Journal of Health Economics 104 (2025) 103073

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/jhe

From addiction to aggression: The spillover effects of opioid policies on intimate partner violence

Dhaval Dave [a,b], Bilge Erten [c,d], Pinar Keskin [d,e,*], Shuo Zhang [f]

[a] Department of Economics, 175 Forest Street, Bentley University, Waltham, MA 02452, United States of America
[b] NBER, United States of America
[c] Department of Economics, 43 Leon Street, 312A Lake Hall, Northeastern University, Boston, MA 02115, United States of America
[d] IZA, Germany
[e] Department of Economics, Pendleton East, Wellesley College, 106 Central Street, Wellesley, MA 02481, United States of America
[f] Department of Economics, 43 Leon Street, 319 Lake Hall, Northeastern University, Boston, MA 02115, United States of America

ARTICLE INFO

JEL classification:
J12
I12
I18

Keywords:
Opioid addiction
Intimate partner violence
Illicit market activity
The United States

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 smaller illicit drug markets, while states with larger 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.

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

* Corresponding author.
E-mail addresses: ddave@bentley.edu (D. Dave), b.erten@northeastern.edu (B. Erten), pinar.keskin@wellesley.edu (P. Keskin), s.zhang@northeastern.edu (S. Zhang).

[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).

https://doi.org/10.1016/j.jhealeco.2025.103073
Received 6 March 2025; Received in revised form 3 October 2025; Accepted 7 October 2025
Available online 24 October 2025

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 exposed to prescription opioids prior to reformulation experienced differential changes in IPV outcomes after the reformulation. The NIBRS data also enable us to identify incidents in which the perpetrator was suspected of using heroin. We use this information to construct a county-level measure of heroin-involved IPV: the number of IPV incidents where law enforcement reported suspected heroin use by the perpetrator, scaled per 1000 residents in the county.

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 7.5 percent annually in the IPV rate following OxyContin's reformulation. We also document corollary declines in injuries and arrests related to IPV (7.3 percent and 6.1 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., 2025; 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. While IPV and child maltreatment are both different forms of family abuse, empirically the rates of IPV and child maltreatment within a county are almost orthogonal to each other.[2] Moreover, a comparison of our NIBRS sample to the sample in Evans et al. (2022) reveals that the counties in our sample are more rural and have smaller illicit markets (Appendix Table A1). These differences in sample composition may help explain the contrasting findings for different forms of family abuse between the two studies. Specifically, the relatively more rural composition of our sample, characterized by a smaller illicit drug market, could result in lower rates of substitution into heroin or other illicit opioids, thereby attenuating the reformulation's adverse spillovers on family abuse. In contrast, the more urban sample in Evans et al. (2022), with greater access to illicit markets, may have experienced higher substitution, thereby amplifying the negative impacts on child maltreatment. Moreover, consistent with their more urban profile, the counties in Evans et al. (2022) contain a younger population, who are more likely to have children in the household. These differences in sample composition may explain the higher estimated risk of child abuse and neglect in the Evans et al. study, and also underscore the role played by the potential for substitution from prescription opioid misuse to illicit opioids in driving any beneficial vs. adverse impacts, on the net, of supply-side interventions.

Our paper also relates to the broader health and crime literature that has documented adverse consequences of OxyContin reformulation on infectious diseases (Beheshti, 2019), food insecurity (Heflin and Sun, 2022), child welfare utilization and caregiving

[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.

patterns (Mackenzie-Liu, 2021; Laurito, 2024), and suicide and homicide rates (Powell, 2023; Park, 2025). In contrast to these studies, which focus on downstream health and socioeconomic harms, we provide the first evidence on the spillover effects of OxyContin reformulation on violent behaviors from intimate partners, highlighting interpersonal violence as an important but previously overlooked dimension of the reformulation's consequences.

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; Erten and Keskin, 2021a; Sanin, 2021), education (Erten and Keskin, 2018), divorce laws (Stevenson and Wolfers, 2006; García-Ramos, 2021), women's political participation (Stern and Erten, 2024; Anukriti et al., 2025), COVID-19 public health crisis (Erten et al., 2022; Bullinger et al., 2021), and trade shocks (Erten and Keskin, 2021b). 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. A related study recently found that while higher temperatures increase IPV risk, the OxyContin reformulation mitigates this effect by reducing the complementary use of other substances, such as alcohol, on hot days (Pavanello and Zappalà, 2024).

This paper is organized as follows. Section 2 provides a brief description of the OxyContin reformulation and the data used for the analysis. Section 3 presents the identification strategy and the empirical results. Section 4 concludes with a discussion of our findings.

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 Fig. 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 et al., 2017; Powell and Pacula, 2021), with the rate of annual opioid prescriptions increasing from 0.83 to 0.91 per person from 2006 to 2010, remaining constant from 2010 to 2012, and then decreasing steadily to 0.53 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 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

[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).

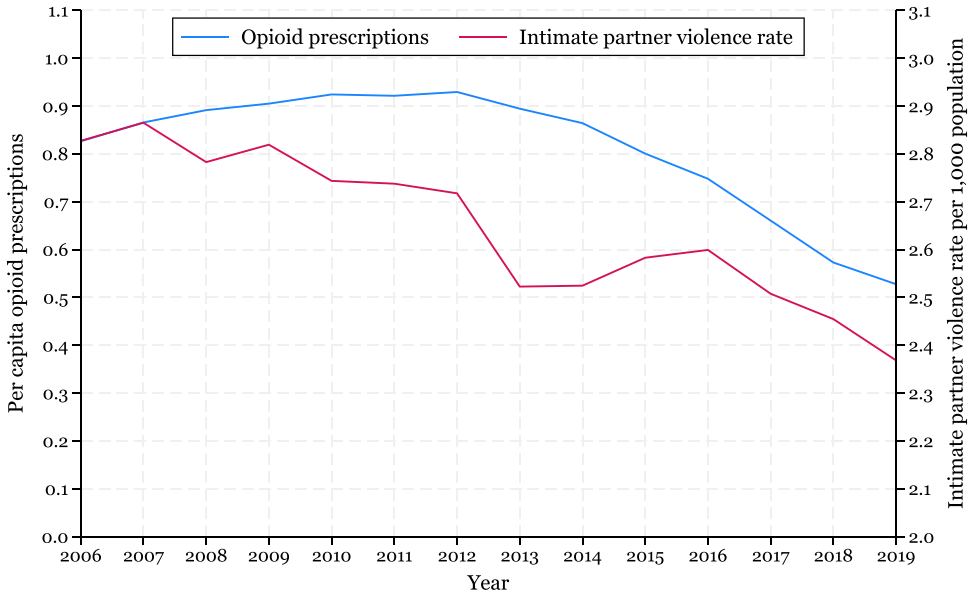

Fig. 1. Opioid prescriptions per capita and intimate partner violence rate.
Note: Figure depicts annual opioid prescriptions per capita reported by the CDC and the intimate partner violence rate per 1000 population calculated from the 2006–2019 NIBRS. Opioid prescriptions per capita refer to the population-weighted 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 1000 population.

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.

In our analysis, we focus on IPV incidents experienced by female victims, where the relationships with the offenders include spouses, common-law spouses, boyfriends/girlfriends, homosexual partners, ex-spouses, and ex-boyfriends/girlfriends. We construct a county-specific sum of all reported IPV incidents, including aggravated assaults, simple assaults, forced sex, and intimidation, based on the reporting agencies in a given county. We normalize this count by the county population, and construct the annual IPV rate per 1000 population at the county level. Our main outcome captures the total IPV rate, representing all IPV incidents reported to law enforcement. Additionally, we also construct measures of the heroin-involved IPV rate, which is the rate of IPV incidents where the police suspected that the offender was using heroin at the time of the incident; the IPV injury rate, comprising incidents that involved physical injury to the victim; and the IPV arrest rate, comprising incidents that resulted in the arrest of the offender.

A limitation of the NIBRS is that state coverage was not universal but expanded over time. To have a consistent set of counties that can be tracked over time, while bypassing any compositional selection stemming from areas entering/exiting the NIBRS sample due to coverage gaps, we construct a balanced panel following other studies in the crime literature (Henke and Hsu, 2020; Fone et al., 2023). Our dataset consists of a balanced panel of county-level data from 2006–2019, including a total of 892 counties.[5] We compare the demographic and socioeconomic characteristics of counties included in our NIBRS sample to those of all counties that are not in our sample for the pre-treatment period (2006–2009) in Appendix Table A1. The normalized differences listed in column 4 are below one-quarter for all characteristics listed, indicating no systematic differences between the two groups. Appendix Table A2 shows that the average county had an IPV incident rate of 2.65 per 1000 population annually over 2006–2019, with 50 percent of these incidents resulting in injuries and 55 percent of them ending with the perpetrator being arrested. Fig. 1 illustrates that the annual IPV rate in the US followed a declining trend from 2.83 per 1000 in 2006 to 2.38 per 1000 in 2019.

2.3. Data on opioid prescriptions and county-level covariates

Our primary proxy for treatment intensity—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). Counties with higher pre-intervention opioid prescribing rates, and thus a higher potential for opioid misuse at baseline, would be more exposed to the reformulation shock relative to counties with lower opioid prescribing rates. Assigning treatment intensity based on pre-intervention opioid prescribing

[5] While the number of agencies reporting in a given county can vary over time, we show in Appendix Table A6 that this is orthogonal to the treatment. We discuss this in more detail in Section 3.4 together with other robustness checks.

Table 1
The effects of OxyContin reformulation on IPV rates.

	(1)	(2)	(3)	(4)	(5)
Intimate partner violence rate	−0.1973***	−0.2136***	−0.2483***	−0.2171***	−0.2132***
	(0.0574)	(0.0581)	(0.0666)	(0.0649)	(0.0651)
Observations	12,530	12,530	12,530	12,348	12,348
Mean in 2006–09	2.8235	2.8235	2.8235	2.8345	2.8345
Heroin-involved IPV rate	0.0002**	0.0001*	0.0002**	0.0002***	0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	12,530	12,530	12,530	12,348	12,348
Mean in 2006–09	0.0001	0.0001	0.0001	0.0001	0.0001
Injury rate	−0.0803***	−0.0715**	−0.1037***	−0.0970***	−0.1006***
	(0.0292)	(0.0309)	(0.0373)	(0.0364)	(0.0377)
Observations	12,530	12,530	12,530	12,348	12,348
Mean in 2006–09	1.3847	1.3847	1.3847	1.3874	1.3874
Arrest rate	−0.0669***	−0.0735***	−0.0955***	−0.0945***	−0.0919***
	(0.0250)	(0.0284)	(0.0298)	(0.0302)	(0.0297)
Observations	12,530	12,530	12,530	12,348	12,348
Mean in 2006–09	1.5092	1.5092	1.5092	1.5097	1.5097
County and year fixed effects	✓	✓	✓	✓	✓
Non-medical pain reliever misuse × year fixed effects	×	✓	✓	✓	✓
Initial county characteristics × year fixed effects	×	×	✓	✓	✓
Time-varying county socioeconomic controls	×	×	×	✓	✓
Any PDMP and 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 1000 population reported by female victims at the county level (N = 12,530 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 general pain reliever misuse (2004–2009) at the state level interacted with year fixed effects; column (3) includes controls for initial county characteristics interacted with year fixed effects, including initial(2006) shares of the population without any college education and of employment share in mining, and 2010 share of rural population; column (4) 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), and column (5) controls for whether the state has a Prescription Drug Monitoring Program (PDMP) of any form, and 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.

Table 2
Robustness to additional control variables.

	IPV rate per 1000 population	Heroin-involved IPV rate per 1000 population	Injury rate per 1000 population	Arrest rate per 1000 population
Controlling for:				
East dummy	−0.2132***	0.0002***	−0.1006***	−0.0919***
	(0.0651)	(0.0001)	(0.0377)	(0.0297)
Good Samaritan laws	−0.2077***	0.0001**	−0.0957***	−0.0880***
	(0.0612)	(0.0001)	(0.0361)	(0.0288)
Naloxone laws	−0.2079***	0.0001**	−0.0962***	−0.0919***
	(0.0615)	(0.0001)	(0.0363)	(0.0288)
Decriminalization of Marijuana	−0.2162***	0.0002**	−0.1036***	−0.0933***
	(0.0652)	(0.0001)	(0.0374)	(0.0296)
Recreational Marijuana laws	−0.1848***	0.0001*	−0.0846**	−0.0749***
	(0.0613)	(0.0001)	(0.0349)	(0.0264)
Physical examination requirements	−0.2051***	0.0001*	−0.0943***	−0.0875***
	(0.0614)	(0.0001)	(0.0358)	(0.0283)
ACA expansion	−0.1994***	0.0002***	−0.0972**	−0.0828***
	(0.0675)	(0.0001)	(0.0395)	(0.0285)
EITC policy	−0.2147***	0.0002**	−0.1017***	−0.0927***
	(0.0651)	(0.0001)	(0.0378)	(0.0297)

Notes: Data are from the 2006–2019 NIBRS. Analyses showing the IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1000 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), pre-treatment shares of the population without any college education, employment share in mining, rural share of population, and non-medical pain reliever misuse, all 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.

rates assumes a correspondence between opioid prescriptions and opioid misuse. An alternate interpretation could be that counties with higher opioid prescriptions prior to the reformulation, representing areas with greater access, may also offer greater potential to substitute into other prescription opioids in the event of a supply shock affecting any particular opioid.

In order to clarify between these interpretations, we undertake two approaches. First, as an alternate proxy for the intensity of treatment exposure, we use a more direct measure of pre-reformulation misuse specifically related to OxyContin, constructed from state-level data on the share of the population reporting non-medical use of OxyContin between 2004 and 2009 derived from the National Surveys of Drug Use and Health (NSDUH). This measure, originally introduced into their analyses by Alpert et al. (2018) and subsequently employed by Evans et al. (2022), captures more salient variation in direct misuse of the reformulated drug, in contrast to broader access to prescription opioids, and is thus conceptually closer to the population most impacted by the reformulation. We note that while this measure provides a more direct proxy for the population at risk of reformulation-related disruption in the prescription opioid market, the constructed rate from the public use NSDUH files varies only at the state level, lacking sub-state variation, and is based on self-reports. Second, we incorporate a broader measure of non-medical pain reliever misuse over 2004–2009 to better identify the role of OxyContin misuse, relative to broader patterns in prescription opioid consumption and access. This strategy builds on insights from Alpert et al. (2018), who emphasize the relevance of accounting for general misuse in order to separate the effects of the OxyContin reformulation from other simultaneous changes, including policy interventions that may have influenced opioid use overall. If the reformulation specifically altered outcomes through reductions in OxyContin misuse, then we would expect the coefficient on this variable to remain dominant when both measures are included. We test this by interacting the broader misuse variable with year indicators and evaluating the stability of our main results.

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). Third, we incorporate the county's share of rural population, as reported by the 2010 Census, interacted with the year fixed effects, to control for time-varying unobserved heterogeneity related to different levels of access to both prescription and illicit opioids, law enforcement capacity, or social norms around IPV reporting (Keyes et al., 2014).

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 A2 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:

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

where Y_{ct} represents the outcome of IPV rate per 1000 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. In supplementary analyses, we proxy $Exposure$ with the more direct measure of the prevalence of OxyContin misuse constructed from the NSDUH, albeit at the less-granular state level.

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 state and county characteristics as discussed in Section 2.3: non-medical pain reliever misuse at the state level over 2004–2009; the share of population without any college education at the county level in 2006, the share of employment in mining at the county level in 2006, and the share of rural population at the county level in 2006. 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:

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

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 the NSDUH (Panel A) and the county-level change in per capita Schedule II opioids from 2008 to 2019 using the administrative 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. Fig. 2 shows event-study plots, showing estimates from specifications with no controls other than county and year fixed effects, and with baseline controls as specified in Eq. (1). In Panel A, we find that in the period prior to the intervention in 2010, the coefficient estimates for IPV rate are statistically indistinguishable from zero.[6] 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.[7] 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 2.1.[8,9] 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.[10] In the first row, the coefficient estimate implies that a one standard deviation increase in pre-reformulation exposure yields about a 7.5 percent annual decline in the IPV rate on average following OxyContin's reformulation.[11,12]

[6] Appendix Figure A2 plots the unconditional trends in our outcome measures, separated across low and high exposure counties based on the median baseline exposure to the OxyContin reformulation in 2006–2009. These figures indicate that IPV rates trended similarly before the policy change across the low and high exposure counties, but started to deviate – with the high-exposure counties experiencing a more pronounced decline – over the post-reformulation period, substantiating our identifying strategy and presaging (albeit noisily) our main findings.

[7] We conducted the pre-trend evaluation, following the approaches outlined by Roth (2022). For all outcomes with baseline controls depicted in Fig. 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 test whether there are linear violations of parallel trends that conventional pretests would detect 50% or 80% of the time (γ0.5 and γ0.8). We found that all slopes are less than 0.05 for more than 50% or 80% of the time.

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

[9] 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, 2025; Gihleb et al., 2022; Powell and Pacula, 2021; Park and Powell, 2021; Dave et al., 2021).

[10] In column 2 of Table 1, we include both the opioid prescription rate from the CDC and the broader NSDUH-based non-medical pain reliever misuse rate. This specification helps address potential concerns that our main exposure variable may be capturing effects associated with more general patterns of opioid misuse. While the two measures are correlated, our estimates indicate that the observed effects are driven by opioid misuse specifically. The coefficient on this variable remains statistically significant.

[11] This reduction represents a decline in IPV of 0.2132 incidents per 1000 population as share of the pre-reformulation outcome mean of 2.8345 (−0.2132/2.8345*100).

In Panel B of Fig. 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 1000 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 offset the decline in total IPV incidents observed overall in affected counties.

The event study results for the injury rate and arrest rate per 1000 population are consistent with the results observed for the IPV rate (Panels C and D of Fig. 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 7.3 percent annual decline in the injury rate and a 6.1 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 Fig. 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.

Following the literature, our main analyses leverage pre-intervention variation in opioid prescription rates as a proxy for treatment intensity, under the reasonable assumption that the scale of opioid prescribing across areas (conditional on observed factors) corresponds to some degree with the scale of OxyContin misuse. In Appendix Table A4, we replicate the analyses using a direct proxy for the intensity of treatment exposure, that is the measure of pre-reformulation misuse specifically related to OxyContin constructed from the NSDUH.[13] Given the lack of granularity and loss of sub-state variation with the NSDUH-based measure, standard errors expectedly inflate resulting in imprecise point estimates. Nevertheless, these findings uncover highly similar patterns.[14] The corresponding event study analyses, presented in Appendix Figure A3, also closely track our main results and provide further support that the observed effects are linked specifically to reductions in OxyContin misuse rather than broader trends in prescription opioid access.

We also replicate our results in Appendix Table A5 by omitting the economic controls (the labor force participation rate and the unemployment rate) and show that results remain very similar. We also note that Table 1 reports results without any time-varying controls, which are again quite similar to the results with all baseline controls, underscoring that the inclusion of county-specific time-varying characteristics do not make a meaningful change in the estimated effects of the OxyContin reformulation.

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

[12] Appendix Table A3 shows that the results are very similar when the outcome is scaled by the size of the female population. Since opioids can affect the risk of women's exposure to IPV both by affecting the risk of perpetration (with most perpetrators of IPV towards women being men) as well as the risk of victimization, we scale our outcome measures by the sum of both male and female populations in a given county at any specific year.

[13] When using the state-level OxyContin misuse rates to capture variation in exposure to treatment, we retain the county-level structure of the analysis to nevertheless exploit geographic variation in outcomes and controls; we cluster the standard errors, however, at the state level to reflect the geographic level at which we are now measuring the treatment intensity.

[14] All of the point estimates for the main IPV outcomes are consistently negative and meaningful; the reformulation of OxyContin led to a decline in IPV incidents in the treated counties. Estimates for IPV incidents involving heroin are positive and statistically significant, confirming our main results regarding the substitution effects arising from highly addicted populations seeking alternate illicit opioids following the reformulation.

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 Fig. 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 baseline pre-reformulation period 2006 to 2009, prior to reformulation in 2010, Gupta and Mazumder (2023).[15] The intuition behind using this ratio as a proxy for the illicit opioid market is related to the findings from Evans et al. (2019), who show that the reformulation did not result in any meaningful reduction in total opioid deaths due to the substitution from prescription opioid mortality to heroin/illicit opioid mortality post-reformulation. Consequently, variation in this substitution risk can be a proxy for the scale of illicit opioid market; areas where illicit opioids were easier and more cheaper to obtain would experience a larger increase in heroin/illicit opioid mortality post-reformulation, which would be replacing the pre-formulation mortality from prescription opioids. Since the post-reformulation heroin/illicit mortality is potentially endogenous however (leading to endogenous stratification if we were to stratify counties based on this measure), we follow Gupta and Mazumder (2023) and utilize the pre-formulation ratio of heroin/illicit mortality to prescription opioid mortality; areas where this ratio is higher generally had larger and better developed illicit drug markets. We then divide 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 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.[16] 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.[17]

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.).[18] The left side of Panel A in Fig. 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.[19] However, the relatively larger reduction in IPV risk for younger women is consistent with 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 Fig. 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 Fig. 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

[15] 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.

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

[17] We also construct an alternative measure where we take the ratio of deaths from illicit and synthetic opioids to the total number of opioid deaths. The results displayed in Appendix Figure A5 are entirely consistent with the results in Fig. 3, and while the quantitative estimates slightly vary, qualitative conclusions remain the same.

[18] Specifically, we count the IPV incidents for each sub-population, and then divide this count by the total county population to obtain the incident rate for a specific sub-population. The pooled estimate in Fig. 4 equals the sum of the sub-population estimates with some rounding error.

[19] Appendix Figure A6 reports that the reductions in injury and arrest rates in more affected counties largely followed similar patterns in heterogeneity by victim characteristics.

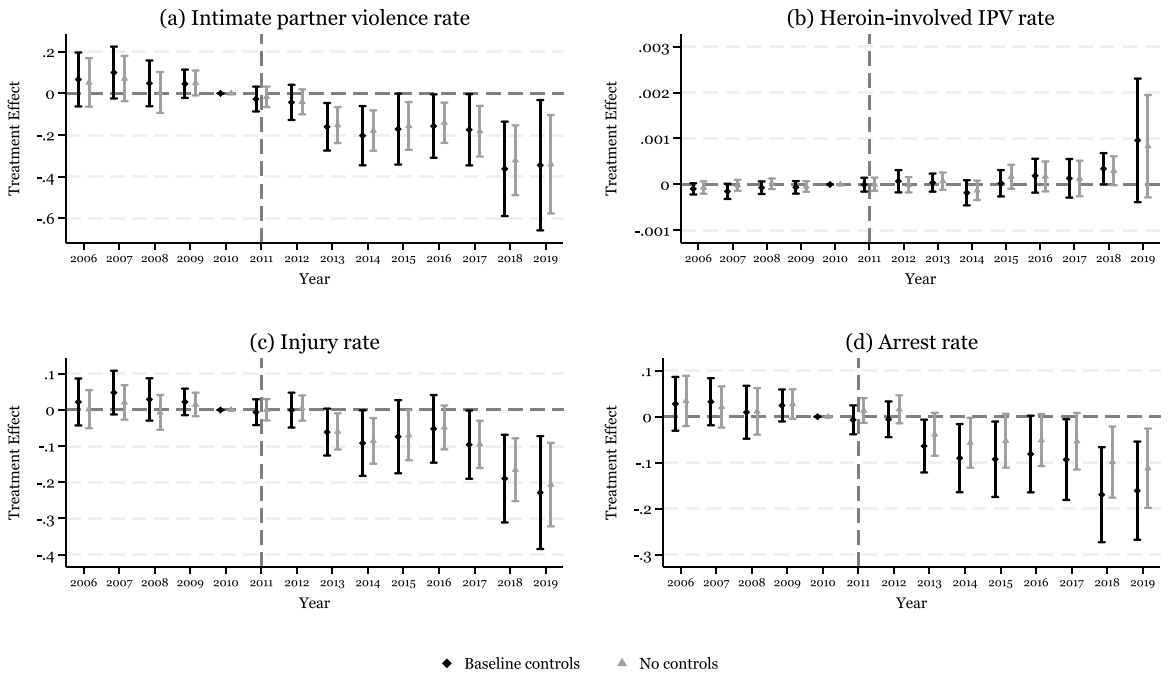

Fig. 2. The effects of OxyContin reformulation on IPV rates over time.
Note: Data are from the 2006–2019 NIBRS. Event-study plots showing the response of IPV rate, heroin-involved IPV rate, injury rate, and arrest rate per 1000 population reported by female victims at the county level (N = 12,530 county-years) 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 with no controls include only county and year fixed effects. Specifications with baseline controls 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), pre-treatment shares of the population without any college education, employment share in mining, rural share of population, and non-medical pain reliever misuse, all 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.

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.[20] Appendix Figure A7 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 women, with no statistically significant differences between these two groups.[21] 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 Table 2, we first test the sensitivity of our estimates to geographic heterogeneity in the evolution of the opioid crisis by including an indicator for whether each state is located east of the Mississippi River interacted with year fixed effects. This distinction captures structural differences in heroin and fentanyl markets: as documented by Abouk et al.

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

[21] 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.

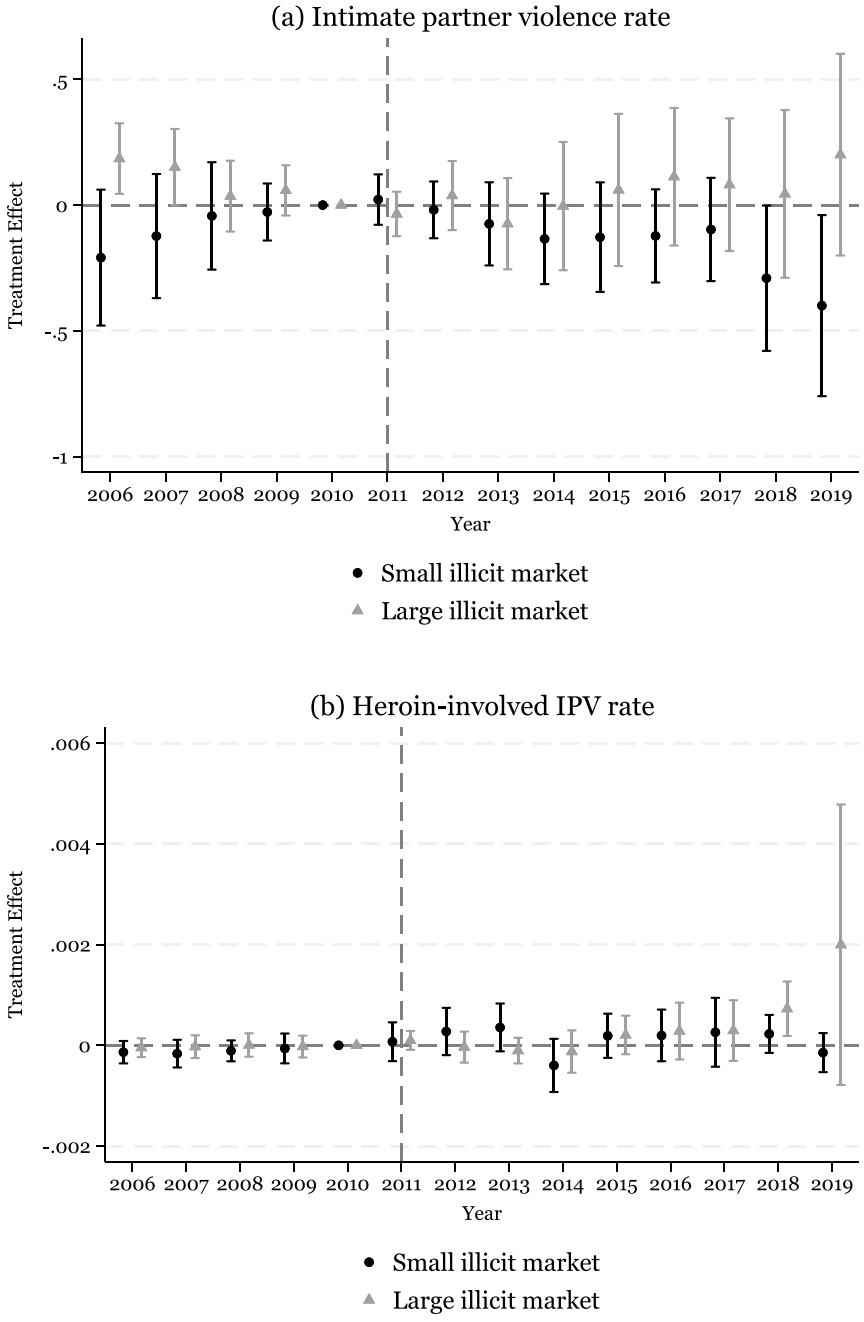

Fig. 3. Heterogeneity analyses by size of illicit opioid market activity.
Note: Data are from the 2006–2019 NIBRS. The figure presents subgroup analyses by the size of the illicit opioid market activity in a state, which is proxied by the ratio of deaths due to illicit or synthetic opioids to deaths from prescription opioids over the period 2006 to 2009, prior to OxyContin reformulation in 2010. All specifications include baseline controls as listed in Fig. 2. The coefficient estimates show treatment effects on IPV rate and heroin-involved IPV rate per 1000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates. Standard errors are clustered at the county level.

(2021), white powder heroin predominates in the East and can be easily mixed with illicitly manufactured fentanyl, while black tar and brown powder heroin, which are less commonly adulterated with fentanyl, dominate markets in the West. These differences may have contributed to regionally divergent time-varying patterns in synthetic opioid mortality. Our estimates are robust to including this measure, suggesting that such geographic variation does not drive our main findings.

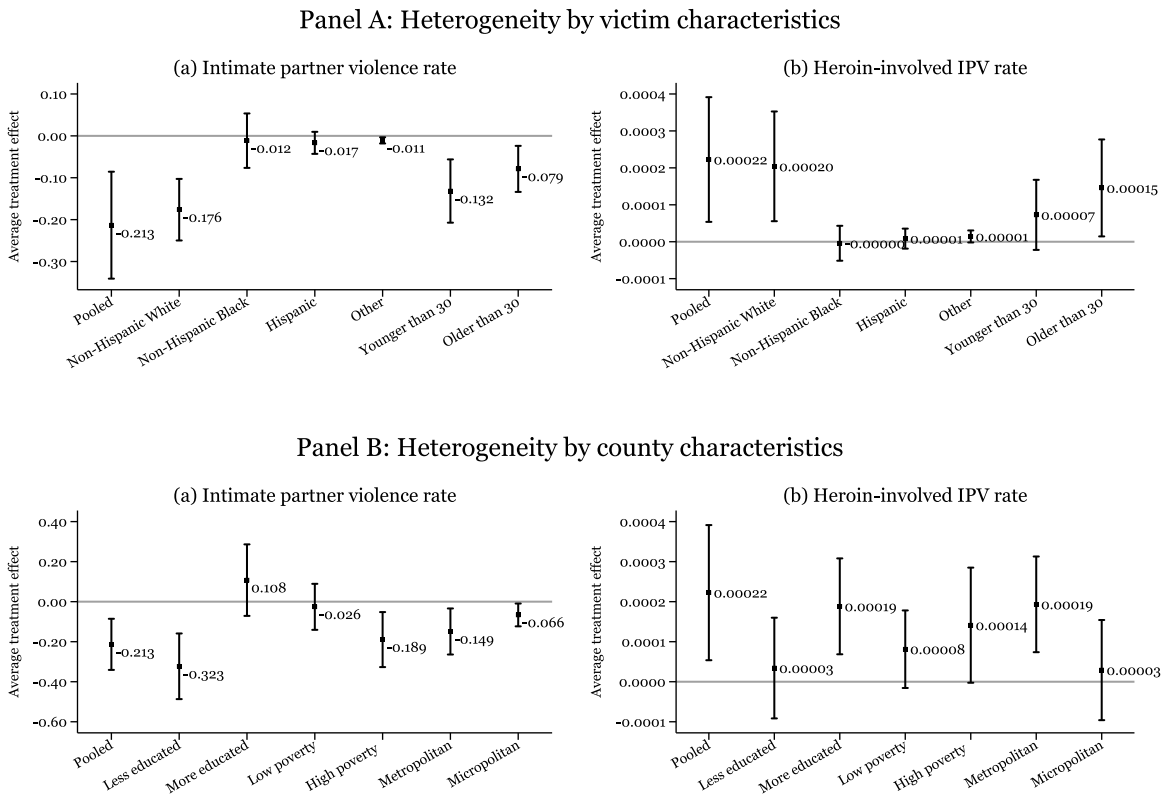

Fig. 4. Heterogeneity analyses.
Note: Data are from the 2006–2019 NIBRS. All specifications include baseline controls as listed in Fig. 2. The figure presents victim-level subgroup analyses in Panel A and county-level subgroup analyses in Panel B, displaying estimated treatment effects on IPV rate and heroin-involved IPV rate per 1000 population, as reported by female victims at the county level. Vertical bars represent the respective 95% confidence intervals for these estimates. Standard errors are clustered at the county level.

We further incorporate additional state policies and regulations that could affect opioid use and IPV prevalence. In Table 2, 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 A6, we examine whether the fraction of IPV incidents that result in an injury or arrest change in response to OxyContin reformulation. The results presented in columns 1 and 2 show no evidence of a significant or meaningful impact on these outcomes. We interpret these findings as indicating that the reformulation did not shift the severity of IPV incidents on the margin. Alternately, if there was a shift in the reporting of IPV incidents towards or away from more severe incidents (as proxied by those involving an injury or resulting in the arrest of the offender), then we would have observed significant effects on these shares. That we do not find any effects here also implies that the treatment is not associated with more severe incidents being reported to the police, which is ex post reassuring in terms of ruling out any potential reporting bias driving our estimates. Results in column 3 explore whether the number of law enforcement agencies reporting IPV data in a given county is associated with the OxyContin reformulation and treatment exposure. The results show no statistically significant change in the number of reporting agencies following the reformulation across areas varying in their treatment intensity, suggesting that our estimates are unlikely to be driven by changes in data coverage or agency participation.

In Appendix Table A7, 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 per capita within each county and year, and we exclude counties with inadequate IPV data reporting following prior studies (Freedman and Owens, 2011; Thomas and Shihadeh, 2013; Fone et al., 2023).[22] Lastly, we restrict the sample to only counties in the Evans et al. (2022) sample

to test whether selection of counties alters our results. These various checks yield findings consistent with our baseline estimates in Table 1.

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 A8 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 reported 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 7.5 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 $53 billion annually.

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.

CRediT authorship contribution statement

Dhaval Dave: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Bilge Erten:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Pinar Keskin:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Shuo Zhang:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

Dave, Erten and Keskin are grateful for funding support for this research from the National Institute of Drug Abuse, National Institutes of Health (NIH) (Grant number R21DA058849).

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jhealeco.2025.103073.

Data availability

Program files to replicate the results presented in the paper are available on request from the corresponding author.

[22] 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.

References

Abouk, Rahi, Helmchen, Lorens, Moghtaderi, Ali, Pines, Jesse, 2021. The ACA medicaid expansions and opioid mortality: Is there a link? Med. Care Res. Rev. 78 (6), 713–724.

Aizer, Anna, 2010. The gender wage gap and domestic violence. Am. Econ. Rev. 100 (4), 1847–1859.

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

Alpert, A., Evans, W.N., Lieber, E.M.J., Powell, D., 2022. Origins of the opioid crisis and its enduring impacts. Q. J. Econ. 137, 1139–1179.

Alpert, Abby, Powell, David, Pacula, Rosalie Liccardo, 2018. Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids. Am. Econ. J.: Econ. Policy 10 (4), 1–35.

Angelucci, M., Heath, R., 2020. Women empowerment programs and intimate partner violence. AEA Pap. Proc. 110, 610–614.

Anukriti, S., Erten, Bilge, Mukherjee, Priya, 2025. Female leaders and intrahousehold dynamics: Evidence from state elections in India. Available at SSRN 5407204.

Barbos, Andrei, Sun, Minglu, 2021. Opioid control policies can also reduce domestic violence. Available at SSRN 4747347.

Beheshti, David, 2019. Adverse health effects of abuse-deterrent opioids: Evidence from the reformulation of OxyContin. Heal. Econ. 28 (12), 1449–1461.

Beheshti, David, Kim, Bokyung, 2025. Beyond opioids: The effect of prescription drug monitoring programs on non-opioid drug prescribing. Contemp. Econ. Policy 43 (3), 452–469.

Bobonis, Gustavo J., Gonzalez-Brenes, Melissa, Castro, Roberto, 2013. Public transfers and domestic violence: The roles of private information and spousal control. Am. Econ. J.: Econ. Policy 5 (1), 179–205.

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

Bullinger, L.R., Carr, J.B., Packham, A., 2021. COVID-19 and crime: Effects of stay-at-home orders on domestic violence. Am. J. Heal. Econ. 7, 249–280.

Butler, S.F., Cassidy, T.A., Chilcoat, H., Black, R.A., Landau, C., Budman, S.H., Coplan, P.M., 2013. 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. J. Pain 14 (4), 351–358.

Case, Anne, Deaton, Angus, 2017. Mortality and morbidity in the 21st century. Brookings Pap. Econ. Act. 2017, 397.

Case, Anne, Deaton, Angus, 2020. Deaths of Despair and the Future of Capitalism. Princeton University Press.

Castilla, C., Aqeel, F., Murphy, D.M.A., 2022. Tipples and quarrels in the household: The effect of an alcohol mitigation intervention on intimate partner violence. Working Paper.

Castilla, C., Murphy, D.M.A., 2022. Bidirectional intimate partner violence: Evidence from a list experiment in Kenya. Working Paper.

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

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

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

Currie, Janet, Jin, Jonas, Schnell, Molly, 2019. US employment and opioids: Is there a connection? In: Health and Labor Markets. Emerald Publishing Limited, pp. 253–280.

Dave, Dhaval, Deza, Monica, Horn, Brady, 2021. Prescription drug monitoring programs, opioid abuse, and crime. South. Econ. J. 87 (3), 808–848.

Dave, Dhaval, Erten, Bilge, Hummel, David, Keskin, Pinar, Zhang, Shuo, 2025. Fighting abuse with prescription tracking: mandatory drug monitoring and intimate partner violence. J. Popul. Econ. 38 (3), 1–27.

El-Bassel, N., Gilbert, L., Wu, E., Chang, M., Fontdevila, J., 2007. Perpetration of intimate partner violence among men in methadone treatment programs in New York City. Am. J. Public Health 97, 1230–1232.

Erten, Bilge, Keskin, Pinar, 2018. For better or for worse?: Education and the prevalence of domestic violence in Turkey. Am. Econ. J.: Appl. Econ. 10 (1), 64–105.

Erten, Bilge, Keskin, Pinar, 2021a. Female employment and intimate partner violence: evidence from Syrian refugee inflows to Turkey. J. Dev. Econ. 150, 102607.

Erten, Bilge, Keskin, Pinar, 2021b. Trade-offs? The impact of WTO accession on intimate partner violence in Cambodia. Rev. Econ. Stat. 1–40.

Erten, Bilge, Keskin, Pinar, Prina, Silvia, 2022. Social distancing, stimulus payments, and domestic violence: Evidence from the us during covid-19. In: AEA Papers and Proceedings. vol. 112, American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203, pp. 262–266.

Evans, M.F., Harris, M.C., Kessler, L.M., 2022. The hazards of unwinding the prescription opioid epidemic: Implications for child maltreatment. Am. Econ. J.: Econ. Policy 14 (4), 192–231.

Evans, W.N., Lieber, E.M.J., Power, P., 2019. How the reformulation of OxyContin ignited the heroin epidemic. Rev. Econ. Stat. 101, 1–15.

Fone, Zachary S., Sabia, Joseph J., Cesur, Resul, 2023. The unintended effects of minimum wage increases on crime. J. Public Econ. 219, 104780.

Freedman, Matthew, Owens, Emily G., 2011. Low-income housing development and crime. J. Urban Econ. 70 (2–3), 115–131.

García-Ramos, Aixa, 2021. Divorce laws and intimate partner violence: Evidence from Mexico. J. Dev. Econ. 150, 102623.

Gihleb, Rania, Giuntella, Osea, Zhang, Ning, 2022. The effect of mandatory-access prescription drug monitoring programs on foster care admissions. J. Hum. Resour. 57 (1), 217–240.

Gupta, Sumedha, Mazumder, Bhashkar, 2023. The effects of prescription drug monitoring programs on labor market activity and credit outcomes. Fed. Reserv. Bank Chic. Work. Pap..

Guy, Jr., Gery P, Zhang, Kun, Bohm, Michele K, Losby, Jan, Lewis, Brian, Young, Randall, Murphy, Louise B, Dowell, Deborah, 2017. Vital signs: changes in opioid prescribing in the United States, 2006–2015. Morb. Mortal. Wkly. Rep. 66 (26), 697.

Harris, Matthew C, Kessler, Lawrence M, Murray, Matthew N, Glenn, Beth, 2020. Prescription opioids and labor market pains: the effect of schedule ii opioids on labor force participation and unemployment. J. Hum. Resour. 55 (4), 1319–1364.

Heflin, Colleen, Sun, Xiaohan, 2022. Food insecurity and the opioid crisis. Ann. Am. Acad. Political Soc. Sci. 703 (1), 262–284.

Henke, Alexander, Hsu, Lin-chi, 2020. The gender wage gap, weather, and intimate partner violence. Rev. Econ. Househ. 18, 413–429.

Hughes, A., Dank, M., Wood, L., Rashid, M., 2019. The socio-ecological intersections of the opioid epidemic and violence against women: A scoping review.

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

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

Keyes, Katherine M., Cerdá, Magdalena, Brady, Joanne E., Havens, Jennifer R, Galea, Sandro, 2014. Understanding the rural–urban differences in nonmedical prescription opioid use and abuse in the United States. Am. J. Public Health 104 (2), e52–e59.

Kim, Bokyung, 2021. Must-access prescription drug monitoring programs and the opioid overdose epidemic: The unintended consequences. J. Heal. Econ. 75, 102408.

Laurito, Agustina, 2024. Spillovers of the heroin epidemic on grandparent caregiving. Popul. Res. Policy Rev. 43 (2), 25.

Mackenzie-Liu, Mattie, 2021. From fostering hope to lingering harm: The unintended impact of the OxyContin reformulation on child welfare utilization. Soc. Serv. Rev. 95 (1), 36–65.
Maclean, Johanna Catherine, Mallatt, Justine, Ruhm, Christopher J, Simon, Kosali, 2022. The opioid crisis, health, healthcare, and crime: A review of quasi-experimental economic studies. Ann. Am. Acad. Political Soc. Sci. 703 (1), 15–49.
Mallatt, J., 2018. The effect of prescription drug monitoring programs on opioid prescriptions and heroin crime rates.
Markowitz, Sara, 2000. The price of alcohol, wife abuse, and husband abuse, 2000. South. Econ. J. 67, 279–303.
Meinhofer, Angélica, 2018. Prescription drug monitoring programs: the role of asymmetric information on drug availability and abuse. Am. J. Heal. Econ. 4 (4), 504–526.
Metcalf, Gilbert E., Wang, Qitong, 2019. Abandoned by Coal, Swallowed by Opioids? Technical Report, National Bureau of Economic Research.
Monnat, Shannon M., 2018. Factors associated with county-level differences in US drug-related mortality rates. Am. J. Prev. Med. 54 (5), 611–619.
National Academies of Sciences, Engineering, and Medicine and others, 2017. Pain Management and the Opioid Epidemic: Balancing Societal and Individual Benefits and Risks of Prescription Opioid Use. National Academies Press.
Packard, G., Warshaw, C., 2018. Framing the issues: Looking at the opioid epidemic in the context of trauma and domestic violence.
Palmer, Roy E, Carrell, David S, Cronkite, David, Saunders, Kathleen, Gross, David E, Masters, Elizabeth, Donevan, Sean, Hylan, Timothy R, Von Kroff, Michael, 2015. The prevalence of problem opioid use in patients receiving chronic opioid therapy: computer-assisted review of electronic health record clinical notes. Pain 156 (7), 1208–1214.
Park, Sujeong, 2025. The growth of illicit drug use and its effects on murder rates. Heal. Econ. 34 (3), 456–471.
Park, Sujeong, Powell, David, 2021. Is the rise in illicit opioids affecting labor supply and disability claiming rates? J. Heal. Econ. 76, 102430.
Pavanello, Filippo, Zappalà, Guglielmo, 2024. Co-benefits of substance abuse regulation on temperature and intimate partner violence. Work. Pap..
Peterson, C., Kearns, M.C., McIntosh, W.L., Estefan, L.F., Nicolaidis, C., McCollister, K.E., Gordon, A., Florence, C., 2018. Lifetime economic burden of intimate partner violence among U.S. adults. Am. J. Prev. Med. 55, 433–444.
Powell, David, 2023. Growth in suicide rates among children during the illicit opioid crisis. Demography 60 (6), 1843–1875.
Powell, D., Pacula, R.L., 2021. The evolving consequences of OxyContin reformulation on drug overdoses. Am. J. Heal. Econ. 7, 41–67.
Pryor, C., Boman, IV, J.H., Hemez, P.l., 2021. Using arrest and prescription data to examine the relationship between intimate partner violence and opioid prescriptions in the United States, 2006–2012. Drug Alcohol Depend. 218, 108389.
Roth, Jonathan, 2022. Pretest with caution: Event-study estimates after testing for parallel trends. Am. Econ. Rev.: Insights 4 (3), 305–322.
Sanin, Deniz, 2021. Paid Work for Women and Domestic Violence: Evidence from the Rwandan Coffee Mills. Technical Report, Working Paper.
Stern, Tomer, Erten, Bilge, 2024. Women's participation in the arab spring protests and the prevalence of domestic violence: Evidence from Egypt. Fem. Econ. 30 (4), 42–74.
Stevenson, Betsey, Wolfers, Justin, 2006. Bargaining in the shadow of the law: Divorce laws and family distress. Q. J. Econ. 121 (1), 267–288.
Stone, R., Rothman, E.F., 2019. Opioid use and intimate partner violence: A systematic review. Curr. Epidemiol. Rep. 6, 215–230.
Thomas, Shaun A., Shihadeh, Edward S., 2013. Institutional isolation and crime: The mediating effect of disengaged youth on levels of crime. Soc. Sci. Res. 42 (5), 1167–1179.
Tran, A., Lin, L., Nehl, E.J., Talley, C.L., Dunkle, K.L., Wong, F.Y., 2014. Prevalence of substance use and intimate partner violence in a sample of A/PI MSM. J. Interpers. Violence 29, 2054–2067.
Unick, George, Rosenblum, Daniel, Mars, Sarah, Ciccarone, Daniel, 2014. The relationship between US heroin market dynamics and heroin-related overdose, 1992–2008. Addiction 109 (11), 1889–1898.
Van Zee, A., 2009. The promotion and marketing of oxycontin: commercial triumph, public health tragedy. Am. J. Public Health 99 (2), 221–227.
Volkow, N.D., 2014. America's addiction to opioids: Heroin and prescription drug abuse. Senat. Caucus Int. Narcotics Control. 14, 1–16.
Warshaw, C., Lyon, E., Bland, P.J., Phillips, H., Hooper, M., 2014. 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.
World Health Organization, 2013. Global and regional estimates of violence against women: prevalence and health effects of intimate partner violence and non-partner sexual violence.