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Sarah Kroeger
U.S. Department of the Treasury

Giulia La Mattina
University of South Florida and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Nuisance Ordinances and Domestic Violence*

Criminal activity nuisance ordinances penalize tenants for calling emergency services in relation to certain illegal events, including domestic violence. There is a widespread concern that these policies will exert a chilling effect on the reporting of domestic violence and potentially increase the incidence and severity of domestic assaults. We exploit the sequential implementation of criminal activity nuisance ordinances by municipalities in Ohio, and estimate the direct impact of these ordinances on intimate partner homicides using a reduced form framework. We rule out an increase in intimate partner homicides following the enactment of a nuisance ordinance; in fact we estimate a negative impact. The effect is driven by a reduction in partner homicides in cities with a higher proportion of renter-occupied homes prior to the implementation of the ordinances. We do not find any evidence that the effect can be attributed to selective migration out of cities that enacted nuisance ordinances or a change in police officer reporting practices.

JEL Classification: J12, J18, K42, R28

Keywords: domestic violence, housing policy, intimate partner homicide, nuisance ordinance

Corresponding author:
Giulia La Mattina
University of South Florida
4202 E Fowler Ave
Tampa, FL 33620
USA

E-mail: glamattina@usf.edu

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1 Introduction

> *"Because a great majority of acts of domestic violence occur at home, which is not true of other crimes, and chronic nuisance laws charge only for the police response to crimes at home, these laws have the potential to affect domestic violence crimes more than stranger crimes."* (Fais 2008, p. 1196).

Over one-third of homicides against women are committed by intimate partners or ex-partners (García-Moreno et al. 2013; Smith, 2022). Intimate partner violence is pervasive, affecting nearly 10 million victims annually (Black et al. 2011).[1] The National Intimate Partner and Sexual Violence Survey found that one in four women in the United States have experienced sexual violence, physical violence, or stalking by an intimate partner, and one in seven women has been seriously injured by an intimate partner (Smith et al. 2017). Hamby et al. (2011) estimate that 12% of all American children have witnessed domestic violence directly, and the majority of these cases are attacks directed at the child's mother. Although domestic violence continues to impose substantial public and private costs on society, the direct causes of domestic violence are still poorly understood.

Previous theoretical and empirical research has highlighted a strong connection between poverty and domestic violence. The risk of domestic violence victimization is higher for women with relatively low bargaining power (Paxson and Waldfogel 2002, Ackerson et al. 2008, Aizer 2010, Bobonis et al. 2013) and for women who are dependent on their partner for food, shelter, and other financial needs (Hornung, McCullough, and Sugimoto 1981, Kalmus and Stras 1982, Basu and Famoye 2004). Psychological factors also play a role as a fear of poverty or homelessness reduces a victim's ability to escape an abusive situation or to report incidents to the police (Williams 1998, Baumer 2002, Gillum 2019). In particular,

[1] Intimate partner homicide is defined as homicide committed by a current or former spouse, domestic partner, boyfriend/girlfriend, or casual romantic partner. In this paper we will use the terms "intimate partner violence" and "domestic violence" interchangeably. In this paper we focus primarily on literature related to the effects of partner violence on female victims. Intimate partner violence against men does also occur but is less frequent, and empirically less likely to result in serious injury (Truman and Morgan 2014). We do include male victims of domestic violence in the empirical analysis nonetheless.

there is a well-documented correlation between domestic violence victimization and a history of housing insecurity (Pavao et al. 2007, Baker, Cook and Norris 2003, Baker et al. 2010, Adams et al. 2021), and policies that impact housing security could also have consequences for the incidence and intensity of partner violence.

In this project, we study the impact of local criminal activity nuisance ordinances on domestic violence. These laws penalize property owners when 911 calls are placed with regard to certain types of criminal activity–including assaults–occurring on or near a residential property. Typically, landlords charged with abating an identified criminal activity nuisance do so by evicting the tenants in the problem property. A defining characteristic of these laws is that they can trigger eviction penalties even for individuals who did not commit the initial criminal activity. Futhermore, because they target criminal activities that take place at home, they are prone to affect domestic violence more than other forms of violent crime. Mead et al. (2017) and Mead et al. (2018) document numerous anecdotes of low-income tenants who were evicted under these laws because they tried to report domestic abuse. Other qualitative studies show that domestic violence victims are unwilling to call for emergency assistance in cities with criminal activity nuisance ordinances, because of their fear of eviction (Fais 2008, Desmond and Valdez 2012, Arnold and Slusser 2015, Arnold 2019).

We use a reduced-form analysis to examine whether these laws have an effect on intimate partner homicides as well as non-fatal partner assaults. Our data include municipal-level crime reports and nuisance ordinance legislation for 167 cities in Ohio with a population of at least 10,000 residents over the period 2000-2016. Ohio is unique in its rapid adoption of 44 new criminal activity nuisance ordinances over a relatively short time span, as shown in Figure 1. Subsequently, the share of Ohioan renters living under an active nuisance ordinance went from 0 in 2003 up to nearly one in three in 2016 (see Figure 2).

We use an event study analysis, a generalized difference-in-differences model, and a triple-difference model to identify the effect of these laws at the city-year level. Our results show a statistically significant decrease in intimate partner homicides for cities that enact a criminal activity nuisance ordinance; this decrease is concentrated among cities that report a high share of renter-occupied housing units. We also show that the policy effect is driven by a lower number of homicides of women, and is statistically significant for homicides of both white and black women. The point estimates are large relative to the mean. As homicides are unlikely to be under-reported, we interpret this result as a decline in intimate partner homicides rather than a chilling effect on reporting. We are able to rule out null and positive effects at a 95% confidence level. When we examine non-fatal domestic violence outcomes, we also see a decline following the introduction of criminal activity nuisance ordinances, but the interpretation of this result is less clear. It is possible that these ordinances do suppress reporting while also reducing actual levels of intimate partner crime. However, Iyengar (2009) finds that a decreased likelihood of reporting domestic violence under mandatory arrest laws resulted in an increase in intimate partner homicides, supporting an interpretation of our findings as an overall decrease in intimate partner violence.

We assess two alternative mechanisms that could generate a negative effect on partner homicides: selective migration in response to the ordinances, and changes in police reporting practices that result in fewer homicides being categorized as intimate partner homicide. The data do not support either of these mechanisms as drivers of our results. Given the lack of evidence for mechanisms like these, we offer the possibility that the nuisance ordinances can also deter would-be domestic abusers by increasing the cost of intimate partner violence in the event that they are reported (Becker, 1968), or by disrupting potentially fatal abusive relationships.

This study contributes to three strands of literature. First, we contribute to an active and consequential policy debate by providing new empirical evidence on the relationship between criminal activity nuisance ordinances and domestic violence. We are aware of two contemporary working papers that examine the effects of a broader class of nuisance ordinances on domestic violence. Aria Golestani (2021) and Emily Moss (2019) both study general nuisance ordinances outside of Ohio, and both find a negative effect of nuisance ordinances on 911 calls in California. Moss (2019) also finds an increase in self-reported victimization among residents of Californian cities that enacted a nuisance ordinance. Golestani finds a positive effect of general nuisance ordinances on partner homicides of male victims during the period 1970-2004. These studies are not able to confirm a positive effect of general nuisance laws on intimate partner homicides of women, and offer mixed evidence on the impact for other measures of partner violence.[2]

We augment the findings of these two studies in several ways. Our primary difference from Moss and Golestani is our specific research question. The focus of our study is on *criminal activity* nuisance ordinances, which are distinctively triggered by criminal activity and hence have the potential to penalize victims who report these crimes. We note that both Golestani (2021) and Moss (2019) look at nuisance ordinances in general, which typically target non-criminal activities like noise and partying.[3] Further differentiating our study, we offer evidence from a relatively recent cluster of policy changes within Ohio that allow us to present more current data on this questionwithin one common state system. To date, this is the only econometric study of criminal activity nuisance ordinances on domestic violence outcomes. Our results contradict the prevailing assumptions regarding the effect of criminal activity nuisance ordinances, and suggest that, at least for fatal outcomes, there is a lack of consistent evidence that these ordinances increase the incidence of domestic violence.

Second, with respect to the specific criminal activity nuisance ordinances in Ohio that are the focus of this paper, we also contribute to a small but growing body of work that evaluates the impact of these laws on various outcomes within Ohio. Kroeger and La Mattina (2020) demonstrate econometrically that the laws increase eviction filings and court-ordered evictions; Falcone (2023) finds varying effects of the ordinances on crime, depending on the

[2]In Golestani's results, the significant effect of nuisance ordinances on female homicides is sensitive to specification.

[3]The studies by Moss and Golestani do not specifically study *criminal activity* nuisance ordinances as we define them, but evaluate general nuisance ordinances. In our review of these statutes, we find that most of the policies coded as "treatment" by Moss and Golestani are broadly targeted at curbing non-criminal activities like littering, noise, and large gatherings; they do not as a rule single out the landlord for abatement responsibility, and do not typically evoke eviction proceedings.

type of crime; Bradford (2022) finds that the ordinances increase county-level mortality rates from accidental drug-related overdoses. We add to this research by studying the effect of the ordinances in question on intimate partner violence.

Finally, this study adds to a broader literature that examines the staggered roll-out of various policies and events on domestic violence (Stevenson and Wolfers 2006, Aizer and Dal Bo 2009, Iyengar 2009, Card and Dahl 2011, Raissian 2016, Miller and Segal 2019, Chin and Cunningham 2019, Carr and Packham 2021).

The remainder of the paper is organized as follows. Section 2 reviews the literature on the relationship between poverty and domestic violence and provides a background on the criminal activity nuisance ordinances that are the focus of our study. Section 3 describes the data and Section 4 discusses the identification strategy. We report the main findings in Section 5 and the results from various robustness tests in Section 6. Section 7 concludes.

2 Poverty and Domestic Violence

More than 10 million people in the United States are abused by a domestic partner each year, and the vast majority of these victims are low-income women (Tjaden and Thoennes 2000). While the factors that contribute to domestic violence are varied and complex, economic disadvantage greatly increases the risk of experiencing domestic violence. There is both theoretical and empirical support for this relationship. A Nash bargaining model predicts that intimate partner violence will increase when a victim's outside option falls (McElroy and Horney 1981, Farmer and Teifenthaler 1996, Lundberg and Pollak 2004, Stevenson and Wolfers 2006, Aizer 2010). As a woman's options for leaving the relationship diminish, the level of violence that she will tolerate within the relationship rises. This model also implies a greater risk of abuse by a partner for women who are pregnant, who have young children, are socially isolated, and are economically or educationally disadvantaged.

Empirically, the correlation between low socioeconomic status and intimate partner violence risk is well documented. For example, Aizer (2010) demonstrates that the risk of hospitalization from domestic assault while pregnant is five times higher for Medicaid recipients than for privately-insured women, and 30 times higher for high school dropouts than for college graduates. Using the Indian Survey of Family Health, Ackerson et al. (2008) find that less educated women are more likely to be victims of domestic violence, and less educated men are more likely to perpetrate domestic violence. Domestic violence is also more prevalent for young women and women of color (Petrosky et al. 2017).

Annual costs of intimate partner violence on health care expenses and lost productivity are estimated to be around $10 billion in 2023 dollars (National Center for Injury Prevention and Control 2003, McLean and Bocinski 2017). Holmes et al. (2018) calculate the economic burden due to childhood IPV exposure at more than $70 billion *per birth cohort* of recent young adults, through the channels of lower lifetime earnings, healthcare costs, and downstream violence or other criminal activity. Campbell (2002) finds that women who experience domestic violence are more likely to report mental health problems and suicidal ideation, and to have sexually transmitted infections and unintended pregnancies. Domestic violence reduces victims' employment and earnings and increases their use of the social safety net (Bhuller et al. 2023; Bindler and Ketel 2022). Adams et al. (2024) show that cohabitation with an abusive partner reduces women's earnings and employment.

Domestic violence also carries serious harm to the victims' children even when they are not the direct target of the assault. Bhuller et al. (2023) show that reporting domestic violence to the police leads to worsened mental health, test scores, and compulsory school completion for the children of victims. They also estimate an increase in child protective services and a short-lived increase in participation in youth crime. Exposure to domestic violence in utero reduces infant birthweight (Aizer 2011, Almond and Currie, 2011, Currie, Muller-Smith, and Rossin-Slater 2018). Additionally, research has shown that domestic violence has negative spillovers within the classroom, thus affecting even children in families where domestic violence does not occur directly. School-age children who witness domestic violence experience behavioral disruptions significant enough to decrease the reading and math test scores of their classmates (Carrell and Hoekstra 2010) and reduce their earnings in the long run (Carrell, Hoekstra, and Kuka 2018). The presence of intimate partner violence in the home is one of the strongest predictors of child maltreatment, and seeing partner violence between adults in the home increases the risk of a child later experiencing first-hand abuse by a factor of 15 (Osofsky 1999, Stith et al. 2009). This correlation is especially high among welfare recipients and homeless families (Appel and Holden 1998). Exposure to intimate partner violence leads to mental and physical health issues in childhood and in later life (Herrenkohl et al. 2008, Sternberg et al. 2006, Shalev et al. 2013), and perpetuates an inter-generational cycle of violence (Roberts et al. 2010, Doyle and Aizer 2018).

2.1 Criminal Activity Nuisance Ordinances

While poverty can be linked to domestic violence through a variety of mechanisms, a central issue for victims of domestic violence is homelessness or housing instability. Studies that highlight the relationship between intimate partner violence and housing instability include Pavao et al. (2007), Tischler et al. (2007), Desmond (2016) and Desmond and Valdez (2013). The relationship between violence and housing stability is complicated because unobservable characteristics that increase homelessness risk are also likely to increase the risk of experiencing domestic violence. However, housing insecurity can be viewed as a driving factor of domestic violence. Prior research has shown that eviction leads to lower earnings and worse housing in worse neighborhoods (Desmond and Shollenberger 2015; Collinson et al. 2024), factors which themselves predict domestic violence (Benson, Fox, DeMaris, and Van Wyk 2003; Wallace, Chamberlain, and Pfeiffer 2021). In light of this there is a concern that any policies known to increase housing instability could also raise the threat of domestic violence. The ordinances under study in this paper could arguably impact domestic violence both with or without a realized eviction or homelessness event.

Criminal activity nuisance ordinances began appearing in American cities throughout the

1980s and 1990s as a way for police departments to deal with an overwhelming number of emergency calls (see Buerger and Mazerolle 1998, Fais 2008, Desmond and Valdez 2012).[4] Under such laws, an apartment, home, or building is designated a "nuisance property" when a certain number of 911 calls are placed from the property within a set time frame, and they place the responsibility of nuisance abatement on the property owner. If the property owner fails to abate the activity and 911 calls continue, the owner can face fines, property seizure, or even jail time. For example, a typical mandate may require that the municipal police department issue a citation for a $1,000 fine upon a landlord when a tenant places three or more 911 calls linked to some criminal activity within a 12-month period. This initial citation could include a charge for the landlord to abate the criminal activity within ten days or face a subsequent fine for a larger amount. Landlords typically abate the criminal activity by evicting (or threatening to evict) the entire tenant unit (Kanovsky 2016). While many of these ordinances were touted as strategies to clamp down on drug-related crimes, Fais (2008) documents that it is not unusual for ordinances to explicitly list calling emergency services to report acts of domestic violence as an activity that warrants nuisance abatement.

As these ordinances grew more common during the 1990s, the American Civil Liberties Union and other civil rights organizations raised concerns about the potential for the laws to undermine public safety by discouraging victims from reporting assaults and other criminal activities (Lepley and Mangiarelli 2019, American Civil Liberties Union 2020). Desmond and Valdez (2012) document that criminal activity nuisance ordinances in Milwaukee, Wisconsin were highly likely to penalize calls related to domestic violence, reflecting the reluctance of many police departments to become involved in domestic violence cases. In particular, Desmond and Valdez provide the first systematic review of how police enforcement of these laws has been applied in an urban setting. They track the universe of nuisance ordinance citations that occurred in Milwaukee from 2008 to 2009 and draw the following conclusions:

[4] These ordinances are an example of a so-called third party policing strategy (Mazerolle and Ransley 2002, Mazerolle, Higginson and Eggins 2013), in which municipal police delegate certain non-police entities such as business owners or licensing bodies with the authority to regulate or punish certain crimes or the underlying behaviors that lead to crimes.

(i) a substantial portion of the citations (roughly one-third) are triggered by 911 calls from female domestic violence victims, and (ii) over 80% of property owners receiving a domestic violence-related citation abated the nuisance by evicting or threatening to evict the tenant in question.[5] Landlords also took other steps to preemptively encourage likely victims of domestic violence to leave their properties, and to prevent such women from moving into their buildings.

In addition to the seminal work by Desmond and Valdez, a series of qualitative research studies link criminal activity nuisance ordinances directly to tenant eviction (Arnold and Slusser, 2015; Desmond 2016; Mead et al., 2017), and several legal reviews raise concerns that nuisance ordinances could harm victims of domestic abuse (Fais 2008; Gavin 2014; Mead et al. 2018). Importantly, even when ordinances explicitly state that calls related to domestic violence are exempt from abatement requirements, local law enforcement and landlords do not consistently acknowledge these exemptions or apply abatement requirements differently as a result (Arnold and Slusser 2015). In addition, these laws do not prevent local law enforcement from issuing a written warning to landlords following repeated 911 calls. This means that if a woman calls 911 for domestic violence protection or medical attention, her landlord might still be notified of the incident and the woman could still be served with a cease and desist order or an alternative form of written warning. Even if the warning documents state that eviction is precluded as a possible outcome, the tenants in question often do not understand the legal language, do not know they may call the police if the are being evicted illegally, and are generally very poorly informed of their legal rights. Undocumented tenants are especially likely to fear any police interactions and thus to avoid reporting illegal eviction. One victims' advocate reported that it was not uncommon for women in this situation to panic and immediately leave their apartments for crisis housing when they received any kind of formal letter from their landlord that they did not understand (Arnold and Slusser 2015). The policies may also be used as an intimidation tool by landlords to remove undesirable

[5]The vast majority of tenants in these cases were female victims of domestic abuse, rather than the abusers themselves.

tenants (Desmond 2012, 2016).

In spite of the large body of qualitative work on criminal activity nuisance ordinances, the existing literature lacks a clear confirmation of the direct impact of the ordinances on domestic violence. In the following sections, we describe our sample, empirical methodology, and findings.

3 Data

This study uses data from Ohio because the state provides a setting in which we have reasonably high quality, municipality-level crime data, and where many municipalities enacted nuisance ordinances within a short and recent period of time. Table 1 list the Ohio cities that had enacted a nuisance ordinance up through 2016. A focus on Ohio allows us to eliminate certain potentially confounding elements like state-level institutional factors and long term trends in overall crime, segregation, and household structure.

To conduct our analysis, we merge publicly available police-reported crime incidents aggregated by the Federal Bureau of Investigations (FBI) and documentation on local city laws collected from municipal websites or LexisNexis. These data on nuisance ordinances for all Ohio cities were obtained from Mead et al. (2017) and confirmed by us.

Our main source of crime data is the FBI Unified Crime Reporting Program (UCR), which contains data from local law enforcement agencies across the country, including both the Supplementary Homicide Reports (SHR) and the National Incident-based Reporting System (NIBRS). Both the SHR and NIBRS report crimes at the incident level, and contain information about the date, location, circumstances, and method of the offenses and the demographic characteristics of victims and perpetrators. The SHR contains only records of homicides. NIBRS includes a wide range of felonies, including assaults, stalking, sexual crimes, drug crimes, and property crimes. As the UCR is a voluntary program, not all police agencies participate fully, and individual crime reports may be entered with missing information about the event or the individuals involved. We follow Amuedo-Dorantes and Deza (2022) and interpret years of non-reports as years with zero homicides. Given the voluntary nature of the UCR process it is possible that some cities with non-reporting years in fact did experience a positive number of partner homicides. To decrease these types of errors, we restrict our SHR sample to cities with at least 10,000 in population throughout the period of study, per Amuedo-Dorantes and Deza (2022).

The NIBRS participation rate is lower than that for the SHR, with fewer than half of the cities in Ohio reporting any crime incidents to NIBRS for every year of the study period (2000-2016), and only two of the cities reporting to NIBRS passed a nuisance ordinance during the study period (Cincinnati and Struthers). Given the lower quality of these data we rely on the SHR for our primary analysis and use the NIBRS data as an auxiliary sample.

We also collect data on several covariates at the city and county level. The FBI's Law Enforcement Officers Killed and Assaulted data set (LEOKA) provides information on the number of police officers and the number of police officers who are female annually at the city level. The annual county unemployment rate is obtained from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS).[6]

Finally, we collect information on the number of evictions and eviction filings, the percentage of renters, and various demographic characteristics of the cities, which we use to compare treated and control cities in the pre-period (2000-2003). These data come from the American Community Survey 5-Year Data and we obtain them from the Eviction Lab at Princeton University.

In total our SHR sample is comprised of 167 cities, 38 of which introduce a criminal activity nuisance ordinance at some point during the study period.[7] In our analysis, we will refer to 38 cities that passed a criminal activity nuisance ordinance as the treated group, and the remaining 129 as the untreated group. As the first criminal activity nuisance ordinances

[6]We are grateful to Kevin Rinz for making the data available on his website.

[7]Six of the cities listed in Table 1 have populations below 10,000 and as such were dropped from our analysis sample.

within our sample were enacted in 2004, we refer to the years 2000-2003 as the pre-period. The pre-period baseline characteristics of the treated and untreated cities in the sample are shown in Table 2. Panel A summarizes the SHR sample and Panel B describes the sample available from NIBRS. Means are population-weighted. Out of the 167 cities included, 23% (38 cities) implemented a criminal activity nuisance ordinance during the sample period. The set of treated cities does differ somewhat from the untreated group in terms of pre-period crime levels, poverty, and racial composition, but our identification strategy does not require an assumption of pre-period balance. During this period, the total homicide rate of the sample was 7.1 homicides per 100,000 residents, compared to 5.5 homicides per 10,000 in population for the United States overall in 2000. The intimate partner homicide rate for the sample was 0.525 per 100,000, which is similar to the national rate of 0.56 at that time (FBI, 2003).

Figure 3 tracks the intimate partner homicide rate over time for cities that did and did not pass a nuisance ordinance during the study period. We will refer to the cities that enacted a nuisance ordinance as the treated group, and all others as the untreated or never treated group. Only cities with at least 10,000 in population are included in our sample.[8] The series in Figure 3 are weighted by population in order to show the aggregate levels for residents of the treated and untreated set of cities. Comparing the raw trends between treated and never treated populations reveals the relative shift in the intimate partner homicide rate among treated cities that coincides with the introduction of the nuisance ordinances. Prior to 2004, the treatment group cities presented higher rates of intimate homicides than the never-treated cities. Between 2004-2006, Cleveland, several suburbs surrounding Cleveland (including Lakewood, Cleveland Heights, Shaker Heights, Bedford, Parma), Cincinnati, and Akron passed ordinances, increasing the rate of Ohioan renters living in a jurisdiction under such an ordinance from 0 to 25% in just three years. Within a couple years of this cascade

[8]Smaller municipalities are more likely to report to the UCR sporadically, and it is harder to distinguish a lack of reporting from an actual occurrence of zero intimate partner homicides. We drop cities below a minimum population of 10,000 in order to minimizes the probability of incorrectly coding non-reporting as zero homicides.

of new laws, we see the intimate partner homicide gap between the treated and untreated group of cities has closed, indicating a relative decrease in the treated cities' intimate partner homicide rates. The following section describes how we test whether this decline can be directly attributed to the nuisance ordinances.

4 Empirical Strategy

4.1 Difference-in-Differences Model

To estimate the impact of criminal-activity nuisance ordinances on intimate-partner homicides, we first utilize a generalized difference-in-differences model, also known as a two-way fixed effects model. Our research design exploits the staggered implementation of criminal activity nuisance ordinances across the set of ever treated cities. If our research design did not have variation of treatment timing across the ever-treated cities, we could estimate the treatment effect using a canonical difference-in-differences specification, that is:

$$Y_{jt} = \gamma + \gamma_j CANO_j + \gamma_t POST_t + \beta^{DD} CANO_j \times POST_t + \epsilon_{jt} \tag{1}$$

where $CANO_j = 1$ if and only if city j is the treatment group, and $POST_t = 1$ if $t \geq T$, where $T =$ the treatment year. $\hat{\beta}^{DD}$ would be the estimated difference in differences, that is: the treatment effect. However, because these laws were enacted in many cities throughout Ohio with variation across cities in when the policy turned on, we implement a city-time level model that includes indicators for the cross-sectional city units (α_j) and time periods (α_t), and a treatment indicator (D_{jt}).

$$Y_{jt} = \alpha_j + \alpha_t + \beta^{DD} D_{jt} + u_{jt} \tag{2}$$

The coefficient β^{DD} in Equation 2 represents the two-way fixed effects estimator, commonly used in situations where different groups are treated at different times.

The identifying assumption of the difference-in-differences model is that, in the absence of these policy changes, cities that passed the ordinances and cities that did not pass the ordinances would have experienced similar trends in outcomes. While we are not able to formally test the validity of this assumption, we can inform the specification with an event study analysis. In particular, we examine whether treated and untreated cities showed parallel trends in the outcome(s) of interest prior to the policy change in the treated cities. An event study depicts the difference in outcomes among the group of cities experiencing the policy change compared to a group that have not experienced the policy. To produce the event study, suppose city j introduced a policy change in year T_j^*, and we denote the number of periods relative to the policy change year as $K_{jt} = t - T_j^*$: $K_{jt} = 0$ if the nuisance ordinance was implemented in year t, 1 the following year, and so on. We regress the following model of Y_{jt}, the outcome of interest for city j in year t:

$$Y_{jt} = \tilde{\alpha}_j + \tilde{\alpha}_t + \sum_{k=-\infty}^{\infty} \gamma_k \mathbf{1}\left\{K_{jt} = k\right\} + u_{jt} \tag{3}$$

In Equation 3, we assign the omitted k category to all observations from never treated cities, so that the set of γ_k estimates represent the relative dynamic effect of the policy over time. We follow convention and omit $k = -1$. Plotting the set of γ_k values gives an indication for the validity of the parallel pre-treatment trends assumption, indicating whether or not the difference in differences estimate is likely to be biased or corrupted by other factors or concurrent trends in the treatment cities. We bin all data points for ever-treated cities prior to $k = -4$ and after $k = 4$.

4.2 Treatment Heterogeneity and Potential Bias

Several studies have pointed out that the two-way fixed effects estimator may not estimate the causal parameter of interest and is potentially biased in the presence of treatment heterogeneity over time or across units (Goodman-Bacon 2018, Callaway and Sant'Anna 2020, Abraham and Sun 2020, De Chaisemartin and D'Haultfeuille 2020a and 2020b). Additionally, Abraham and Sun (2020) demonstrate that the estimated coefficients on treatment leads and lags in a two-way fixed effects regression can also be biased when the treatment effect is heterogeneous across treatment unit groups.

To address these concerns, we adopt the estimator proposed by Callaway and Sant'Anna (2021), which is designed to be robust to heterogeneous treatment effects. Under the dual assumptions of parallel trends in an untreated counterfactual and an absence of anticipatory anticipation effects, the Callaway and Sant'Anna methodology allows us to estimate the city-year average treatment effect on the treated population (known as the ATT) for the group of cities g that enacted the criminal-activity nuisance ordinance at time t: this can be expressed as $ATT(g,t)$. All the individual city-year ATTs can be aggregated into a "simple" ATT for all cities across all years in the study sample.

The Callaway and Sant'Anna (2021) estimator is suitable for research designs when treatment is an absorbing, or irreversible, state. This implies that treatment is defined as having experienced a policy change at some point in the past, but does not rule out dynamic treatment effects. In our analysis sample, there is one city that first passed and later repealed its nuisance ordinance during the study period (Loraine). To implement the Callaway and Sant'Anna estimator, we code the repealing city as remaining treated in all years following the initial policy change, and do not change the treatment coding after the repeal year.

4.3 Sensitivity Analysis

As a sensitivity analysis, we use the robust inference method developed by Rambachan and Roth (2023) to assess quantitatively whether the existence of a trend in the period before the enactment of a nuisance ordinance poses a threat to the validity of the estimate of the policy's treatment effect. To this aim, we report the robust confidence intervals by Rambachan and

Roth (2023) for the Callaway and Sant'Anna ATT estimator.[9]

4.4 Triple Difference Analysis

To address any concerns that the pre-period trends in intimate partner homicides might not be sufficiently comparable between the treated and untreated cities, we also use a triple difference specification to supplement the findings from difference in differences and Callaway and Santana estimators.

This part of the analysis categorizes all cities in the analysis sample according to the fraction of residents that are living in rental units. We will use the term "high renter share" to describe the set of cities in which the renting share of residents was above the 75th percentile of our sample during the pre-treatment period (2000-2003). We run a triple difference model using an interaction of the policy indicator and *TOP25*, an indicator for high renter share.The triple difference effect is estimated using the following specification:

$$\begin{aligned} Y_{jt} = \beta_1 D_{jt} + \beta_2 D_{jt} \times TOP25_j \quad &+ \beta_3 TOP25_c \\ + \delta_c + \lambda_t + X^{'}_{ct}\gamma + \epsilon_{jt} \end{aligned} \tag{4}$$

where $D_{jt} = 1$ if city j had an active criminal activity nuisance ordinance during year t. The coefficient of interest in this specification is β_2, the interaction of D_{it} and *TOP25*. This coefficient captures the additional treatment effect of the laws on partner homicides in high renter cities relative to low renter cities. The identifying assumption of Equation 4 is that in the absence of any policy change, the difference in partner homicide rates between treated and never treated cities would have followed similar trends in cities with high baseline shares of renters and cities with low baseline shares of renters. To assess the support for this assumption, we estimate the event study analysis of the triple difference model with the

[9] Rambachan and Roth (2023) develop confidence intervals for the treatment effect that are uniformly valid under imposed restrictions on the potential violations of the counterfactual post-treatment trends. The robust inference method allows us to construct confidence intervals valid under imposed restrictions on the magnitude of post-treatment violation of parallel trends. More specifically, the restriction imposes that post-treatment violations cannot be greater than a constant M times the maximal pre-treatment violation.

following regression model:

$$Y_{jt} = \tilde{\beta}_j + \tilde{\beta}_t + TOP25 \sum_{k=-4}^{4} \delta_k \mathbf{1}\{K_{jt} = k\} + \sum_{k=-4}^{4} \gamma_k \mathbf{1}\{K_{jt} = k\} + u_{jt} \tag{5}$$

again assigning all never treated cities to the omitted category ($t = -1$) and binning all treatment city data prior to $t = -4$ and after $t = 4$.

5 Results

5.1 Two-way Fixed Effects

We use intimate partner homicides as our primary outcome of interest as this measure of domestic violence is the least likely to be confounded by a decrease in reporting propensity. Compared to other forms of assaults, homicides are unlikely to be under-reported and official homicide counts are considered relatively accurate and highly correlated with the true incidence of other violent crimes (Fajnzylber, Lederman, and Loayza 2002).

Table 3 shows the difference-in-differences estimate of the effect of nuisance ordinances on intimate partner homicides, using various specifications. All regressions are weighted by population, following the convention in the crime literature (Evans and Owens 2007, Chalfin and McCrary 2018, Owens and Ba 2021, Chalfin et al. 2022).[10] In addition to city and year fixed effects, we add the following controls in succession: column (2) adds the local unemployment rate (measured at the county level), column (3) adds the number of police officers per 1,000 residents and the female share of the local police force, column (4) adds the rate of non-partner homicides, and column (5) adds city-specific time trends (that is, city fixed effects times a linear time variable. The effect size is relatively stable across specification, and indicates an annual reduction in partner homicides ranging from 0.408 to

[10] The regressions are weighted using the city population in year 2000, before the first ordinance was passed in Ohio, following Chalfin et al. (2022). Using the population before the policy enactment to weigh the regressions addresses the concern that the city-level population may be endogenous because it can be affected by the nuisance ordinances.

0.425 fewer homicides per 100,000 residents. This magnitude is large relative to the sample mean of 0.77 partner homicides per 100,000 in population.

5.2 Difference-in-Differences Event Study

To assess the validity of our identifying assumption of parallel trends in the counterfactual, Figure 4 shows the event study coefficients for the estimation of Equation 3, for the outcome all homicides by an intimate partner or ex-partner. Specifically, the figure graphs the effects of the policy in each year leading up to and following the policy change, controlling for the annual county unemployment rate, the number of police officers per 1,000 inhabitants, the share of police officers that is female, and the number of non-intimate partner homicides per 100,000. The graph shows reasonably comparable trends between cities with and without ordinances in the pre-policy period, as well as a noticeable post-policy decline in this rate among cities implementing an ordinance. (The p-value of the F-test of joint significance for the pre-periods -4 to -1 is 0.249, and for the post-periods 0 to 4 it is 0.021.) While the coefficient on $t = -4$ is positive and nearly statistically significant, the coefficients from $t = -3$ onward are statistically equal to zero.

We find similarly negative results when restricting the sample to intimate partner homicides of women in Figure 5, however Figure 6 shows no statistically significant impact of the laws on intimate partner killings of male victims.

To supplement our analysis of the SHR sample, we also estimate the effect of the ordinances using data from the FBIs Unified Crime Report (UCR). Table 4 examines the ordinance effect on partner homicides in the UCR, by race and sex of the victim, and Table 5 measures the impact of the policy on assaults on women in the home. While these regressions also show negative and largely statistically significant effects, the SHR dataset remains our preferred sample due to data quality.

5.3 Estimates Using Callaway and Sant'Anna (2021)

Table 6 reports the estimates of the ATT obtained using the Callaway and Sant'Anna (2021) estimator. We find that cities that enact a criminal activity nuisance ordinance experience a 0.59 percentage points decline in IPV homicides relative to the cities that never passed an ordinance. The estimate is statistically significant at the 10 percent level. We also estimate a decline in IPV homicides when we divide the population into four subgroups based on race and gender of the victims, although the subsample estimates are not statistically significant at the conventional level. Figure 7 reports the event study estimates for the Callaway and Sant'Anna (2021) estimator. The estimates are similar to the ones shown in Figure 4, with the coefficient on $t = -4$ being positive and nearly statistically significant at the 5 percent level.

5.4 Sensitivity Analysis

Next, we use the robust inference method developed by Rambachan and Roth (2023) to assess whether the positive coefficient in $t = -4$ estimated in Figure 7 may pose a threat to the validity of the treatment-effect estimate. In Figure 8, we estimate robust confidence intervals for the Callaway and Sant'Anna estimator separately for the four coefficients in the post-treatment period: Periods $t = 1$ (a), $t = 2$ (b), $t = 3$ (c), and $t = 4$ (d). We report a robust confidence interval for two different values of the constant M, which measures how large the post-treatment violation of parallel trends is relative to the maximal pre-treatment violation of parallel trends estimated in Figure 7 using the Callaway and Sant'Anna estimator. We use $M = 1$ and $M = 2$. When $M = 1$, the robust confidence intervals are estimated under the restriction that post-treatment violations of parallel trends cannot be larger than the maximal pre-treatment violation of parallel trends estimated in in Figure 7. When $M = 2$, the robust confidence intervals are estimated under the restriction that post-treatment violations of parallel trends cannot be larger than twice the maximal pre-treatment violation of parallel trends estimated in Figure 7.

The results reported in Figure 8 show that the robust confidence intervals for all four coefficients include zero for $M = 1$, meaning that the estimates are not robust to allowing for post-treatment violations of parallel trends of the same magnitude of the maximal pre-treatment violation of parallel trends estimated in Figure 7. Therefore, based on the results in Figure 8, we conclude that the positive coefficient in $t = -4$ estimated in Figure 7 may pose a threat to the validity of the treatment effect estimate in our setting.

5.5 Triple Difference Estimates

As described previously, to address a potential concern that pre-existing domestic violence trends in the set of treated cities may be driving the negative difference-in-differences effect that we find in Table 4, we take advantage of very similar trends among treated cities and untreated cities with a high share of renters in the pre-treatment period–prior to 2004. We observe that the difference in intimate partner homicides between the treated and untreated groups follows very similar trends among cities with pre-period renter-occupied rates in the top quartile of our sample and cities with lower renter-occupied rates (see Appendix Figure A1). Accordingly, we make use of these trends by running a triple difference model that includes an interaction of the policy and a high renter share indicator.

First, the triple difference event study analysis is shown in Figure 9, which plots the coefficients from Equation 5 and shows a relative decline in intimate partner homicides following the enactment of a criminal activity nuisance ordinance.

The results of the triple difference model (Equation 4) are summarized in Table 7. Column (1) of Table 7 is the baseline model that controls only for two-way fixed effects, and Columns (2) through (5) add other covariates successively. The policy effect for for cities with renter-occupied rates below the 75th percentile is given in the row labelled "Nuisance Ordinance" (second row of coefficients), and the additional policy effect for high renter cities is the triple difference coefficient, captured by the interaction term "Nuisance Ordinance X High % Renters." The results in Table 7 show a null effect of the policy among cities with a lower renter-occupied rate, although the point estimate on this set is still negative. The triple difference coefficient shows a negative and significant effect within the high renter-share set of cities, with an effect size ranging from -0.458 to -0.560. Again, these magnitudes are large relative to the sample mean of 0.77. In specifications (1) through (4) we can rule out a null or negative effect for high renter cities with 95% confidence: the 95% confidence interval in the specification shown in column (4) ranges from -0.926 to -0.052. Essentially, these results indicate that the full difference-in-differences estimate presented in Table 4 is acting through a reduction in partner homicides within the high renter share set of cities.

When we use the triple difference model to separately examine partner homicides by type of relationship (Table 8), we see that most of the effect in Table 7 is driven by a decrease in murders committed by unmarried partners (these regressions follow the specification in column 4 of Table 7).

In Table 9, we separate the triple difference estimates by gender and race of the victim. Column (1) shows results for intimate partner homicides of white women; Column (2) reports the results for homicides of black women; Columns (3) and (4) display estimates for intimate partner homicides with white male victims and black male victims respectively.[11] These results by subsample suggest that the negative impact of criminal nuisance ordinances on intimate partner homicides appears to be driven by a decline in homicides with female black victims. For all other groups, the triple difference coefficient is not statistically different from zero. However, Wald tests cannot confirm a statistically significant difference between the measured policy effect on the homicides of white women and those of black women.

[11]While we require information about the relationship between the perpetrator and the victim to classify any homicide as an intimate partner homicide, some intimate partner homicides are missing demographic characteristics about the victim and cannot be placed into one of the four columns in Table 9. As a result, summing the triple difference coefficients across the columns in Table 9 adds up to a smaller total magnitude than the -0.489 point estimate shown in column 4 of Table 7.

6 Robustness Checks

6.1 Alternative Mechanisms

6.1.1 Selective Migration Flows

To address the possibility that our negative results may be due to some masked mechanism, we consider two alternatives to a true decline in intimate partner homicides. First, we investigate the possibility that the negative effect of the nuisance ordinances on intimate partner homicides could be driven by selective migration flows, namely out-migration of individuals with a greater propensity to experience and commit acts of domestic violence.We calculate migration into and out of Ohio cities using data collected by Infutor Data Solutions, a consumer reference agency. These data track individuals' address histories by aggregating address information collected by entities such as advertisers, subscription services, and utility companies. For each city in Ohio we construct an inflow measure defined as the number of individuals newly moving to that city each year, and an outflow measure defined as the number of individuals moving out of that city each year. We do not consider intra-city moves in constructing these measures. We measure both overall inflows and outflows, and in and out-migration by race. In Table 10 we do find increased flows of black residents both in and out of cities that introduce a nuisance ordinance; this could be consistent with an increase in evictions. However, the similar magnitudes of the in- and out-migration flows suggest that the policies do not lead to any measurable change in racial composition due to selective migration. We also rerun the triple-difference analysis of intimate partner homicides removing from the control group all non-treatment cities that border a treatment city; results are shown in Table 11. We would expect that if the negative effect observed for intimate homicides was driven by a displacement of homicides to bordering non-nuisance cities, the estimation within this revised sample would show reduced coefficients compared to the main results in Table 3. Given that we do not find this, we do not see evidence that consistently supports a migration mechanism.

6.1.2 Change in Police Officer Reporting Practices

Second, we consider the fact that this negative effect could be due to a change in police officer practices in categorizing intimate partner homicides. To check this possibility we measure the effect of the policies on all homicides of women. This is a reasonable proxy as Table 9 showed that most of the (negative) effect of the policies on partner homicides is attributable to a change in homicides of female victims, and because most homicides of women are committed by partners or ex-partners (Aizer 2010). If in fact the negative coefficients shown in Table 9 are due to a change in the reporting practices of responding officers thereby masking an increase in the incidence of partner homicides, we would expect to find a positive effect of the laws on the total number of homicides of females. Instead, Table 12 still shows a negative policy effect.

6.2 Other Outcomes

The data were not available for us to investigate the effect of the criminal activity nuisance ordinances on two important outcomes that are often used in the domestic violence literature: 911 calls and hospitalization (e.g., Hsu and Henke 2020, Leslie and Wilson 2020, Miller, Segal, and Spencer 2022, Schneider and Piazza 2023, Miller, Segal, and Spencer 2024). However, we look at several other auxiliary outcomes to supplement our analysis of partner homicides.

6.2.1 Google Trends Data

First, we used publicly available Google Trends data to investigate any increase any change in the levels of certain search topics following the enactment of a nuisance ordinance. For a given search topic or search term, Google assigns an index value that increases with the term's share of all local Google searches. A search topic covers many related specific google search terms, which are individual queries. We measured the effect of the nuisance on the following topics: "Eviction," "Family Law," and "Domestic Violence." We find positive effects on searches related to Family Law and Domestic Violence, and positive but statistically insignificant effects of the policy changes on Eviction related searches. These regression results are shown in Appendix Table A1. While this suggests there may be more concern about domestic violence following the enactment of a nuisance ordinance, it is difficult to interpret this result. For example, if the introduction of a nuisance ordinance made women more reluctant to call emergency services, one possible alternative would be for victims to look for legal aid or other services for assistance in leaving an abusive relationship. [12]

6.2.2 Evictions and Eviction Filings

We also measure the effect of the criminal activity nuisance ordinances on evictions and eviction filings within our sample. We do not consider evictions to be a traditional first stage in the relationship between the ordinances and domestic violence: the fact that criminal activity nuisance ordinances are triggered by events in the home provides a channel of connection that does not require eviction. However, eviction threat is documented as a reason why domestic violence victims are unwilling to call emergency services (Arnold and Slusser, 2015). Further, prior research has demonstrated the adverse effect of eviction on local crime rates (Glaeser, Sacerdote, Scheinkman, 1996; Chyn and Katz, 2021) which could exacerbate any baseline effect of the laws on partner homicides. We use our difference-in-differences framework to estimate the impact of the ordinances on evictions and eviction filings and the results are presented in Appendix Table A2. In columns (1) and (2) each city-year is weighted equally, and the regressions and pre-treatment means in columns (3) and (4) are weighted by population. We find a generally positive effect of the policy change on evictions and filings in cities with a fraction of renters below the 75t̂h percentile, although this effect is

[12] The Google Trends data are downloaded at the year-metro area level. The earliest year available is 2004; as such there are no pre-treatment data available for cities that passed a criminal activity nuisance ordinance from 2004-2006. Google metro areas are weakly larger than a municipality and can include several different municipalities. We calculate exposure to a nuisance ordinance during each year as the percentage of individuals within each metro area who also live in a city with a nuisance ordinance during that year. Google trends data report the relative search frequency of a topic per metro area and year, i.e., the metro area that has the highest proportion of google searches related to "Domestic violence" will receive a score of 100. An area where the "Domestic Violence" share of searches is half as great as the metro area with score 100 will receive a score of 50. The index represents the relative frequency of a search topic across locations or year, but cannot be used to compare one search topic to another.

not statistically significant in the population-weighted regressions. However, in contrast to our results for intimate partner homicides, we do not find that the impact of criminal activity nuisance ordinances in cities with a high proportion of renters is statistically different from the effect in cities with a lower proportion of renters.[13]

7 Conclusion

While criminal activity nuisance ordinances are nominally intended to reduce local crime rates, these laws have drawn criticism for their potential to increase the risk of both evictions and domestic violence. Victim advocates have raised concerns that domestic violence victims will be forced to choose between enduring repeated assaults or housing insecurity. However, the current literature does not provide definitive evidence that the ordinances increase domestic violence.

The net effect of these laws on domestic violence incidents and reported crimes is ex-ante ambiguous. Because the mandates specifically punish the act of reporting assaults to emergency services, this class of nuisance ordinances is widely expected to deter victims from calling 911 and thus decrease reporting rates (the share of crimes reported). A chilling effect on reporting would decrease the expected cost to the perpetrator of committing domestic violence and result in an increased frequency of assault, or allow domestic assault to escalate. At the same time, it is possible that the ordinances also act as a deterrent to perpetrators: even if the probability that domestic violence crimes are reported decreases, the penalty in the event of a 911 call increases to include a potential eviction. Additionally, if criminal activity nuisance ordinances increase evictions for those most likely to experience domestic violence, the laws could decrease domestic violence incidence over time by affecting the composition of the municipality population. Note that this change in population composition could also

[13] These results differ slightly from Kroeger and La Mattina [2020] because in the current paper, we report the results on evictions for the cities in our homicide sample, and we measure evictions and filings per population instead of per renter population. Additionally, in this paper, we use a triple difference analysis while in Kroeger and La Mattina (2020) we utilize a two-way fixed effects model.

occur as a result of a perceived eviction threat, and hence may impact households who have not themselves been evicted.

We contribute to this policy concern by providing an econometric analysis of the relationship between nuisance ordinances and intimate partner homicides within the state of Ohio, which experienced a rapid rise in the rate of nuisance ordinance jurisdictions within a relatively short time. We find no evidence to support the hypothesis that domestic violence increases following passage of a nuisance ordinance. We find that the ordinances decrease intimate partner homicide, with point estimates greater than 50% of the pre-treatment mean. This effect is fully attributable to relative declines in partner homicides within cities with high shares of renter-occupied homes. We also find a negative effect of the ordinances on non-fatal intimate partner assaults, although the data coverage on non-fatal domestic violence is less complete and is potentially confounded by under-reporting.

While the policy effect of these laws on reported incidents of domestic assaults and intimate partner homicides has a negative and statistically significant point estimate, the mechanism behind this result is not completely clear. We do not find evidence consistent with selective migration out of cities that enact an ordinance, or any displacement of partner homicides from nuisance ordinance cities to non-ordinance cities. We also rule out the possibility that changes in police officer reporting practices are masking an increase in partner homicides, suggesting that the ordinances might in fact deter would-be domestic violence offenders. Conversations that we had with local women's shelter providers in the greater Cleveland area provided anectdotal evidence that at least in Cleveland, police officers were more likely to respond to calls of partner violence by connecting victims with support services, rather than by pursuing eviction.

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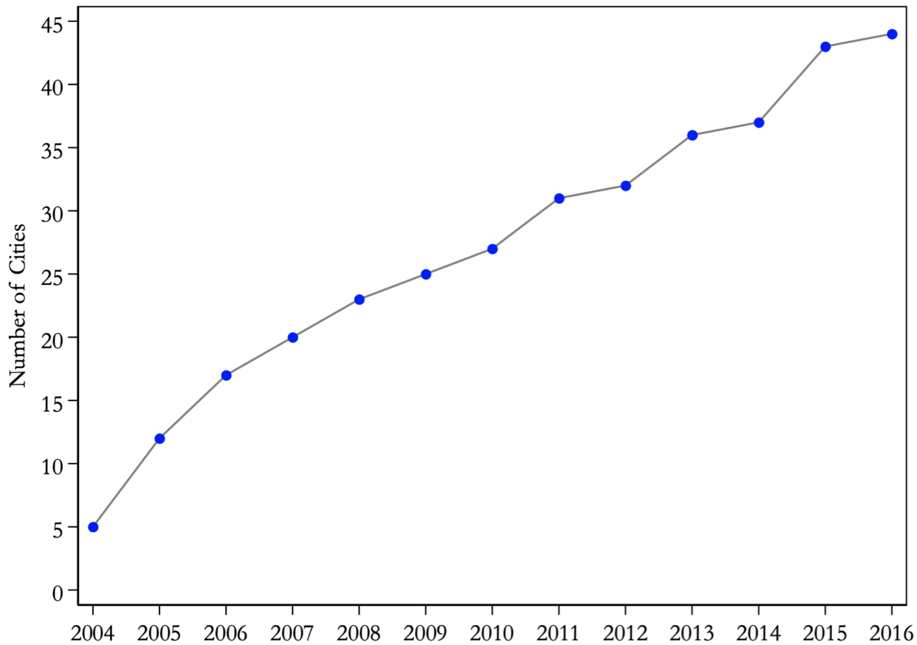


Figure 1: Criminal Activity Nuisance Ordinances in Ohio Over Time
Notes: The data on nuisance ordinances for all Ohio cities were obtained from Mead et al. (2017) and confirmed by us. The sample includes all cities in Ohio.

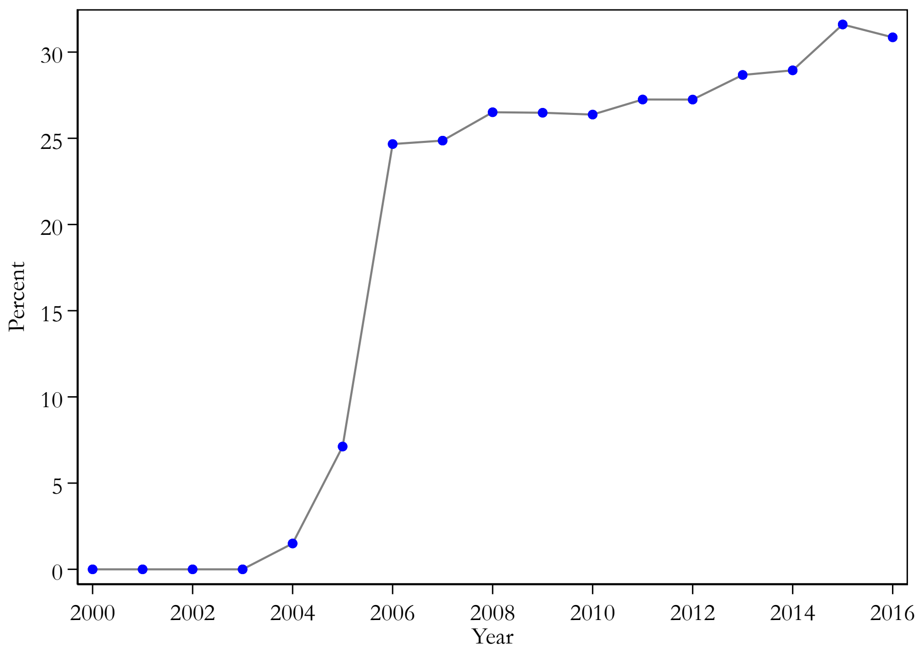


Figure 2: Percent of Ohio Renters Subject to Criminal Activity Nuisance Ordinances

Notes: The data on nuisance ordinances for all Ohio cities were obtained from Mead et al. (2017) and confirmed by us. The data on renters were obtained from The Eviction Lab. The sample includes all cities in Ohio.

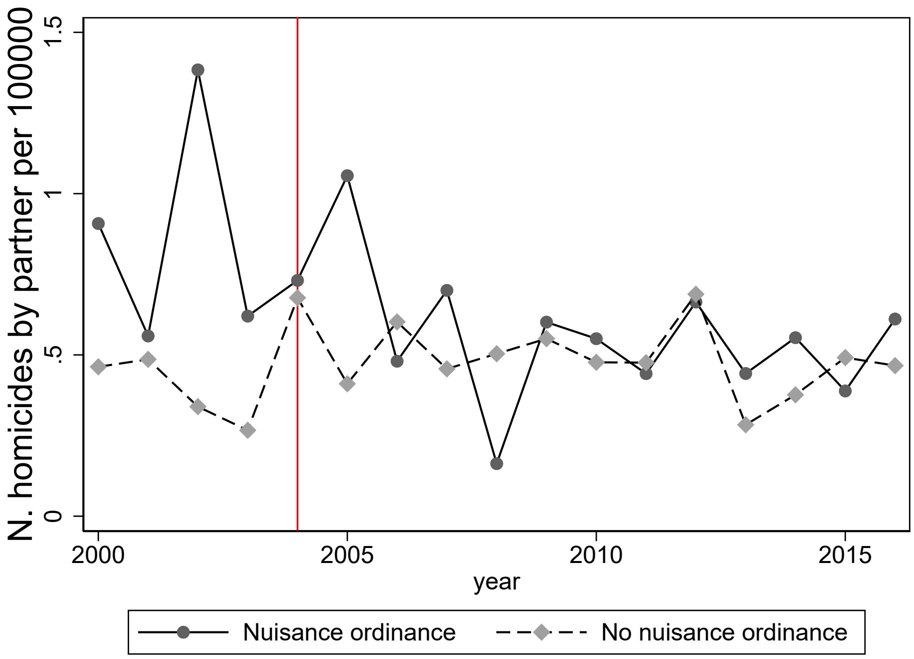


Figure 3: Intimate Partner Homicides in Ohio Cities Over Time

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. The figure shows average homicide rates weighted by 2000 city population. The vertical line indicates the year the first nuisance ordinance was adopted in Ohio (2004). The sample includes Ohio cities with a population of at least 10,000 from 2000 to 2016.

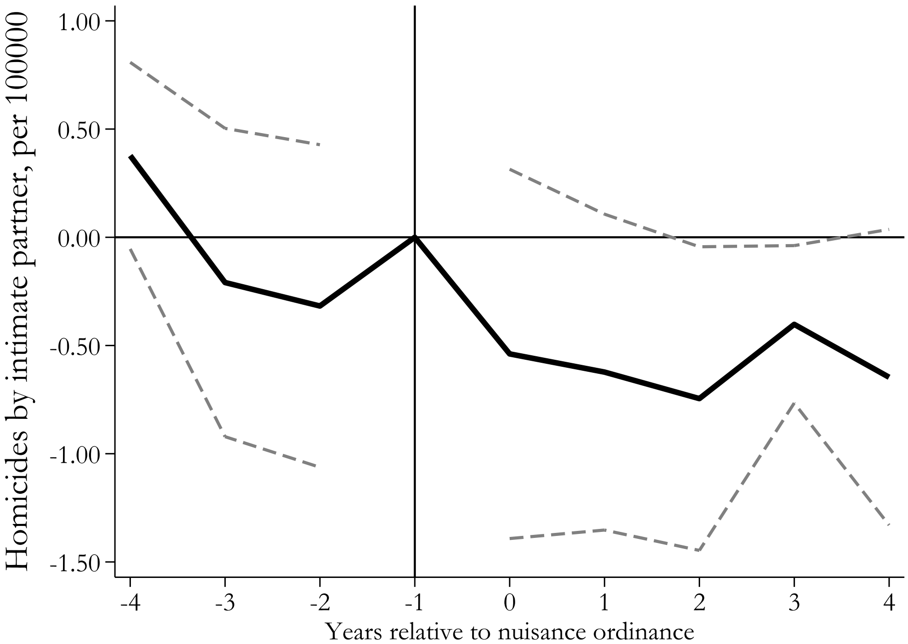


Figure 4: Event-Study Estimates of Nuisance Ordinances on All Intimate Partner Homicides

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. The graph displays coefficients and 95 percent confidence intervals for the event study coefficients on leads and lags of "Nuisance Ordinance". The regression controls for city fixed effects, year fixed effects, the annual county unemployment rate, the number of police officers per 1,000 inhabitants, the share of police officers that is female, and the number of non-intimate partner homicides per 100,000. The sample includes Ohio cities with a population of at least 10,000 from 2000 to 2016. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. The p-value of the F-test of joint significance for the pre-periods -4, -3 and -2 is 0.2488. The p-value of the F-test of joint significance for the post-periods 0, 1, 2, 3 and 4 is 0.0210.

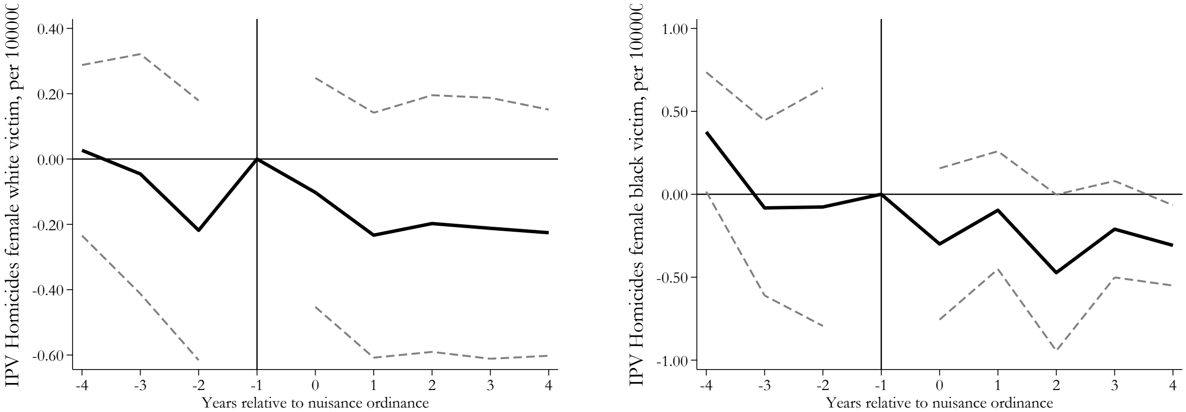


Figure 5: Event-Study Estimates of Nuisance Ordinances on Intimate Partner Homicides of Women, By Race of the Victims

Notes: See notes to Figure 4.

Female White Victims: The p-value of the F-test of joint significance for the pre-periods -4, -3 and -2 is 0.1331. The p-value of the F-test of joint significance for the post-periods 0, 1, 2, 3 and 4 is 0.8329.

Female Black Victims: The p-value of the F-test of joint significance for the pre-periods -4, -3 and -2 is 0.1010. The p-value of the F-test of joint significance for the post-periods 0, 1, 2, 3 and 4 is 0.0004.

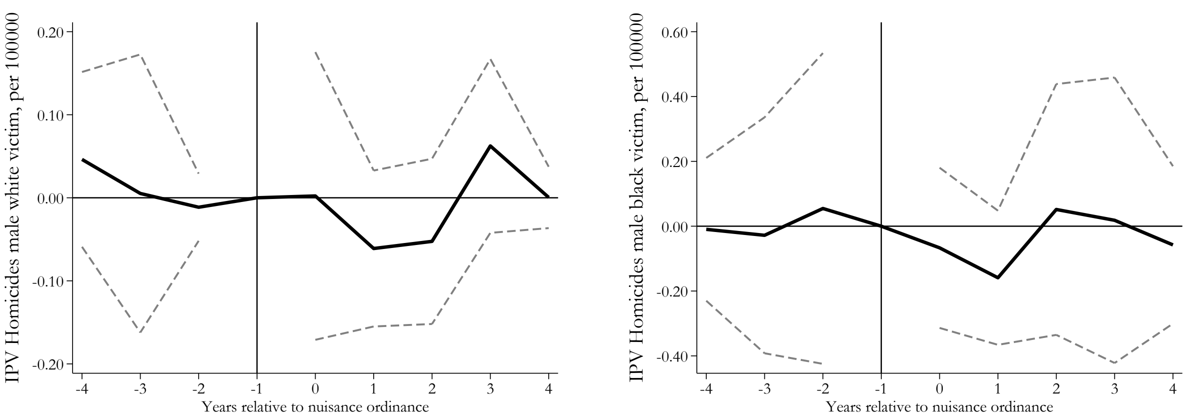


Figure 6: Event-Study Estimates of Nuisance Ordinances on Intimate Partner Homicides of Men, By Race of the Victims

Notes: See notes to Figure 4.

Male White Victims: The p-value of the F-test of joint significance for the pre-periods -4, -3 and -2 is 0.746. The p-value of the F-test of joint significance for the post-periods 0, 1, 2, 3 and 4 is 0.146.

Male Black Victims: The p-value of the F-test of joint significance for the pre-periods -4, -3 and -2 is 0.0046. The p-value of the F-test of joint significance for the post-periods 0, 1, 2, 3 and 4 is 0.0001.

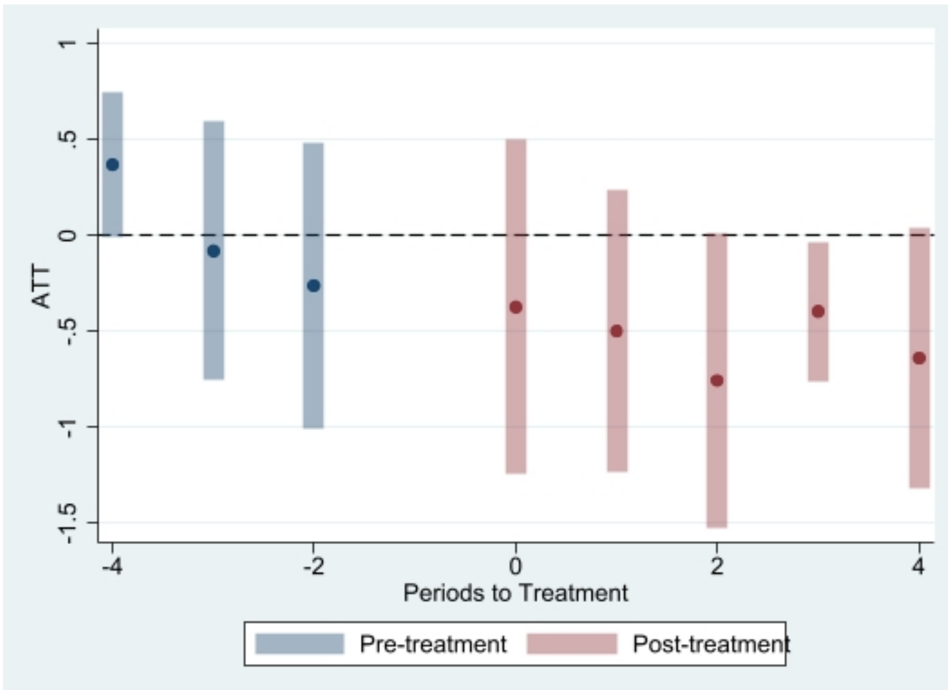
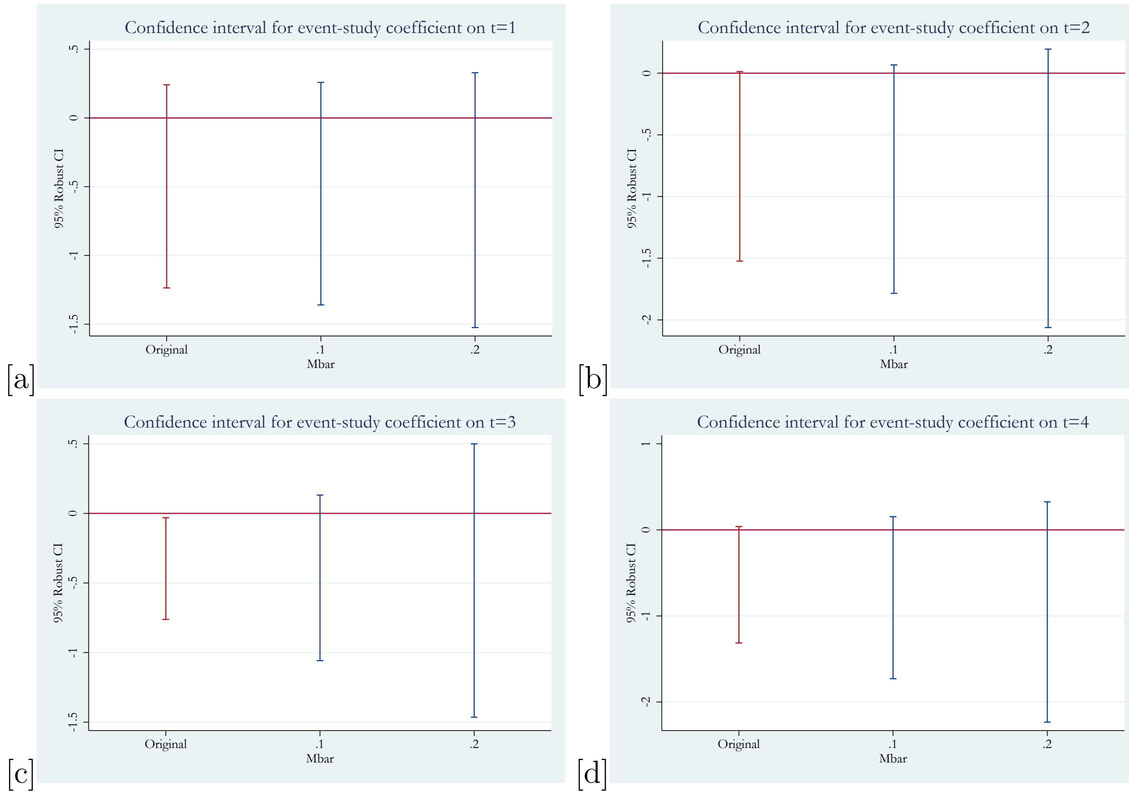


Figure 7: Callaway & Sant'Anna (2021) Event-Study Estimates of Nuisance Ordinances on Intimate Partner Homicides of Women

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. The graph displays coefficients and 95 percent confidence intervals for the event study coefficients on leads and lags of relative to treatment, where treatment is the enactment of a nuisance ordinance. The time-invariant covariates are: the county unemployment rate in year 2000, the number of police officers per 1,000 inhabitants in year 2000, the share of police officers that is female in year 2000, and the number of non-intimate partner homicides per 100,000 in year 2000. The sample includes Ohio cities with a population of at least 10,000 from 2000 to 2016. The 2000 city population is used as weight. Robust standard errors clustered at the city level. The plot was generated using the Stata command *csdid2*.

Figure 8: Rambachan and Roth (2023) Robust Confidence Intervals (CI)



Notes: The robust confidence intervals were constructed using the Stata command *honestdid*.

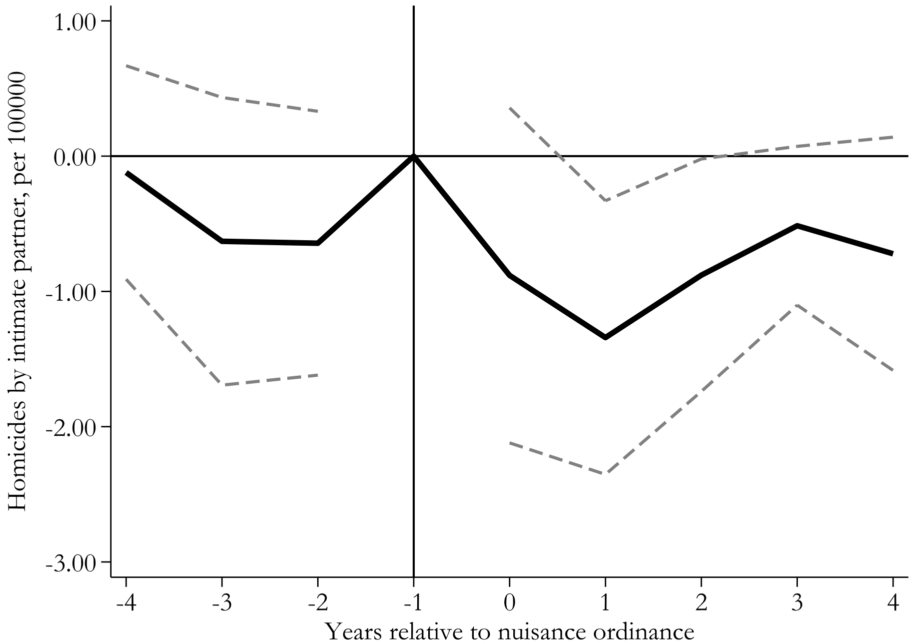


Figure 9: Event-Study Estimates of Nuisance Ordinances on All Intimate Partner Homicides, Triple Difference Analysis

Notes: The graph displays coefficients and 95 percent confidence intervals for the event study coefficients on leads and lags of the interaction between "Nuisance Ordinance" and an indicator for having a fraction of renter-occupied homes above the 75th percentile in years 2000-2003 (average across years). Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. The regression controls for year fixed effects, city fixed effects, the annual county unemployment rate, the number of police officers per 1,000 inhabitants, the share of police officers that is female, the number of non-intimate partner homicides per 100,000, and interactions between an indicator for having a fraction of renter-occupied homes above the 75th percentile in years 2000-2003 and year fixed effects. The sample includes Ohio cities with a population of at least 10,000 from 2000 to 2016. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population.

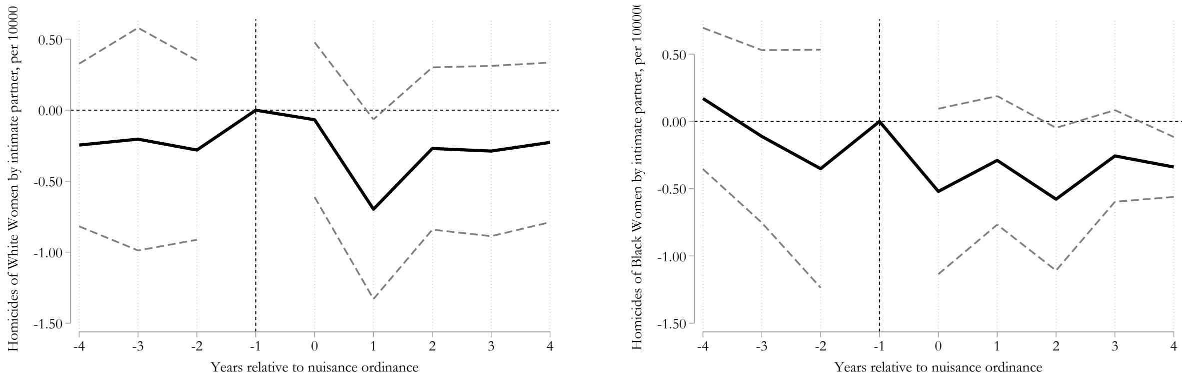


Figure 10: Event-Study Estimates of Nuisance Ordinances on Intimate Partner Homicides of Women, By Race of the Victims, Triple Difference Analysis
Notes: See notes to Figure 9.

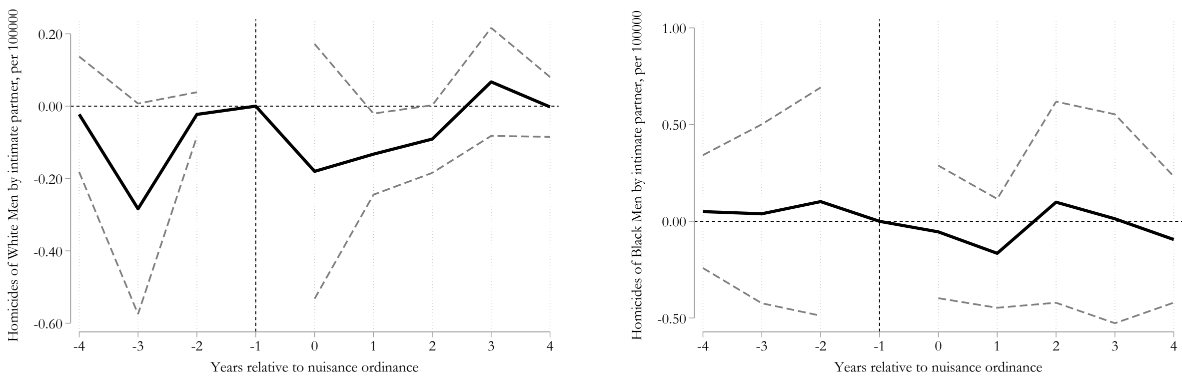


Figure 11: Event-Study Estimates of Nuisance Ordinances on Intimate Partner Homicides of Men, By Race of the Victims, Triple Difference Analysis
Notes: See notes to Figure 9.

Table 1: Ohio Cities with Nuisance Ordinances

City	Year Criminal Activity Nuisance Ordinance Enacted	Year Criminal Activity Nuisance Ordinance Repealed
Fairview Park	2004	
Kent	2004	
Sandusky	2004	
South Euclid	2004	
University Heights	2004	
Akron	2005	
Barberton	2005	
Bedford	2005	
Brooklyn	2005	
Brunswick	2005	
Parma	2005	
Shaker Heights	2005	
Campbell	2006	
Cincinnati	2006	
Cleveland	2006	
Euclid	2006	
Maple Heights	2006	
Bedford Heights	2007	
Cheviot	2007	
North College Hill	2007	
Lakewood	2008	
North Olmsted	2008	
Painesville	2008	
Lyndhurst	2009	
Orrville	2009	
Aurora	2010	
Norton	2010	
Ashtabula	2011	
East Liverpool	2011	
Garfield Heights	2011	
Ravenna	2011	
Struthers	2012	
Eaton	2013	
Lorain	2013	2016
Niles	2013	
Wadsworth	2013	
Chillicothe	2014	
Alliance	2015	
Avon Lake	2015	
Cleveland Heights	2015	
Fairborn	2015	
Fairlawn	2015	
Middletown	2015	
Warrensville Heights	2016	

Notes: The data on nuisance ordinances for all Ohio cities were obtained from Mead et al. (2017) and confirmed by us. The sample includes all cities in Ohio.

Table 2: Baseline Characteristics of Treated and Untreated Cities (2000-2003)

Panel A: SHR sample (38 Treated cities, 129 untreated cities)	Mean Treated	Mean Control	Difference In means
N. homicides by partner per 100000	0.874	0.390	0.484***
... with female & black victim per 100000	0.348	0.074	0.275***
... with male & black victim per 100000	0.168	0.043	0.125***
... with female & white victim per 100000	0.268	0.249	0.019
... with male & white victim per 100000	0.077	0.024	0.052*
N. non-intimate homicides per 100000	7.579	5.314	2.265***
Poverty rate	16.831	12.034	4.797***
Renter occupied rate	43.609	37.129	6.480***
Fraction White	63.883	80.598	-16.715***
Fraction Black	29.207	13.700	15.507***
Fraction Hispanic	3.572	2.053	1.518***
Police officers per 1000 pop	2.667	2.456	0.211**
Female share of police officers	0.111	0.088	0.024***
County level unemployment rate	4.669	5.002	-0.333***
Panel B: NIBRS sample (24 Treated cities, 137 untreated cities)			
N. homicides by partner per 100000	0.642	0.138	0.504***
IPV assaults and sex crime, per 10000	55.546	33.939	21.607***
IPV assaults, per 10000	54.920	33.667	21.253***
Poverty rate	16.845	12.208	4.637***
Renter occupied rate	48.618	37.179	11.439***
Fraction white	65.401	78.760	-13.359***
Fraction black	29.776	15.861	13.915***
Fraction Hispanic	1.345	1.776	-0.431***
Officer rate per 1000 pop	2.560	2.438	0.123
Share of female officers	0.134	0.091	0.043***
County level unemployment rate	5.053	5.452	-0.399**

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean is weighted by the city-level population in year 2000.

Table 3: Difference-in-Differences Estimates of the Effect of Nuisance Ordinances on Intimate Partner Homicides

	(1)	(2)	(3)	(4)	(5)
	Intimate Partner Homicides per 100,000 (mean=0.77)				
Nuisance ordinance	-0.413***	-0.411***	-0.414***	-0.425***	-0.408**
	(0.133)	(0.130)	(0.135)	(0.130)	(0.162)
City fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
City controls	No	Yes	Yes	Yes	Yes
Police controls	No	No	Yes	Yes	Yes
Non-IPV Homicides	No	No	No	Yes	Yes
City FE x Year Trend	No	No	No	No	Yes
R^2	0.194	0.194	0.194	0.194	0.249
N	2839	2839	2833	2833	2833

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy.

Table 4: Reported IPV Homicides by Race and Gender

DV: Partner homicides per 100K (mean 0.768)	(1)	(2)	(3)
All victims (mean=0.768)	-0.446***	-0.446***	-0.491***
	(0.137)	(0.140)	(0.155)
Female victim (mean=0.560)	-0.368***	-0.366***	-0.459***
	(0.134)	(0.138)	(0.150)
Male victim (mean=0.208)	-0.077*	-0.079**	-0.028
	(0.041)	(0.039)	(0.036)
White victim (mean=0.317)	-0.172**	-0.167*	-0.241**
	(0.085)	(0.086)	(0.104)
Black victim (mean=0.437)	-0.251***	-0.257***	-0.229***
	(0.060)	(0.063)	(0.088)
Observations	2,856	2,856	2,856
Number of clusters	168	168	168
Controls		X	X
City linear trends			X

Notes: Data come from the FBI Unified Crime Reports. Sample includes all Ohio cities with population greater than 100,000, for years 2000-2016. Robust standard errors clustered at the city level reported in parentheses. All regressions are weighted by population. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Reported Assaults on Women at Home

DV: Assaults per 10K (sample mean 95.77)	(1)	(2)	(3)
Nuisance ordinance	-38.16**	-48.40***	-49.10**
	(17.81)	(17.99)	(22.51)
City controls	No	Yes	Yes
Police controls	No	Yes	Yes
Non-IPV Homicides	No	Yes	Yes
City FE x Year Trend	No	No	Yes
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R^2	0.872	0.890	0.905
N	1188	1185	1185

Notes: Data come from the FBI Unified Crime Reports. Sample includes all Ohio cities with population greater than 100,000, for years 2000-2016. Robust standard errors clustered at the city level reported in parentheses. Regressions are weighted by population. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Callaway and Sant'Anna Estimator

Dependent variable	ATT	Standard Error	P-value
All IPV homicides	-0.588	0.329	0.074
IPV homicides with a female black victim	-0.255	0.170	0.134
IPV homicides with a female white victim	-0.161	0.188	0.391
IPV homicides with a male black victim	-0.098	0.119	0.409
IPV homicides with a male white victim	-0.006	0.037	0.870

Notes: The table reports the simple ATT, which estimates the ATT for all cities across all years. We use the Stata *csdid* command. Asymptotic standard errors clustered at the city level are obtained using influence functions.

Table 7: Triple-Difference Estimates of the Effect of Nuisance Ordinances on Intimate Partner Homicides

	(1)	(2)	(3)	(4)	(5)
	Intimate Partner Homicides per 100000 (mean=0.77)				
Nuisance Ordinance ×					
High % Renters	-0.458**	-0.473**	-0.468**	-0.489**	-0.560*
	(0.196)	(0.227)	(0.225)	(0.223)	(0.312)
Nuisance Ordinance	-0.0995	-0.0919	-0.0886	-0.0885	-0.0844
	(0.148)	(0.151)	(0.153)	(0.153)	(0.241)
City fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
City controls	No	No	Yes	Yes	Yes
Police controls	No	No	Yes	Yes	Yes
Non-IPV Homicides	No	No	No	Yes	Yes
High % Renters × Year FE	No	Yes	Yes	Yes	Yes
City FE x Year Trend	No	No	No	No	Yes
R^2	0.196	0.205	0.205	0.205	0.258
N	2839	2839	2833	2833	2833

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports (SHR) in the Uniform Crime Reports. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy. "High % Renters" is defined as having a fraction of renters-occupied homes above or equal to the 75^{th} percentile in years 2000-2003 (average across years).

Table 8: Triple-Difference Estimates of the Effect of Nuisance Ordinances on Intimate Partner Homicides, By Type of Relationship

	(1)	(2)	(3)
	Intimate Partner Homicides by Relationship		
	Current Spouse	Former Spouse	Unmarried Partner
Nuisance Ordinance × High Fraction Renters	0.0627	-0.137	-0.414***
	(0.110)	(0.0953)	(0.135)
Nuisance Ordinance	-0.128	0.0746	-0.0355
	(0.0783)	(0.0838)	(0.0895)
City controls	Yes	Yes	Yes
Police controls	Yes	Yes	Yes
Non-IPV Homicides	Yes	Yes	Yes
High Fraction Renters × Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R$\hat{2}$	0.0800	0.0851	0.226
N	2833	2833	2833

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports (SHR) in the Uniform Crime Reports. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. All regressions include city controls, police controls, non-IPV homicides per 100,000 population, city fixed effects and year fixed effects. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy. "High % Renters" is defined as having a fraction of renters-occupied homes above or equal to the 75^{th} percentile in years 2000-2003 (average across years).

Table 9: Triple-Difference Estimates of the Effect of Nuisance Ordinances on Intimate Partner Homicides, By Characteristics of Victims

	(1)	(2)	(3)	(4)
	Intimate Partner Homicides with Victim			
	Female		Male	
	White	Black	White	Black
Nuisance Ordinance X				
High % Renters	-0.190	-0.210**	-0.0203	-0.0562
	(0.138)	(0.0848)	(0.0694)	(0.0631)
Nuisance Ordinance	-0.0447	-0.0334	0.0126	-0.00920
	(0.0927)	(0.0670)	(0.0614)	(0.0354)
R^2	0.0980	0.255	0.0754	0.259
N	2833	2833	2833	2833
	Wald test statistic (p-value)			
Female White vs. Male White	6.51 (0.011)			
Female Black vs. Male Black	4.62 (0.032)			
Female White vs. Female Black	0.01 (0.932)			
Male White vs. Male Black	3.31 (0.069)			

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports (SHR) in the Uniform Crime Reports. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. All regressions include city controls, police controls, non-IPV homicides per 100,000 population, city fixed effects, year fixed effects and the interaction between "High % Renters" and year fixed effects. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy. "High % Renters" is defined as having a fraction of renters-occupied homes above or equal to the 75^{th} percentile in years 2000-2003 (average across years).

Table 10: Migration Flows

	(1)	(2)	(3)	(4)
	Log White in-migration	Log Black in-migration	Log White out-migration	Log Black out-migration
Nuisance ordinance	0.0125	0.0835*	-0.00408	0.0839*
	(0.0231)	(0.0414)	(0.0284)	(0.0398)
Obs.	2833	2833	2833	2833

Notes: Controls include the share of the population under the poverty line, the percent of housing units that are renter occupied, the share white, and city and year fixed effects. Robust standard errors are clustered at the city level, shown in parentheses. * p<0.05, ** p<0.01, *** p<0.001 .

Table 11: Robustness checks: Triple-Difference Estimates Excluding Bordering Cities

	(1)	(2)	(3)	(4)	(5)
	Intimate Partner Homicides per 100000				
CANO × High Fraction Renters	-0.473**	-0.523**	-0.544**	-0.584***	-0.628**
	(0.200)	(0.227)	(0.222)	(0.221)	(0.310)
Nuisance Ordinance	-0.125	-0.102	-0.0990	-0.0930	-0.0927
	(0.154)	(0.160)	(0.163)	(0.165)	(0.257)
City controls	No	No	Yes	Yes	Yes
Police controls	No	No	Yes	Yes	Yes
Non-IPV Homicides	No	No	No	Yes	Yes
High Fraction Renters × Year FE	No	Yes	Yes	Yes	Yes
City FE x Year Trend	No	No	No	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
$R\hat{2}$	0.216	0.225	0.227	0.228	0.277
N	2278	2278	2272	2272	2272

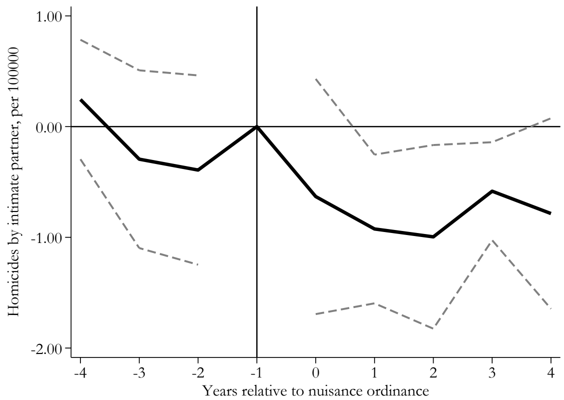
Notes: A bordering city is a Ohio city that never passed a nuisance ordinance borders with at least a city that passed a nuisance ordinance, in the years after the neighboring city passed the law. Data on homicides come from the FBI's Supplementary Homicide Reports (SHR) in the Uniform Crime Reports. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy. "High % Renters" is defined as having a fraction of renters-occupied homes above or equal to the 75^{th} percentile in years 2000-2003 (average across years).

Table 12: Triple-differences Estimates of the Effect of Nuisance Ordinances on All Homicides of Females

	(1)	(2)	(3)
Homicides with female victim per 100,000			
CANO × High Fraction Renters	-0.348	-0.209	-0.205
	(0.267)	(0.355)	(0.361)
Nuisance Ordinance	0.175	0.161	0.143
	(0.183)	(0.195)	(0.193)
City controls	No	No	Yes
Police controls	No	No	Yes
High Fraction Renters × Year FE	No	Yes	Yes
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R$\hat{2}$	0.438	0.445	0.445
N	2839	2839	2833

Notes: Data on homicides come from the FBI's Supplementary Homicide Reports in the Uniform Crime Reports. Robust standard errors clustered at the city level. Regressions are weighted using 2000 city population. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy. "High % Renters" is defined as having a fraction of renters-occupied homes above or equal to the 75^{th} percentile in years 2000-2003 (average across years).

Online Appendix



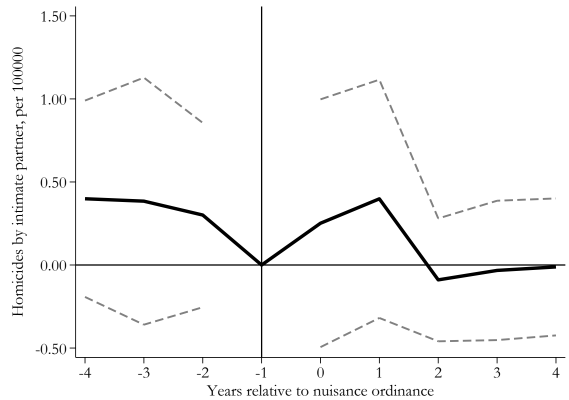


Figure A1: Event-Study Estimates of Nuisance Ordinances on Intimate Partner Homicides of Men, By Baseline Share of Renters, Difference-in-differences Analysis

Notes: Cities with a high baseline share of renters (top 25%) are in the left panel; cities with a low baseline share of renters are in the right panel. See notes to Figure 4.

Table A1: Google Trends, at Metro Area

	(1) DV Searches	(2) Eviction	(3) Family Law
Percent of Metro Area with CANO	31.51*	16.48	41.42**
	(16.48)	(21.76)	(18.42)
Year FEs	Yes	Yes	Yes
Metro Area FEs	Yes	Yes	Yes
Clustered at Metro Area	Yes	Yes	Yes
Mean of Outcome	65.34	57.85	51.14
N	2158	2158	2158

Notes: Controls include the share of the population under the poverty line, the percent of housing units that are renter occupied, the share white, and city and year fixed effects. Robust standard errors are clustered at the city level, shown in parentheses. * p<0.05, ** p<0.01, *** p<0.001 .

Table A2: Triple-differences Estimates of the Effect of Nuisance Ordinances on Evictions and Eviction Filings per 1000 Residents

	(1) Eviction Rate	(2) Eviction Filing Rate	(3) Eviction Rate	(4) Eviction Filing Rate
CANO × High Fraction Renters	1.109	0.713	-1.090	1.509
	(1.054)	(1.500)	(1.839)	(2.496)
Nuisance Ordinance	0.522*	1.052**	0.657	1.301*
	(0.272)	(0.521)	(0.407)	(0.729)
City controls	Yes	Yes	Yes	Yes
Police controls	Yes	Yes	Yes	Yes
Non-IPV Homicides	Yes	Yes	Yes	Yes
High Fraction Renters × Year FE	Yes	Yes	Yes	Yes
Weights	No	No	Yes	Yes
$R\hat{2}$	0.844	0.911	0.842	0.904
N	2685	2685	2685	2685

Notes: The rate is the number of evictions or eviction filings per 1000 residents. All regressions include city controls, police controls, non-IPV homicides per 100,000 population, city fixed effects and year fixed effects. City controls include the annual county unemployment rate. Police controls include the number of police officers per 1,000 inhabitants and the share of police officers that is female. Non-IPV homicides is the number of homicides in which the relationship between offender and victim is not described as intimate partners, per 100,000 population. The sample includes Ohio cities with a population of at least 10,000 during the period 2000-2016. The mean of the dependent variable is the weighted average of the dependent variable in cities that adopted a nuisance ordinance for the years before they passed the policy. "High % Renters" is defined as having a fraction of renters-occupied homes above or equal to the 75^{th} percentile in years 2000-2003 (average across years).