David W Hummel, Pinar Keskin, and Shuo Zhang, "Fighting Abuse with Prescription Tracking: Mandatory Drug Monitoring and Intimate Partner Violence," Technical Report, National Bureau of Economic Research 2024.

Dave, Dhaval, Monica Deza, and Brady Horn, "Prescription drug monitoring programs, opioid abuse, and crime," *Southern Economic Journal*, 2021, *87* (3), 808–848.

El-Bassel, N., L. Gilbert, E. Wu, M. Chang, and J. Fontdevila, "Perpetration of Intimate Partner Violence Among Men in Methadone Treatment Programs in New York City," *American Journal of Public Health*, 2007, *97*, 1230–1232.

Erten, Bilge and Pinar Keskin, "For better or for worse?: Education and the prevalence of domestic violence in Turkey," *American Economic Journal: Applied Economics*, 2018, *10* (1), 64–105.

_ and _ , "Trade-offs? The impact of WTO accession on intimate partner violence in Cambodia," *The Review of Economics and Statistics*, 2021, pp. 1–40.

Evans, M. F., M. C. Harris, and L. M. Kessler, "The hazards of unwinding the prescription opioid epidemic: Implications for child maltreatment," *American Economic Journal: Economic Policy*, 2022, *14* (4), 192–231.

Evans, W. N., E. M. J. Lieber, and P. Power, "How the Reformulation of OxyContin Ignited the Heroin Epidemic," *The Review of Economics and Statistics*, 2019, *101*, 1–15.

Fone, Zachary S, Joseph J Sabia, and Resul Cesur, "The unintended effects of minimum wage increases on crime," *Journal of Public Economics*, 2023, *219*, 104780.

Freedman, Matthew and Emily G Owens, "Low-income housing development and crime," *Journal of Urban Economics*, 2011, *70* (2-3), 115–131.

Gihleb, Rania, Osea Giuntella, and Ning Zhang, "The Effect of Mandatory-Access Prescription Drug Monitoring Programs on Foster Care Admissions," *Journal of Human Resources*, 2022, *57* (1), 217–240.

Gupta, Sumedha and Bhashkar Mazumder, "The Effects of Prescription Drug Monitoring Programs on Labor Market Activity and Credit Outcomes," *Federal Reserve Bank of Chicago Working Paper*, 2023.

Harris, Matthew C, Lawrence M Kessler, Matthew N Murray, and Beth Glenn, "Prescription opioids and labor market pains: the effect of schedule II opioids on labor force participation and unemployment," *Journal of Human Resources*, 2020, *55* (4), 1319–1364.

Hughes, A., M. Dank, L. Wood, and M. Rashid, "The Socio-Ecological Intersections of the Opioid Epidemic and Violence Against Women: A Scoping Review," 2019.

Humphreys, K., C. L. Shover, C. M. Andrews, A. S. B. Bohnert, M. L. Brandeau, J. P. Caulkins, J. H. Chen, M. F. Cuéllar, Y. L. Hurd, D. N. Juurlink et al., "Responding to the opioid crisis in North America and beyond: recommendations of the Stanford–Lancet Commission," *The Lancet*, 2022, *399* (10324), 555–604.

Hwang, C. S., H. Chang, and G. C. Alexander, "Impact of abuse-deterrent OxyContin on prescription opioid utilization," *Pharmacoepidemiology and Drug Safety*, 2015, *24*, 197–204.

Jr, Gery P Guy, Kun Zhang, Michele K Bohm, Jan Losby, Brian Lewis, Randall Young, Louise B Murphy, and Deborah Dowell, "Vital signs: changes in opioid prescribing in the United States, 2006–2015," *Morbidity and Mortality Weekly Report*, 2017, *66* (26), 697.

Kim, Bokyung, "Must-access prescription drug monitoring programs and the opioid overdose epidemic: The unintended consequences," *Journal of Health Economics*, 2021, *75*, 102408.

Maclean, Johanna Catherine, Justine Mallatt, Christopher J Ruhm, and Kosali Simon, "The opioid crisis, health, healthcare, and crime: A review of quasi-experimental economic studies," *The ANNALS of the American Academy of Political and Social Science*, 2022, *703* (1), 15–49.

Mallatt, J., "The Effect of Prescription Drug Monitoring Programs on Opioid Prescriptions and Heroin Crime Rates," 2018.

Markowitz, Sara, "The price of alcohol, wife abuse, and husband abuse, 2000," *Southern Economic Journal*, 2000, *67*, 279–303.

Meinhofer, Angélica, "Prescription drug monitoring programs: the role of asymmetric information on drug availability and abuse," *American Journal of Health Economics*, 2018, *4* (4), 504–526.

Metcalf, Gilbert E and Qitong Wang, "Abandoned by Coal, Swallowed by Opioids?," Technical Report, National Bureau of Economic Research 2019.

Monnat, Shannon M, "Factors associated with county-level differences in US drug-related mortality rates," *American journal of preventive medicine*, 2018, *54* (5), 611–619.

National Academies of Sciences, Engineering, and Medicine and others, *Pain management and the opioid epidemic: balancing societal and individual benefits and risks of prescription opioid use*, National Academies Press, 2017.

Packard, G. and C. Warshaw, "Framing the issues: Looking at the opioid epidemic in the context of trauma and domestic violence," 2018.

Palmer, Roy E, David S Carrell, David Cronkite, Kathleen Saunders, David E Gross, Elizabeth Masters, Sean Donevan, Timothy R Hylan, and Michael Von Kroff, "The prevalence of problem opioid use in patients receiving chronic opioid therapy: computer-assisted review of electronic health record clinical notes," *Pain*, 2015, *156* (7), 1208–1214.

Park, Sujeong and David Powell, "Is the rise in illicit opioids affecting labor supply and disability claiming rates?," *Journal of Health Economics*, 2021, *76*, 102430.

Peterson, C., M. C. Kearns, W. L. McIntosh, L. F. Estefan, C. Nicolaidis, K. E. McCollister, A. Gordon, and C. Florence, "Lifetime Economic Burden of Intimate Partner Violence Among U.S. Adults," *American Journal of Preventive Medicine*, 2018, *55*, 433–444.

Powell, D. and R. L. Pacula, "The Evolving Consequences of OxyContin Reformulation on Drug Overdoses," *American Journal of Health Economics*, 2021, *7*, 41–67.

Pryor, C., J. H. Boman IV, and P. l. Hemez, "Using arrest and prescription data to examine the relationship between intimate partner violence and opioid prescriptions in the United States, 2006-2012," *Drug and alcohol dependence*, 2021, *218*, 108389.

Roth, Jonathan, "Pretest with caution: Event-study estimates after testing for parallel trends," *American Economic Review: Insights*, 2022, *4* (3), 305–322.

Stevenson, Betsey and Justin Wolfers, "Bargaining in the Shadow of the Law: Divorce Laws and Family Distress," *Quarterly Journal of Economics*, 2006, *121* (1), 267–288.

Stone, R. and E. F. Rothman, "Opioid Use and Intimate Partner Violence: A Systematic Review," *Current Epidemiology Reports*, 2019, *6*, 215–230.

Thomas, Shaun A and Edward S Shihadeh, "Institutional isolation and crime: The mediating effect of disengaged youth on levels of crime," *Social Science Research*, 2013, *42* (5), 1167–1179.

Tran, A., L. Lin, E. J. Nehl, C. L. Talley, K. L. Dunkle, and F. Y. Wong, "Prevalence of Substance Use and Intimate Partner Violence in a Sample of A/PI MSM," *Journal of Interpersonal Violence*, 2014, *29*, 2054–2067.

Unick, George, Daniel Rosenblum, Sarah Mars, and Daniel Ciccarone, "The relationship between US heroin market dynamics and heroin-related overdose, 1992–2008," *Addiction*, 2014, *109* (11), 1889–1898.

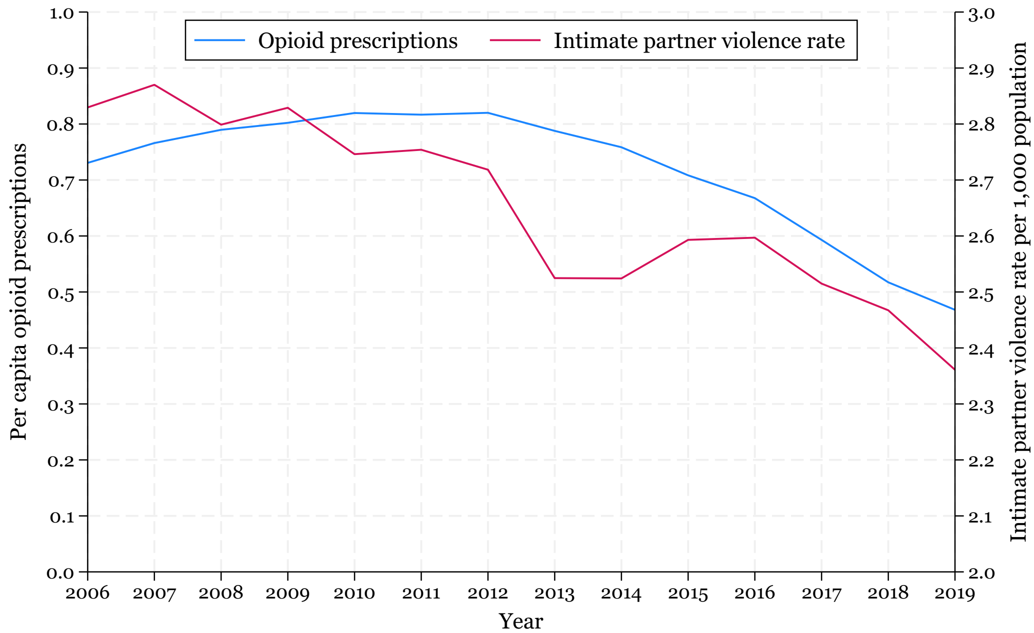
Volkow, N. D., "America's addiction to opioids: Heroin and prescription drug abuse," *Senate Caucus on International Narcotics Control*, 2014, *14*, 1–16.

Warshaw, C., E. Lyon, P. J. Bland, H. Phillips, and M. Hooper, "Mental Health and Substance Use Coercion Surveys: Report from the National Center on Domestic Violence, Trauma & Mental Health and the National Domestic Violence Hotline," Technical Report, National Center on Domestic Violence, Trauma, & Mental Health and the National Domestic Violence Hotline 2014.

World Health Organization, *Global and regional estimates of violence against women: prevalence and health effects of intimate partner violence and non-partner sexual violence* 2013.

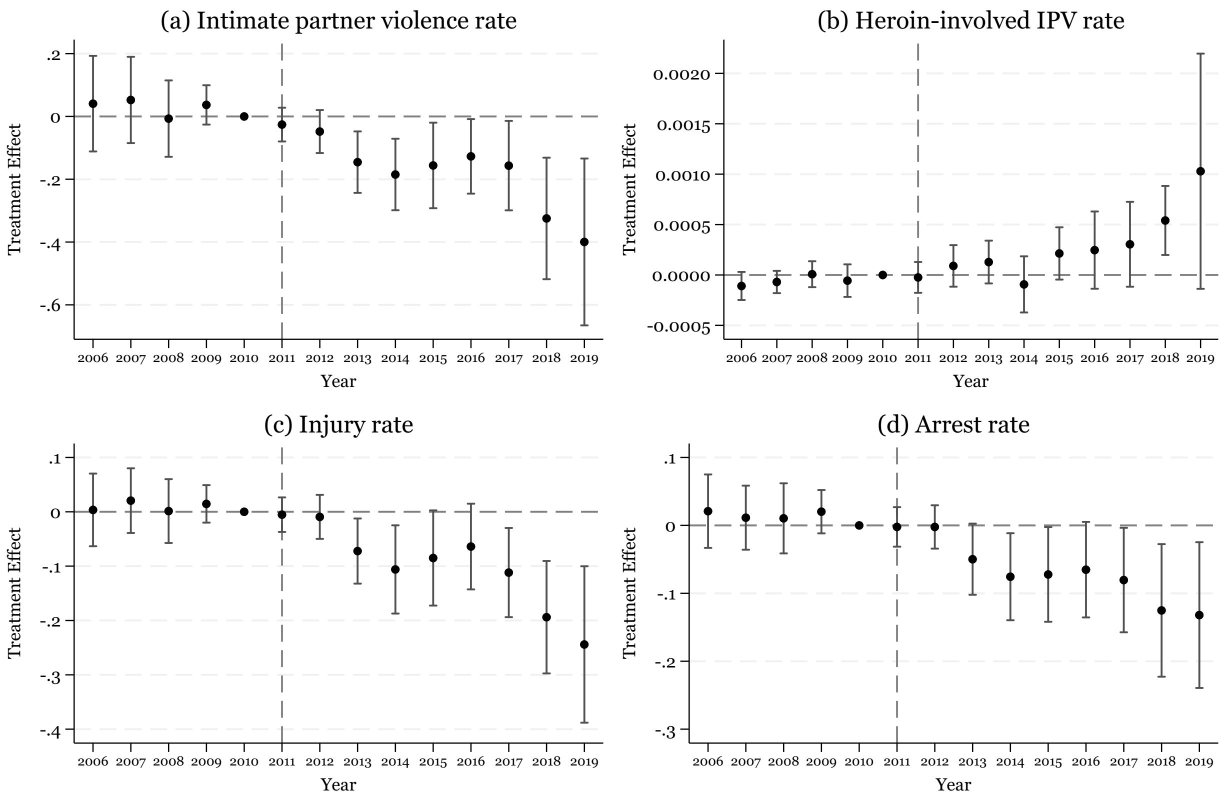
Zee, A. Van, "The promotion and marketing of oxycontin: commercial triumph, public health tragedy," *American journal of public health*, 2009, *99* (2), 221–227.

Figure 1: Opioid Prescriptions per capita and Intimate Partner Violence Rate



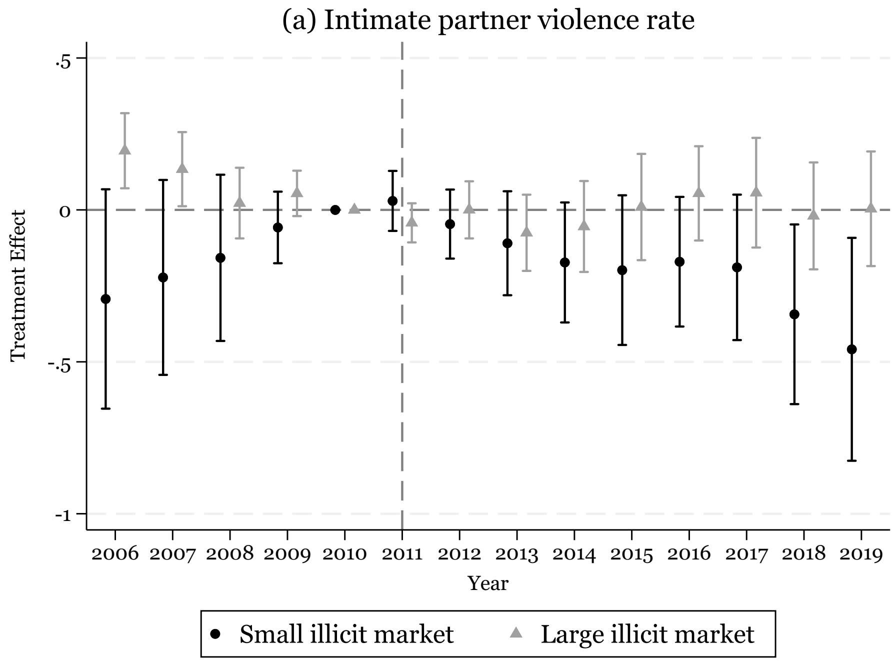
Note: Figure depicts annual opioid prescriptions per capita reported by the CDC and the intimate partner violence rate per 1,000 population calculated from the 2006–2019 NIBRS. Opioid prescriptions per capita refer to the population-weighted median per capita prescriptions in a given year. Intimate partner violence rate refers to the annual means based on the number of intimate partner violence incidents per 1,000 population.

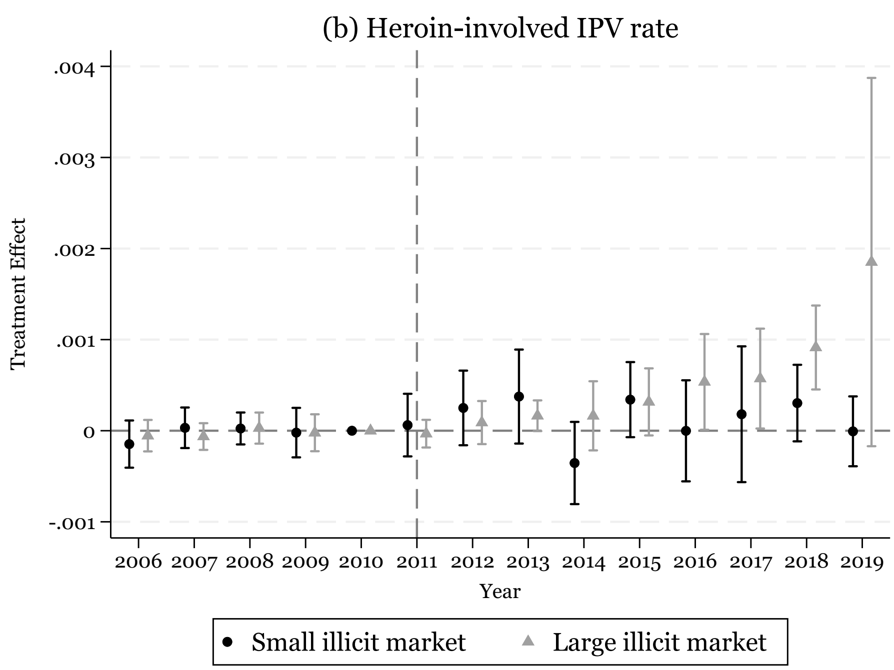
Figure 2: The Effects of OxyContin Reformulation on IPV Rates over time



Note: Data are from the 2006–2019 NIBRS. Event-study plots showing the response of IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1,000 population reported by female victims at the county level (N=12,516 county-years) to OxyContin reformulation in 2010. The population-weighted mean opioid prescriptions per capita during the pre-reformulation period (2006–2009) is standardized (mean=0, std=1). Each figure reports treatment effect estimates and 95 percent confidence intervals with 2010, the reformulation year, normalized to zero. Specifications include county and year fixed effects, county-level covariates (percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population under age 19, between 20 and 24, between 25 and 34, between 35 and 44, between 45 and 54, and between 55 and 64; unemployment and labor force participation rates), initial (2006) shares of the population without any college education and of employment share in mining, both interacted with year fixed effects, and state-level policies (indicators for a PDMP of any form and a medical marijuana law). Standard errors are clustered at the county level.

Figure 3: Heterogeneity Analyses by Size of Illicit Opioid Market Activity



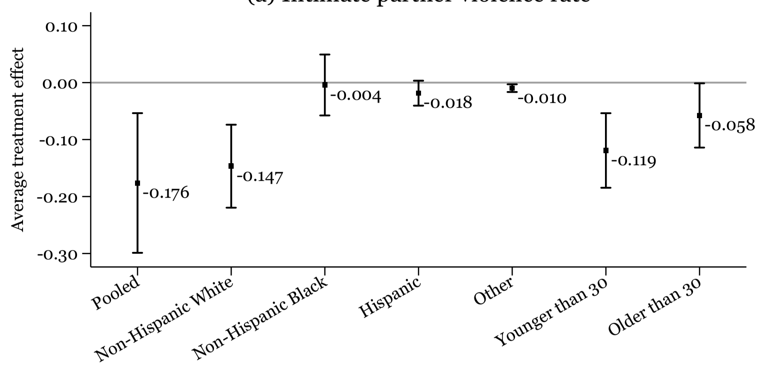


Note: Data are from the 2006–2019 NIBRS. The figure presents subgroup analyses by the size of the illicit opioid market activity in a state, which is proxied by the share of deaths due to illicit or synthetic opioids in total deaths due to opioids over the period 2006 to 2009, prior to OxyContin reformulation in 2010. The coefficient estimates show treatment effects on IPV rate and heroin-involved IPV rate per 1,000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates.

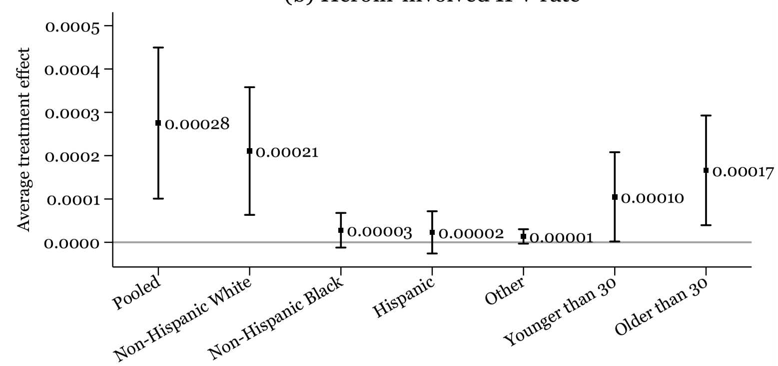
Figure 4: Heterogeneity Analyses

Panel A: Heterogeneity by victim characteristics

(a) Intimate partner violence rate

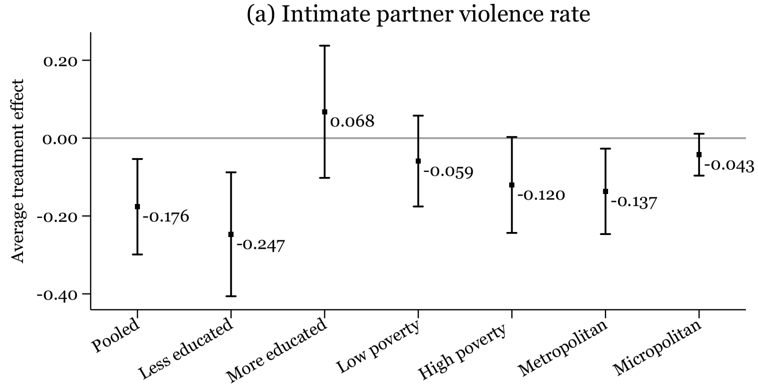


(b) Heroin-involved IPV rate

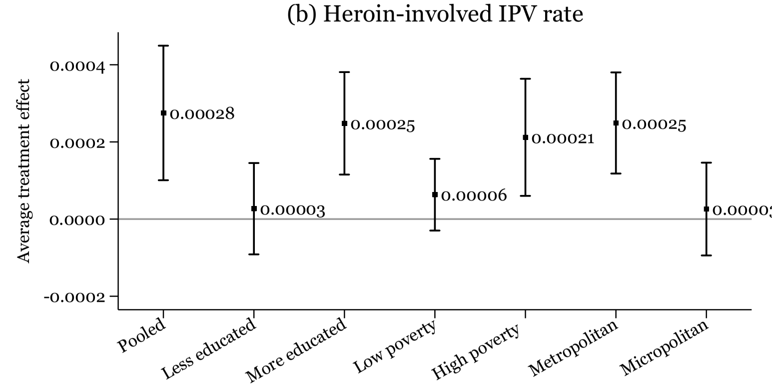


Panel B: Heterogeneity by county characteristics

(a) Intimate partner violence rate



(b) Heroin-involved IPV rate



Note: Data are from the 2006–2019 NIBRS. The figure presents victim-level subgroup analyses in Panel A and county-level subgroup analyses in Panel B, displaying estimated treatment effects on IPV rate and heroin-involved IPV rate per 1,000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates.

Table 1: The Effects of OxyContin Reformulation on IPV Rates

	(1)	(2)	(3)	(4)	(5)
Intimate partner violence rate	-0.2081***	-0.1756***	-0.1764***	-0.1767***	-0.1762***
	(0.0558)	(0.0578)	(0.0618)	(0.0619)	(0.0625)
Observations	12,516	12,516	12,516	12,516	12,516
Mean in 2006-09	2.8319	2.8319	2.8319	2.8319	2.8319
Heroin-involved IPV rate	0.0002**	0.0002**	0.0003***	0.0003***	0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	12,516	12,516	12,516	12,516	12,516
Mean in 2006-09	0.0001	0.0001	0.0001	0.0001	0.0001
Injury rate	-0.0823***	-0.0783**	-0.0918***	-0.0915***	-0.0920***
	(0.0286)	(0.0339)	(0.0349)	(0.0347)	(0.0351)
Observations	12,516	12,516	12,516	12,516	12,516
Mean in 2006-09	1.3857	1.3857	1.3857	1.3857	1.3857
Arrest rate	-0.0740***	-0.0636**	-0.0708**	-0.0712**	-0.0716**
	(0.0248)	(0.0285)	(0.0294)	(0.0293)	(0.0293)
Observations	12,516	12,516	12,516	12,516	12,516
Mean in 2006-09	1.5122	1.5122	1.5122	1.5122	1.5122
County and year fixed effects	✓	✓	✓	✓	✓
Time-varying county socioeconomic controls	×	✓	✓	✓	✓
Initial county characteristics X year fixed effects	×	×	✓	✓	✓
Any Prescription Drug Monitoring Program	×	×	×	✓	✓
Medical marijuana law	×	×	×	×	✓

Notes: Data are from the 2006–2019 NIBRS. Estimates of average treatment effects of the OxyContin reformulation on the IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1,000 population reported by female victims at the county level (N=12,516 county-years). The population-weighted mean opioid prescriptions per capita during the pre-reformulation period (2006–2009) is standardized (mean=0, std=1). All specifications include county and year fixed effects. Column (2) adds county-level demographic, health, and economic controls (percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population under age 19, between 20 and 24, between 25 and 34, between 35 and 44, between 45 and 54, and between 55 and 64; unemployment and labor force participation rates), column (3) controls for initial (2006) shares of the population without any college education and of employment share in mining, both interacted with year fixed effects, column (4) controls for whether the state has a Prescription Drug Monitoring Program (PDMP) of any form, and column (5) controls for whether the state has a medical marijuana law. Outcome means for the pre-reformulation period (2006–2009) are listed in rows under standard errors. Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

APPENDIX

Appendix A Additional Data Sources

A.1 Additional county-level data

For heterogeneity analysis based on county characteristics, we collected data on average educational attainment, poverty rate, and metropolitan share of population. For educational attainment, we use the percentage of adults whose highest level of education is higher than high school degree. To account for the effect of urbanization, we classified counties into three categories based on their metropolitan status: metropolitan, micropolitan, or non-core. Metropolitan counties have a core urban area of 50,000 or more people. They are generally characterized by significant economic ties throughout the area, including social and economic integration, as indicated by commuting patterns. In contrast, micropolitan counties have an urban core of at least 10,000 (but less than 50,000) people. Non-core counties do not have an urban core of 10,000 or more people and thus represent the most rural counties. We use the share of metropolitan population for our subgroup analyses.

In conducting our robustness checks, we compiled housing market indicators at the county level. These include the Housing Market Index (HHI) from the Federal Housing Finance Agency, and the mortgage delinquency rate based on mortgages delinquent by 90 days or more from the Consumer Financial Protection Bureau. These data span the period 2008–2019.

The coverage indicator represents the effective coverage of reporting of IPV by agencies for a county in a given year, with higher values representing more complete coverage. Following Fone et al. (2023), the coverage indicator is calculated as:

$$CI_{c,t} = (1 - \sum_{a=1}^{n_{c,t}} \left(\frac{A_{a,c,t}}{T_{c,t}} \cdot \frac{12 - M_{a,t}}{12} \right)) \times 100 \tag{3}$$

where $CI_{c,t}$ is the coverage indicator for county c in year t; $n_{c,t}$ is the number of agencies in county c at time t; $A_{a,c,t}$ is the population of agency a in county c in year t; $T_{c,t}$ is the total population in county c in year t; and $M_{a,t}$ is the number of months agency a reported in year t.

A.2 Additional state-level data

Our baseline analysis controls for two state-level policies: indicators for whether the state has a Prescription Drug Monitoring Program (PDMP) of any form and for whether it has a medical marijuana law. Specifically, we accounted for whether a Prescription Drug Monitoring Program (PDMP) law was in effect in a given state during a particular year. These laws establish electronic databases that track controlled substance prescriptions within a state, providing valuable and timely information to health authorities about prescribing and patient behaviors that may lead to substance misuse. We accounted for the presence of a PDMP of any form, whether it involves voluntary database access or requires mandatory queries before prescribing or dispensing controlled substances. We also controlled for whether the state has a medical marijuana law in place considering marijuana as a therapeutic substitute to opioid consumption.

In our robustness analysis, we included several state policies to control for state-level differences in social welfare and health policies. Our analysis incorporated Good Samaritan Laws that protect individuals who provide emergency aid during a medical emergency or call for help during a drug-related overdose, and Naloxone Laws that give legal protection to healthcare providers who prescribe or dispense naloxone. We also added indicators for whether a state decriminalized the use of marijuana, and whether a state has passed recreational marijuana laws to reflect the legal status of marijuana in a state for a given year. We also accounted for the Physical Examination Laws, which vary among states but require a licensed practitioner to examine a patient before prescribing medication. Next, we included a binary variable that indicates whether a state expanded Medicaid coverage under the Affordable Care Act (ACA) provisions. Moreover, we incorporated the State Earned Income Tax Credit (EITC) policy as a percentage of the federal EITC. This policy reduces the tax owed by low to moderate-income working individuals and couples on a dollar-for-dollar basis. Lastly, we used the police per capita as a control variable, reflecting the number of law enforcement officers per 1,000 residents in a state for a given year.

Moreover, at the state level, we have integrated controls for the rate of mortgage delinquency, specifically focusing on mortgages overdue by 90 days or more, utilizing data derived from the Residential Mortgage Performance Statistics.

Additional Tables

Table A1: Summary Statistics

	Pre-reformulation (2006–2009)	Post-reformulation (2010–2019)	Whole period (2006–2019)				
	Mean	Mean	Mean	SD	Min	Max	N
Intimate partner violence rate (per 1,000)	2.8319	2.5613	2.6517	1.9127	0	15.0699	12,516
Heroin-involved IPV rate (per 1,000)	0.0001	0.0005	0.0004	0.0034	0	0.125	12,516
Injury rate (per 1,000)	1.3857	1.3111	1.3353	0.9626	0	9.0921	12,516
Arrest rate (per 1,000)	1.5122	1.4274	1.4573	0.9814	0	7.9778	12,516
Percent female	0.5032	0.5013	0.5019	0.0174	0.2631	0.5642	12,516
Percent Black	0.0764	0.08	0.0788	0.1242	0.0011	0.7447	12,516
Percent White	0.8968	0.8881	0.891	0.1319	0.1914	0.9961	12,516
Percent Hispanic	0.0527	0.0639	0.0602	0.0791	0.0032	0.6446	12,516
Percent under age 0 to 19	0.2655	0.25	0.2551	0.034	0.1234	0.397	12,516
Percent age 20 to 24	0.0627	0.0642	0.0636	0.0257	0.0262	0.2833	12,516
Percent age 25 to 34	0.1144	0.1179	0.1168	0.0202	0.0531	0.2832	12,516
Percent age 35 to 44	0.1299	0.1167	0.1208	0.0156	0.0646	0.2031	12,516
Percent age 45 to 54	0.1509	0.1329	0.1392	0.0177	0.0586	0.2219	12,516
Percent age 55 to 64	0.1243	0.1403	0.1353	0.0213	0.0467	0.2139	12,516
Percent age over age 64	0.1522	0.178	0.1692	0.0426	0.0424	0.3788	12,516
Cancer deaths per 100,000 population	231.0289	237.7321	235.4256	66.4929	35.2602	697.6744	12,516
Unemployment rate	6.2791	5.6195	6.0619	2.9403	1.1	25.6	12,516
Labor force participation rate	0.6191	0.5914	0.6004	0.0838	0.2759	1.2668	12,516
Indicator for having medical marijuana law	0.1385	0.3528	0.2797	0.4489	0	1	12,516
Indicator for having a PDMP of any form	0.7199	0.9814	0.8968	0.3043	0	1	12,516
Initial percent without any college education (in 2006)	0.5112	0.511	0.511	0.1155	0.1501	0.7846	12,516
Initial percent of employment in mining (in 2006)	0.0071	0.007	0.007	0.027	0	0.2773	12,516

Notes: Data are from the 2006–2019 NIBRS. The table presents the means, standard deviations, minimum and maximum values, and the number of observations for variables used in the analysis at the county level (N=12,516 county-years).

Table A2: Pre-trend Analysis

	# pre-periods	# significant	Max | t |	Joint p-value	| t | for slope	$\gamma 0.5$	$\gamma 0.8$
Intimate partner violence rate	4	0	1.153	0.221	0.530	0.033	0.049
Heroin-involved IPV rate	4	0	1.486	0.324	1.069	0.000	0.000
Injury rate	4	0	0.844	0.483	0.396	0.014	0.022
Arrest rate	4	0	1.228	0.677	0.259	0.011	0.017

Notes: This table presents the pre-trend analysis of our main outcomes, as shown in Figure 2. Following the methodology of Roth (2022), it reports the individual significance and t-values of the pre-treatment coefficients, the p-values from a joint test of all pre-period coefficients, and the significance of the slope for the fitted line on the pre-period coefficients. The last two columns provide the slopes corresponding to the tests on linear violations of parallel trends that conventional pretests would detect 50% or 80% of the time ($\gamma 0.5$ and $\gamma 0.8$).

Table A3: Robustness Analysis-I

	IPV rate per 1,000 population	Heroin-involved IPV rate per 1,000 population	Injury rate per 1,000 population	Arrest rate per 1,000 population
Controlling for:				
Good Samaritan Laws	-0.1587***	0.0002**	-0.0796**	-0.0662**
	(0.0603)	(0.0001)	(0.0339)	(0.0287)
Naloxone Laws	-0.1590***	0.0002**	-0.0801**	-0.0675**
	(0.0602)	(0.0001)	(0.0338)	(0.0286)
Decriminalization of Marijuana	-0.1747***	0.0003***	-0.0904***	-0.0712**
	(0.0622)	(0.0001)	(0.0345)	(0.0292)
Recreational Marijuana Laws	-0.1573***	0.0002**	-0.0790**	-0.0653**
	(0.0598)	(0.0001)	(0.0334)	(0.0282)
Physical Examination Requirements	-0.1602***	0.0001**	-0.0821**	-0.0673**
	(0.0592)	(0.0001)	(0.0334)	(0.0277)
ACA Expansion	-0.1703***	0.0003***	-0.0904**	-0.0682**
	(0.0620)	(0.0001)	(0.0352)	(0.0285)
EITC Policy	-0.1678***	0.0003***	-0.0887**	-0.0675**
	(0.0609)	(0.0001)	(0.0346)	(0.0281)

Notes: Data are from the 2006–2019 NIBRS. Analyses showing the IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1,000 population reported by female victims at the county level (N=12,516 county-years). The population-weighted mean opioid prescriptions per capita during the pre-reformulation period (2006–2009) is standardized (mean=0, std=1). Specifications include county and year fixed effects, county-level covariates (percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population under age 19, between 20 and 24, between 25 and 34, between 35 and 44, between 45 and 54, and between 55 and 64; unemployment and labor force participation rates), initial (2006) shares of the population without any college education and of employment share in mining, both interacted with year fixed effects, and state-level policies (indicators for a PDMP of any form and a medical marijuana law). Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A4: Robustness Analysis-II

	IPV rate per 1,000 population	Heroin-involved IPV rate per 1,000 population	Injury rate per 1,000 population	Arrest rate per 1,000 population
Clustering at the State Level	-0.1762**	0.0003**	-0.0920**	-0.0716**
	(0.0805)	(0.0001)	(0.0425)	(0.0330)
Observations	12516	12516	12516	12516
Controlling for:				
Census division × post-reformulation dummy	-0.1586*	0.0003***	-0.0797	-0.1089***
	(0.0851)	(0.0001)	(0.0485)	(0.0371)
Observations	12516	12516	12516	12516
Police per capita (in logs)	-0.1620**	0.0003***	-0.0866**	-0.0671**
	(0.0632)	(0.0001)	(0.0360)	(0.0296)
Observations	12516	12516	12516	12516
Number of agencies reporting at the county	-0.2105***	0.0003***	-0.1087***	-0.0849***
	(0.0623)	(0.0001)	(0.0360)	(0.0291)
Observations	12516	12516	12516	12516
Dropping counties below 65% coverage rate	-0.1977***	0.0003***	-0.1104***	-0.0831***
	(0.0663)	(0.0001)	(0.0382)	(0.0319)
Observations	9316	9316	9316	9316

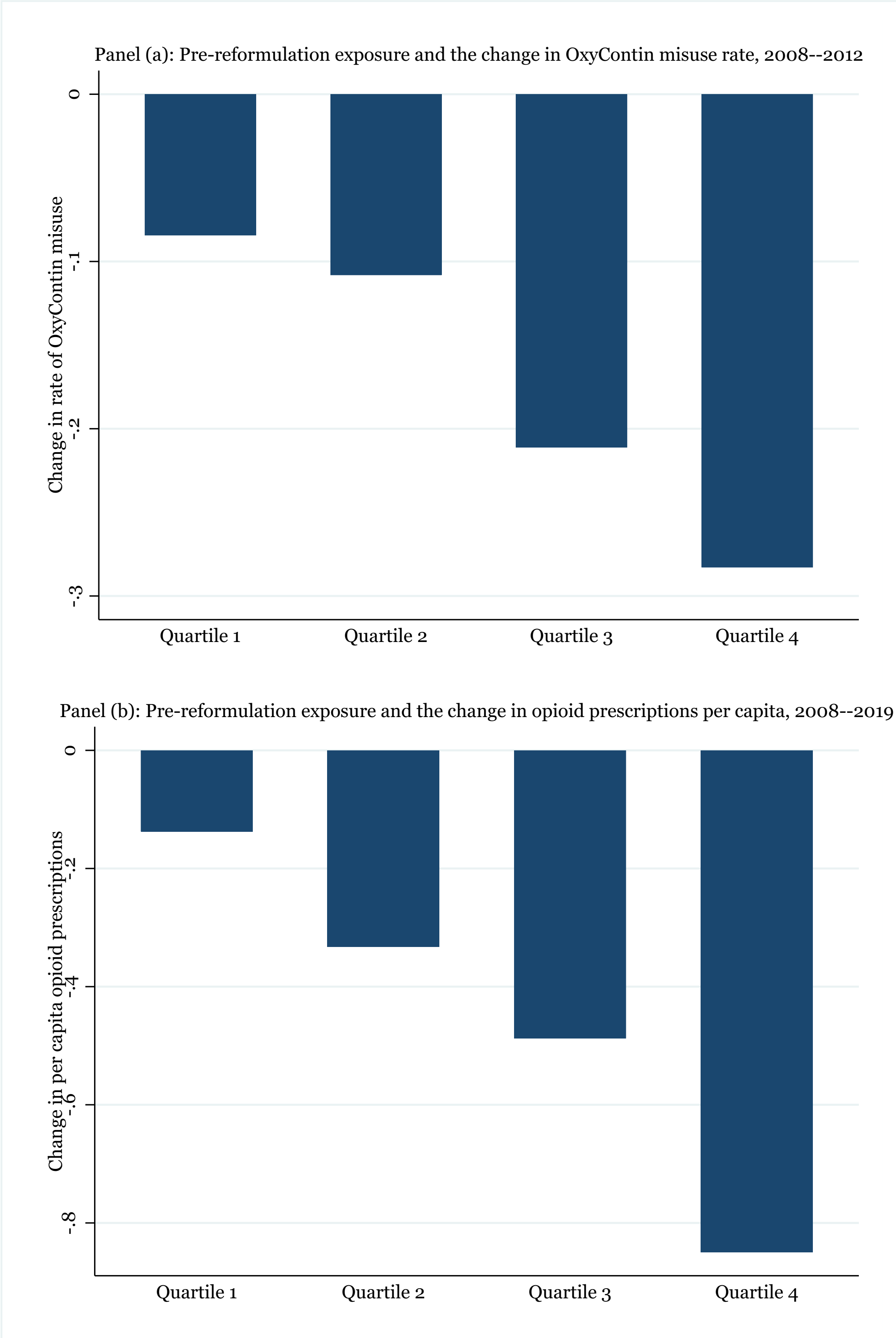
Notes: Data are from the 2006–2019 NIBRS. Analyses showing the IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1,000 population reported by female victims at the county level. County-year observations are noted for each regression. The population-weighted mean opioid prescriptions per capita during the pre-reformulation period (2006–2009) at county level and state-level Oxycontin misuse is standardized (mean=0, std=1). Specifications include county and year fixed effects, county-level covariates (percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population under age 19, between 20 and 24, between 25 and 34, between 35 and 44, between 45 and 54, and between 55 and 64; unemployment and labor force participation rates), initial (2006) shares of the population without any college education and of employment share in mining, both interacted with year fixed effects, and state-level policies (indicators for a PDMP of any form and a medical marijuana law). Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A5: Robustness Analysis-III

	IPV rate per 1,000 population	Heroin-involved IPV rate per 1,000 population	Injury rate per 1,000 population	Arrest rate per 1,000 population
Controlling for:				
Intimate partner violence rate	-0.2081***	-0.1756***	-0.1764***	-0.1767***
-0.1762***				
	(0.0558)	(0.0578)	(0.0618)	(0.0619)
(0.0625)				
	12,516	12,516	12,516	12,516
12,516				
	2.8319	2.8319	2.8319	2.8319
2.8319				
Mortgage delinquency rate	-0.1861***	0.0003***	-0.0983***	-0.0724**
	(0.0659)	(0.0001)	(0.0369)	(0.0312)
Observations	11,072	11,072	11,072	11,072
State level mortgage delinquency rate	-0.1645***	0.0003***	-0.0962***	-0.0801***
	(0.0599)	(0.0001)	(0.0369)	(0.0303)
Observations	10,373	10,373	10,373	10,373

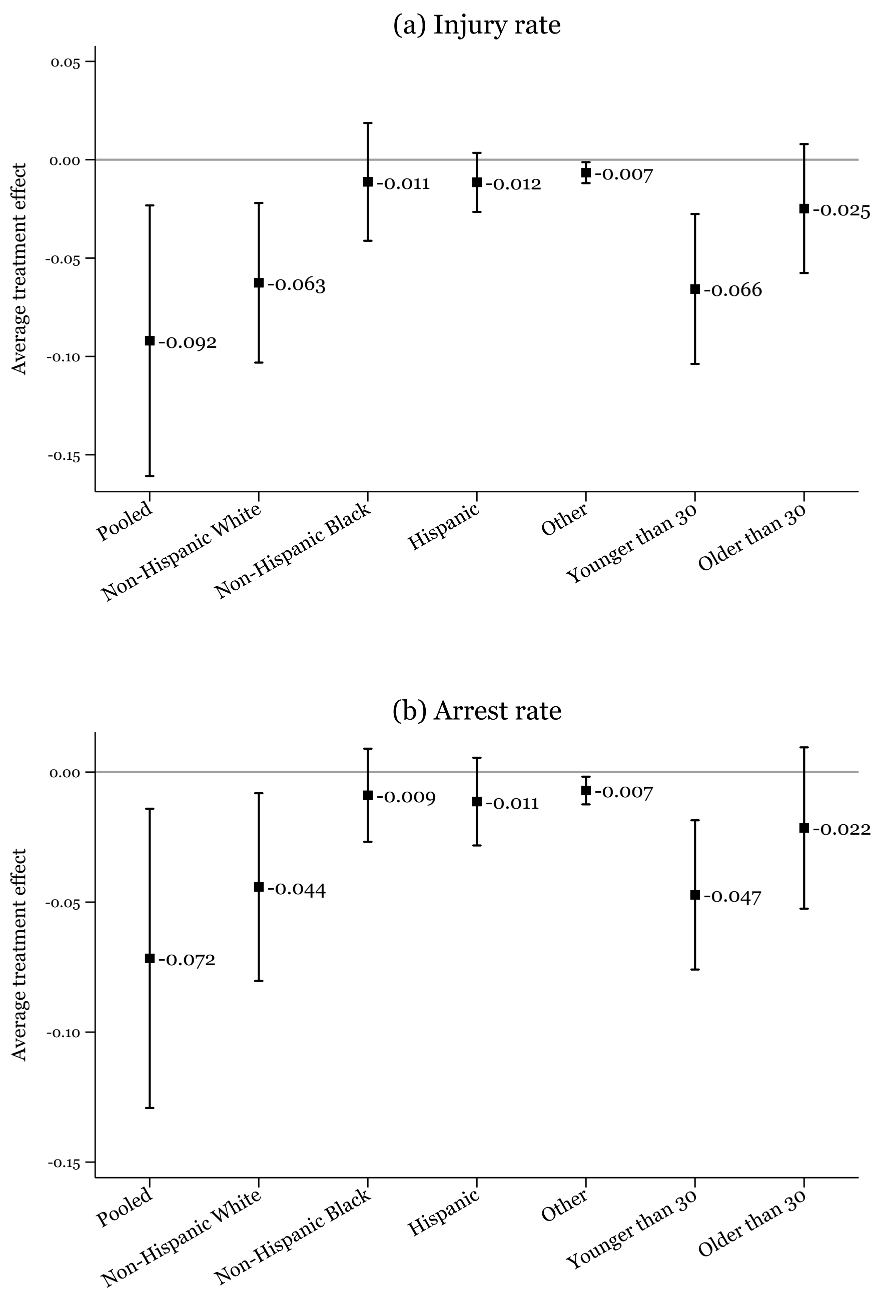
Notes: Data are from the 2006–2019 NIBRS. Analyses showing the IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1,000 population reported by female victims at the county level. The population-weighted mean opioid prescriptions per capita during the pre-reformulation period (2006–2009) is standardized (mean=0, std=1). Specifications include county and year fixed effects, county-level covariates (percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population under age 19, between 20 and 24, between 25 and 34, between 35 and 44, between 45 and 54, and between 55 and 64; unemployment and labor force participation rates), initial (2006) shares of the population without any college education and of employment share in mining, both interacted with year fixed effects, and state-level policies (indicators for a PDMP of any form and a medical marijuana law). Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Figure A1: Relationship between Pre-reformulation Exposure and the Change in Prescription Opioid Misuse



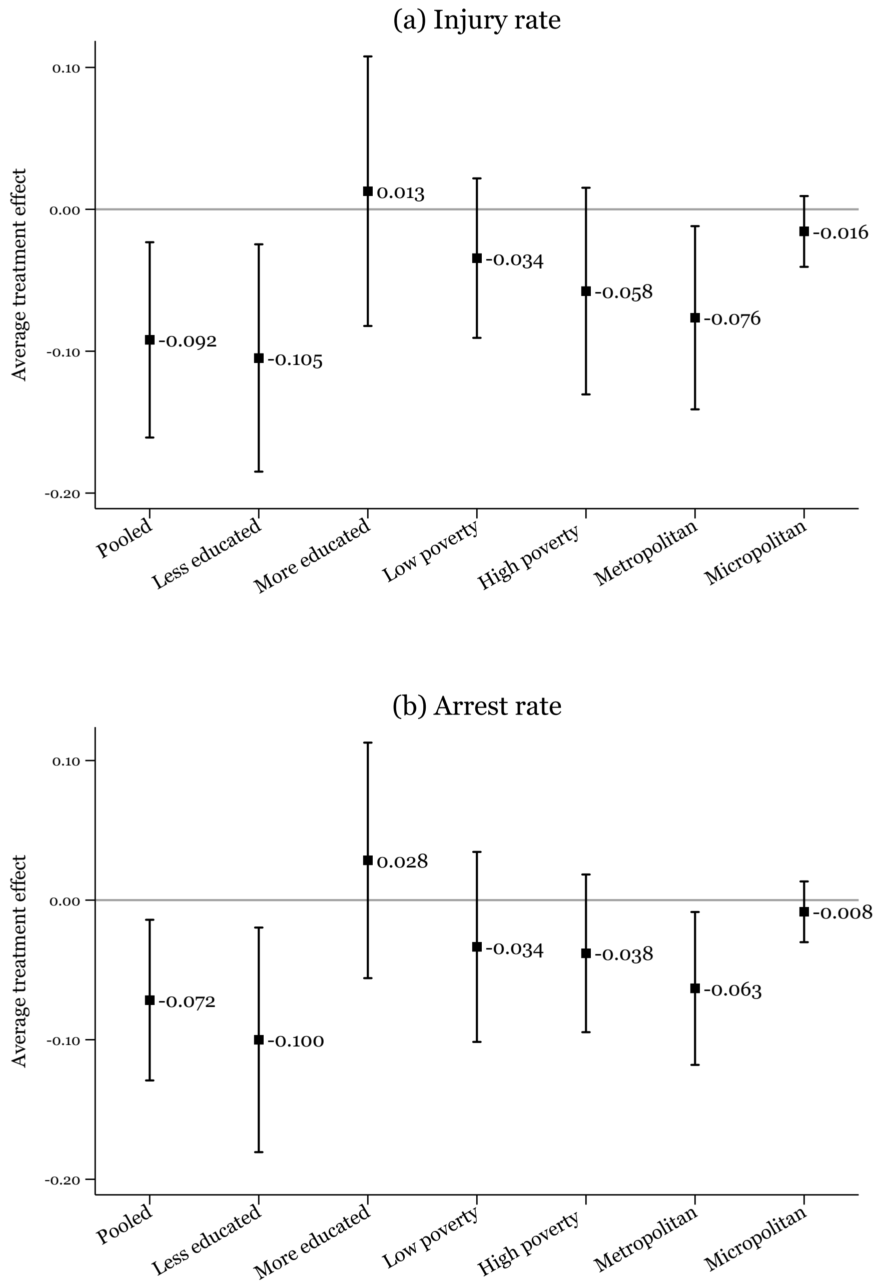
Note: We use the population-weighted mean per capita opioid prescriptions in the county from 2006 to 2009, pre-reformulation exposure period, to create quartiles of exposure (with the quartile 1 representing lowest average exposure and quartile 4 highest average exposure). Panel (a) plots the reductions in the state-level population-weighted average rate of OxyContin misuse from 2008 to 2012, while Panel B plots the reductions in the county-level population-weighted average per capita Schedule II opioid prescriptions based on the CDC data from 2008 to 2019.

Figure A2: Heterogeneity Analyses by Victim Characteristics



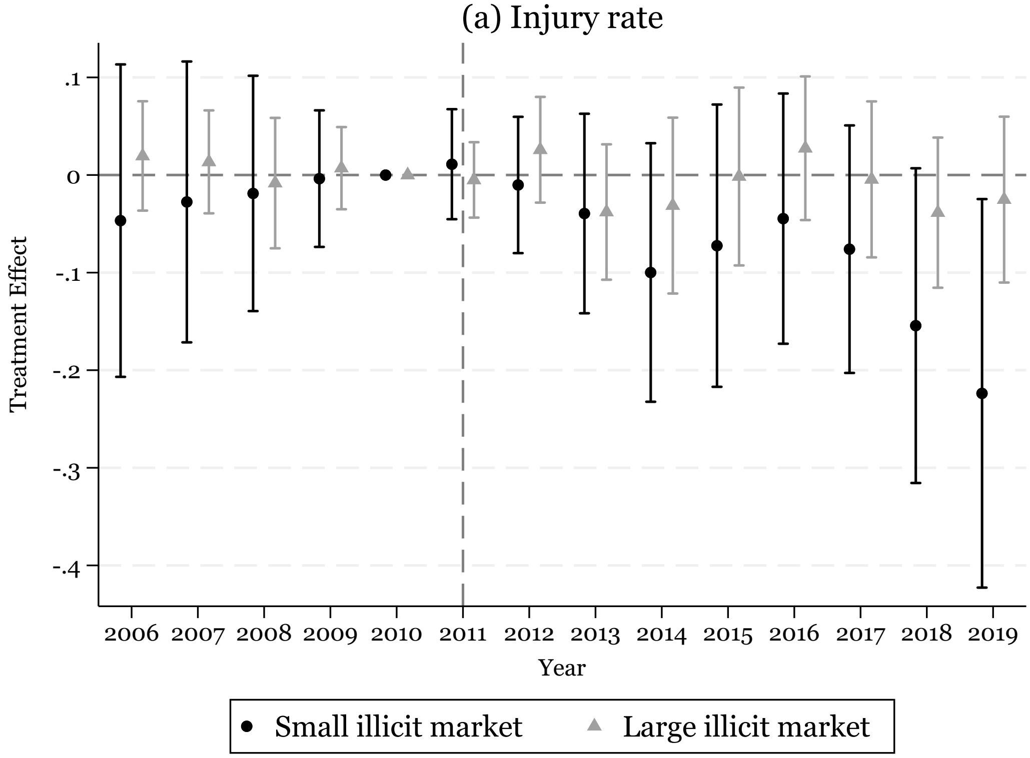
Note: Data are from the 2006–2019 NIBRS. The figure presents victim-level subgroup analyses, displaying estimated treatment effects on injury rate and IPV arrest rate per 1,000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates.

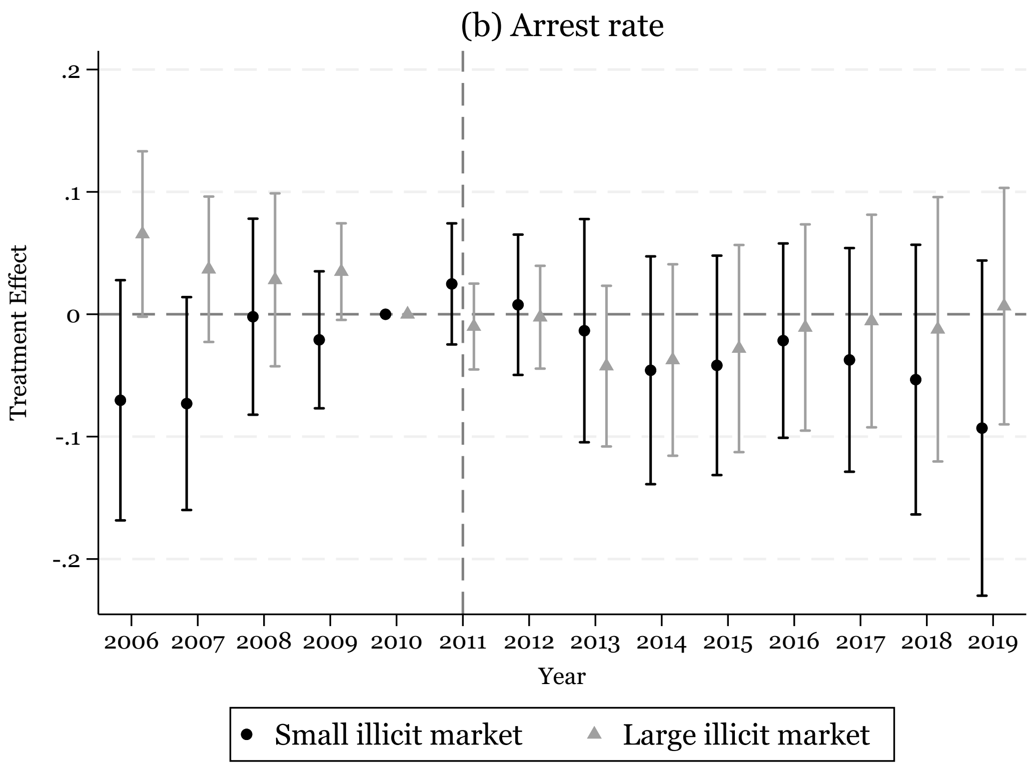
Figure A3: Heterogeneity Analyses by County Characteristics



Note: Data are from the 2006–2019 NIBRS. The figure presents county-level subgroup analyses, displaying estimated treatment effects on injury rate and IPV arrest rate per 1,000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates.

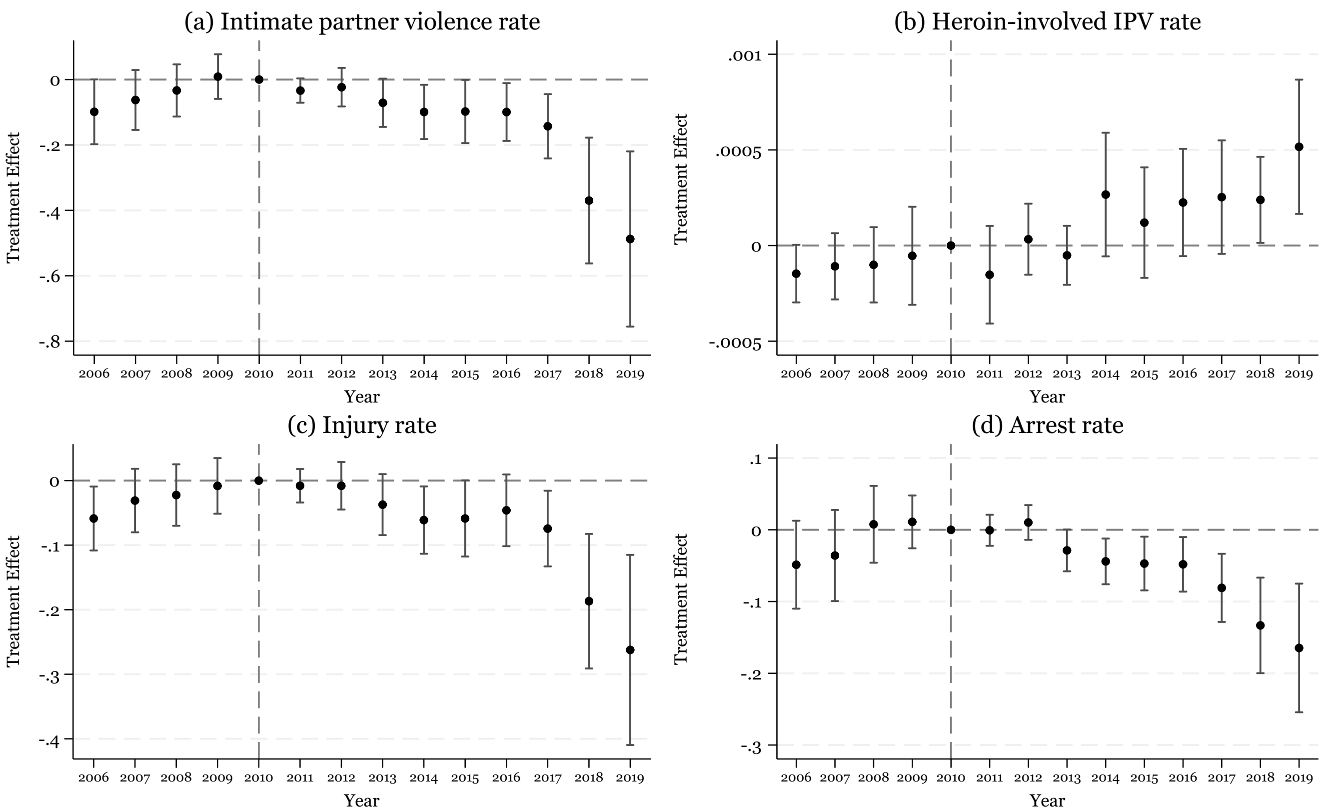
Figure A4: Heterogeneity Analyses by Size of Illicit Opioid Market Activity





Note: Data are from the 2006–2019 NIBRS. The figure presents subgroup analyses by the size of the illicit opioid market activity in a state, which is proxied by the share of deaths due to illicit or synthetic opioids in total deaths due to opioids over the period 2006 to 2009, prior to OxyContin reformulation in 2010. The coefficient estimates show treatment effects on injury rate and arrest rate per 1,000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates.

Figure A5: Event Study Results using Alpert et al. (2018) OxyContin Misuse Measure



Note: Data are from the 2006–2019 NIBRS. Event-study plots showing the response of IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1,000 population reported by female victims at the county level (N=12,516 county-years) to OxyContin reformulation in 2010. State-level OxyContin misuse prior to the intervention is obtained from Alpert et al. (2018), and is standardized. Each figure reports treatment effect estimates and 95 percent confidence intervals with 2010, the reformulation year, normalized to zero. Specifications include county and year fixed effects, county-level covariates (percent female, White, Black, Hispanic population; number of cancer deaths per 100,000 population; percent population under age 19, between 20 and 24, between 25 and 34, between 35 and 44, between 45 and 54, and between 55 and 64; unemployment and labor force participation rates), initial (2006) shares of the population without any college education and of employment share in mining, both interacted with year fixed effects, and state-level policies (indicators for a PDMP of any form and a medical marijuana law). Standard errors are clustered at the county level.