

2019

# Why She Didn't Just Leave: The Effect of Nuisance Ordinances on Domestic Violence

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Why She Didn't Just Leave:  
The Effect of Nuisance Ordinances on Domestic Violence

Emily Moss

Submitted in Partial Fulfillment of the  
Prerequisite for Honors in Economics

April 2019

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## Acknowledgements

This thesis would not have been possible without my advisor, Kartini Shastry, whose willingness to grant me the space to run with this project was invaluable. Studying and researching with her over the past two and a half years has been one of my most rewarding experiences at Wellesley. I have endless appreciation for the amount of time, expertise, and enthusiastic support she has invested in my work. I know that I would not be where I am now without her persistent belief in me – both professionally and personally. I only hope that one day I can be a mentor for others the way she has been a mentor for me.

Thank you to Eric Hilt for his guidance during the thesis process and for providing a forum of insightful feedback through the Economics Research Seminar. I am grateful to the entire Economics Department for the passion they bring to the discipline and for their commitment to teaching women economics. I am particularly grateful to Sheila Datz for her daily encouragements; to Dan Fetter for generously introducing me to economics research so early in my Wellesley career; to Dan Sichel for being a role model in both scholarship and service since my first days at Wellesley; and to Courtney Coile for four years of energizing conversations and thoughtful mentorship. Additional thanks to Kristin Butcher and Kathy Moon for serving on my thesis committee and for always challenging me to think analytically and critically.

I am grateful to Julia Tomassilli and the staff of the Open Justice initiative at the California Department of Justice for their work on the California Women's Health Survey and the Domestic Violence-Related Calls for Assistance datasets, respectively. I am also grateful to the Knapp Social Science Center for awarding a Knapp Social Science Fellowship in support of this project.

Thank you to my friends – especially Aksha, Kat, Sophie, and Stephen – for being consistent sources of both levity and love.

I would also like to acknowledge the women guests of the Harvard Square Homeless Shelter whom I have been privileged to meet over the last few years. Their stories have profoundly impacted my worldview, and I wrote this thesis out of deep respect for their bravery.

Finally, I am infinitely grateful to my mom for raising me to be the imperfect but resilient woman that I continue to grow into today. Her compassion, selflessness, and strength are my greatest inspirations. This thesis is dedicated to her.

## **Abstract**

Municipalities throughout the country have adopted nuisance ordinances to recruit landlords in assisting with crime control. Nuisance ordinances are municipality-level policies that sanction landlords if police are frequently called to respond to a landlord's tenants. To avoid a fine, landlords must abate tenants whose conduct was considered a nuisance under the municipality's ordinance; most often, this abatement process involves evicting the tenant. Little research has evaluated the impact of nuisance ordinances on innocent tenants – particularly, female victims of domestic violence. Because domestic violence is seldom excluded from the list of nuisance activities considered under the ordinances, legal advocates have warned that the threat of eviction associated with nuisance ordinances could discourage domestic violence victims from reporting their abuse to the police. This thesis contributes the first econometric analysis of the effect of nuisance ordinances on domestic violence reporting and incidence. The variation in nuisance ordinance enactment across municipalities and over time provides the framework to identify the causal effect of nuisance ordinances on domestic violence using a difference-in-differences strategy. I find that nuisance ordinance enactment leads to a 16.5-23.2 percent reduction in domestic violence-related 911 calls for assistance and a 0.4-0.7 percentage point increase in self-reported domestic violence incidence in California. Nationwide, I also find nuisance ordinance enactment is associated with a statistically significant increase in online search activity related to domestic violence as a proxy for domestic violence incidence. These results suggest that nuisance ordinances have the unintended consequence of discouraging domestic violence victims from reporting their abuse, thereby allowing the incidence of domestic violence to persist.

## Contents

1	Introduction.....	1
2	Background and Literature Review .....	4
2.1	Nuisance Ordinances .....	4
2.2	Domestic Violence Reporting.....	7
2.3	Hypotheses on the Relationship Between Ordinances and Violence .....	8
3	Empirical Strategy .....	9
4	Data and Results .....	11
4.1	California Analysis .....	12
4.1.1	Effect on Reported Domestic Violence in California .....	12
4.1.2	Effect on Domestic Violence Incidence in California .....	14
4.1.3	Addressing Threats to Identification.....	16
4.2	Nationwide Proxies for Domestic Violence Incidence.....	21
4.2.1	Female Mortality from Assault.....	22
4.2.2	Google Trends.....	24
5	Conclusion .....	26
	References.....	29
	Tables.....	34
	Figures.....	41
	Appendix.....	43

# 1 Introduction

Nearly 20 people are physically abused by an intimate partner in the United States each minute (National Coalition Against Domestic Violence). Intimate partner abuse accounts for three quarters of all violence against women (Tjaden & Thoennes, 2000); most incidents of intimate partner abuse occur in the home where women represent 85 percent of domestic violence victims (Catalano, 2015; Fais, 2008). Beyond the physical and emotional harm domestic violence victims directly suffer, domestic violence also inflicts significant costs on society at-large. Approximately 4.1 billion dollars in annual medical costs are associated with domestic violence-related injuries (Centers for Disease Control and Prevention, 2003). Domestic violence-related injuries additionally impair victims' ability to work, resulting in total annual costs of nearly 0.9 billion dollars in lost productivity from paid work outside the home as well as unpaid household chores (Centers for Disease Control and Prevention, 2003). Though the prevalence and costs of domestic violence are already high, they are likely underestimates as incidents of domestic violence are often unreported (Aizer & Dal Bó, 2009; Catalano, 2015; Tauchen & Witte, 1995).

Nuisance ordinances could pose one barrier to reporting facing domestic violence victims. Municipalities throughout the country have adopted nuisance ordinances to recruit landlords in assisting with crime control. Nuisance ordinances are municipality-level policies that sanction landlords if police are frequently called to respond to a landlord's tenants (Fais, 2008). To avoid a fine, landlords must abate tenants whose conduct was considered a nuisance under the municipality's ordinance; most often, this abatement process involves evicting the tenant (Desmond & Valdez, 2012; Greif, 2018). Proponents of nuisance ordinances argue that the ordinances improve the efficiency of police resource utilization, protect tenant safety, and deter tenants – including resident perpetrators of domestic violence – from engaging in criminal activity (Arnold & Slusser, 2015; Desmond & Valdez, 2012; Fais, 2008; Greif, 2018; Thacher, 2008). Yet because domestic violence is seldom excluded from the list of nuisance activities considered under the ordinances, legal advocates<sup>1</sup> have warned that the threat of eviction associated with nuisance ordinances could discourage domestic violence victims from reporting their abuse to the police (Arnold & Slusser, 2015; Desmond & Valdez, 2012; Fais, 2008). Despite conflicting perspectives on the merit of nuisance ordinances as a crime prevention policy, little empirical research has

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<sup>1</sup> e.g. American Civil Liberties Union (“I Am Not a Nuisance”), National Housing Law Project (“Nuisance and Crime-Free Ordinances Initiative”), Sargent Shriver National Center on Poverty Law (“The Cost of Being ‘Crime Free’”).

elucidated the impact of nuisance ordinances on innocent tenants – particularly, female victims of domestic violence.

This paper contributes the first econometric analysis of the effect of nuisance ordinances on domestic violence reporting and incidence. It is important to consider not only the impact of nuisance ordinances on reported domestic violence, but also on domestic violence incidence because nuisance ordinances could plausibly affect the proportion of domestic violence incidents reported. I hypothesize that if a nuisance ordinance is enacted in a municipality, then reports of domestic violence will decrease because of the potential consequence of eviction that nuisance ordinances impose on calling for police services. The expected change in domestic violence incidence following ordinance enactment is ambiguous. Nuisance ordinances could discourage abusers from perpetrating domestic violence if an abuser fears their actions will be reported; regardless of whether criminal charges are pursued, an abuser could still face eviction under a nuisance ordinance. Alternatively, domestic violence incidence could remain unchanged or even increase if victims are unable to call for help.

To evaluate this hypothesis, I employ a difference-in-differences strategy comparing changes in domestic violence reporting and incidence in municipalities that enacted nuisance ordinances relative to those that did not. The identifying assumption necessary to interpret my results as causal depends on parallel trends in domestic violence across municipalities; I demonstrate my results are robust to the inclusion of county-specific linear trends as well as other time-varying control variables to strengthen the validity of the parallel trends assumption. I also conduct several event study analyses to confirm my results and further evaluate potential bias from differential trends. Due to data availability, my analysis of reported domestic violence is limited to California where I use data on domestic violence-related 911 calls for assistance. I also use the California Women's Health Survey as a measure of domestic violence incidence in California, as well as data on female mortality from assault from the National Vital Statistics System and Google Trends data on online search activity related to domestic violence as additional proxies for domestic violence incidence on a national scale.

My results support the hypothesis that nuisance ordinances cause a decline in reported domestic violence, while domestic violence incidence persists. I find that nuisance ordinance enactment leads to a 16.5-23.2 percent reduction in domestic violence-related 911 calls for assistance and a 0.4-0.7 percentage point increase in self-reported domestic violence incidence in

California. Nationwide, I do not find evidence that nuisance ordinance enactment affects domestic violence incidence as measured by female mortality from assault. However, this does not necessarily imply nuisance ordinances hold no effect over domestic violence incidence nationwide. Analyzing the impact of municipality-level nuisance ordinances on county-level mortality data introduces measurement error that could bias my results to zero. Furthermore, mortality is a severe outcome of domestic violence. I do find nuisance ordinance enactment is associated with a statistically significant increase in online search activity related to domestic violence, as a less extreme proxy for domestic violence incidence on a national scale.

My findings contribute to a growing literature on the economics of crime. Most closely related is research on nuisance ordinances; however, existing research on nuisance ordinances is either focused on the legal or policy background of nuisance ordinances (Cameron, 2012; Fais, 2008; Gavin, 2014; Kastner, 2015; Mead et al., 2018; Moran-McCabe, Gutman, & Burris, 2018; Seiler, 2008), is qualitative in methodology (Arnold, 2019; Arnold & Slusser, 2015; Greif, 2018), or is limited to single-city case studies of nuisance ordinances' effects (Desmond & Valdez, 2012). This paper provides the first causal estimates of the impact of nuisance ordinances. More broadly, this paper contributes to our understanding of the interactions between police and communities, and their implications for criminal activity. Social science research has devoted considerable attention to traditional policing efforts (Braga, Welsh, & Schnell, 2015; Kent & Jacobs, 2005); less attention has been paid to the rise of third-party policing mechanisms like nuisance ordinances that outsource crime control to landlords or other community stakeholders (Kraakman, 1986; Mazerolle & Ransley, 2002). Lastly, this paper contributes to the domestic violence literature in economics by studying both reported domestic violence as well as domestic violence incidence, providing insight into determinants of whether victims report domestic violence and consequences of policies that affect reporting (Aizer & Dal Bó, 2009; Card & Dahl, 2011; Carrell & Hoekstra, 2012; Iyengar, 2009; Markowitz, 2000; Miller & Segal, 2018; Raissian, 2016; Stevenson & Wolfers, 2006; Tauchen & Witte, 1995).

The remainder of this paper is organized as follows. Section 2 provides background information on nuisance ordinances, as well as a survey of the existing literature on domestic violence reporting to inform potential hypotheses on the relationship between nuisance ordinances and domestic violence. Section 3 outlines my empirical strategy used to identify the effect of nuisance ordinance enactment on domestic violence reporting and incidence. Section 4 describes



my data and presents my results for nuisance ordinances' effect on domestic violence reporting and incidence in California, followed by analyses of nuisance ordinances' impact on several nationwide proxies for domestic violence incidence. Section 5 concludes.

## **2 Background and Literature Review**

### **2.1 Nuisance Ordinances**

Against a backdrop of “tough on crime” politics, police forces across the country began to expand their presence throughout the second half of the twentieth century (Kent & Jacobs, 2005; Mazerolle & Ransley, 2002). To accommodate this growth in police services without imposing too great a burden on local resources, municipalities enlisted other community stakeholders to assist with crime control through third-party policing (Mazerolle & Ransley, 2002). Third-party policing describes police efforts to “persuade or coerce third parties, such as landlords, parents, local governments and other regulators, and business owners to take some responsibility for preventing crime” (Mazerolle & Ransley, 2002, p. 1).

Nuisance ordinances are a common third-party policing tactic (Desmond & Valdez, 2012). Nuisance ordinances are municipality-level policies that sanction landlords if police are frequently called to respond to a landlord's tenants (Fais, 2008). To avoid a fine, landlords must abate tenants whose conduct was considered a nuisance under the municipality's ordinance; most often, this abatement process involves evicting the tenant (Desmond & Valdez, 2012; Greif, 2018). Although the details of nuisance ordinances vary by municipality, nuisance ordinances share several common features. Tenants are regarded as a nuisance when they make a certain number of calls for police service within a certain time period (Fais, 2008). Some ordinances maintain a list of specific offenses that constitute a nuisance activity; common offenses include battery, assault, stalking, sexual violence, weapons violations, drug-related conduct, and general harassment or domestic disturbances. Other ordinances are more vague such that the aforementioned nuisance activities would be implicitly relevant under the ordinance (Fais, 2008). It is important to note that the majority of nuisance ordinances do not distinguish between the offender and victim of a crime, so the consequences of abatement are applied to the entire tenant unit (Kanovsky, 2016). Furthermore, the majority of nuisance ordinances do not exempt domestic violence incidents from

being considered a nuisance; in fact, many nuisance ordinances explicitly include domestic violence, assault, sexual abuse, or battery as nuisance activities (Fais, 2008).

Nuisance ordinances became increasingly common beginning in the 1990s as an effort to combat drug dealing. Yet police departments quickly capitalized on nuisance ordinances to delegate the policing of tenant behavior to landlords (Desmond & Valdez, 2012; Mazerolle & Ransley, 2002; Thacher, 2008). Nuisance ordinances were not the only crime control measures outsourced to residential properties during this period. The Anti-Drug Abuse Act of 1988, reaffirmed by the Cranston-Gonzalez National Affordable Housing Act of 1990, similarly enabled public housing authorities (PHAs) to evict tenants for criminal behavior (Walter, Viglione, & Tillyer, 2017). Motivated by high crime rates afflicting public housing, President Clinton signed the Housing Opportunity Program Extension Act of 1996 to authorize the “one strike and you’re out” initiative (commonly referred to as “One Strike”). One Strike increased the level of discretion awarded to PHAs in controlling their public housing populations by allowing PHAs to screen applicants for prior criminal activity and evict current tenants for criminal activity – regardless of whether the criminal activity occurred on the public housing premises or involved police arrest or conviction (Renzetti, 2001; Walter et al., 2017). Given the prevalence of domestic violence in public housing (Renzetti, 2001), the threat of eviction under One Strike was particularly salient for domestic violence victims. The reauthorization of the Violence Against Women Act (VAWA) in 2005 ultimately addressed domestic violence victims’ vulnerability to the eviction consequences of One Strike by making it unlawful to apply One Strike policies to domestic violence victims in subsidized housing. VAWA did not, however, regulate eviction practices of landlords in private, non-subsidized housing. Thus, most nuisance ordinances still lack protections for domestic violence victims (Fais, 2008).

Police departments and municipal governments endorse nuisance ordinances as a useful incentive tool for holding landlords accountable to their properties while ensuring a secure living environment for tenants. Landlords can also benefit from nuisance ordinances because a safer residential atmosphere helps maintain their rental rates and minimize damage sustained to their property (Fais, 2008; Greif, 2018). Meanwhile, police can focus their attention on higher-priority crimes by downgrading certain disorderly conduct to the status of “nuisance” delegated to landlords (Desmond & Valdez, 2012). Police departments can also expect a reduction in calls for

their services, which further aids in reducing the strain on municipal resources (Fais, 2008; Thacher, 2008).

Despite nuisance ordinances' alleged objective of promoting public safety, nuisance ordinances additionally function as an exclusionary mechanism to deter racial and economic integration. This subterfuge manifests in both the intent and enforcement of nuisance ordinances. For example, after voting to enact a nuisance ordinance in Bedford, Ohio, a city councilor acknowledged the racialized motives driving the ordinance's passage:

“We believe in those middle-class values of neighborhoods where people can go home and their home is their castle and feel safe...We believe in neighborhoods not hoods. We will do everything we can to maintain those quality of life issues...That is one of the reasons we passed that nuisance law tonight...these are predominantly African American kids who bring in that mentality from the inner city...We are trying to stop that.” (*Beverley Somai v. City of Bedford, Ohio: First Amended Complaint*, 2019, p. 8)

Once enacted, nuisance ordinances apply to an entire municipality. Nevertheless, nuisance ordinances are especially enforced against racial minorities; landlords of properties in predominantly Black communities disproportionately receive abatement directives (Desmond & Valdez, 2012). Considering that the expansion of police presence during the second half of the twentieth century was particularly concentrated in cities with higher proportions of Black residents (Kent & Jacobs, 2005), nuisance ordinances emerged as a tactic to police disadvantaged and minority communities (Desmond & Valdez, 2012).

For tenants, the possible consequences of eviction associated with nuisance ordinances are severe. Renters with a court record of eviction often struggle to secure new shelter in the housing market. With years-long waiting lists for affordable housing programs, public housing authorities must be highly selective in granting assistance; evictions can easily result in a rejected application (Desmond, 2012). Because many landlords discriminate against those with eviction histories (Greif, 2018; Kleysteuber, 2007), evicted tenants tend to relocate to more disadvantaged neighborhoods or even enter a period of homelessness (Desmond, 2012). The consequences of eviction are especially dire for domestic violence victims who seldom have access to alternative resources (Fais, 2008). In a qualitative study of the impact of St. Louis's nuisance ordinance on

domestic violence survivors, participants expressed that they stopped calling 911 to avoid the negative repercussions of eviction (Arnold, 2019).

Law enforcement officials and domestic violence advocates hold divergent perspectives on nuisance ordinances' effectiveness at protecting tenants experiencing domestic violence. While law enforcement officials view nuisance ordinances as a disincentive mechanism to deter abusers from perpetrating domestic violence, victim advocates argue nuisance ordinances only exacerbate the risks and barriers to safety that domestic violence survivors already face (Arnold & Slusser, 2015). Law enforcement and advocates also differ on whether to interpret a reduction in calls for police services as an indication that domestic violence has decreased or that victims are now averse to calling for help in fear of eviction (Arnold & Slusser, 2015).

Little research, however, has clarified the impact of nuisance ordinances on innocent tenants – particularly, victims of domestic violence. Desmond and Valdez (2012) contributed the first and only empirical study to evaluate this question. In analyzing every nuisance citation distributed in Milwaukee over a two-year period, they found that nearly a third of all citations were generated by domestic violence; most landlords abated their “nuisance” with eviction. The authors conclude: “The nuisance property ordinance has the effect of forcing abused women to choose between calling the police on their abusers (only to risk eviction) or staying in their apartments (only to risk more abuse)” (Desmond & Valdez, 2012, p. 137). However, their study was limited to the city of Milwaukee. Identifying the causal effect of nuisance ordinances on domestic violence reporting and incidence requires a convincing counterfactual.

## **2.2 Domestic Violence Reporting**

A key policy response to domestic violence is to support victims in reporting their abuse and acquiring the help of legal and social services. Prior research has documented that reporting domestic violence can serve as an effective deterrent of future abuse (Felson, Ackerman, & Gallagher, 2005). In a longitudinal analysis of domestic violence incidents, Felson et al. (2005) found that police intervention – regardless of whether an arrest was ultimately made – reduced the likelihood that domestic violence would be repeated. Children exposed to domestic violence may also benefit from reporting. Because schools are notified once domestic violence is reported in a student's household, school staff can ensure that affected students receive the academic and emotional supports they need to succeed (Carrell & Hoekstra, 2012).

Despite the potential benefits of reporting domestic violence, many victims still choose not to seek the help of law enforcement and remain with their abusers. Scholars have considered a variety of reasons to explain this decision. First, the psychological and emotional aspects of domestic violence can make it difficult for a victim to break their commitment to their partner. Many domestic violence victims develop feelings of helplessness during an abusive relationship, which can cause victims to dismiss reporting mechanisms as futile (Smith, 2000). Even if a victim can perceive some benefit in reporting, the power dynamics of the abusive relationship often impose guilt on a victim that discourages them from leveraging consequences against their abuser (Smith, 2000). Victims may also choose to remain committed to their relationship out of love for their partner and hope that their partner will eventually change their abusive behavior (Aizer & Dal Bó, 2009; Strube & Barbour, 1984).

Second, victims may fear retaliation from their abuser if they report – especially if no legal action is taken against the abuser after the report is filed (Singer, 1988). In response to the lack of enforcement against domestic violence, some states have passed mandatory arrest laws that require police to arrest abusers whenever an incident of domestic violence is reported (Felson et al., 2005; Iyengar, 2009; Lyon, 1999; Smith, 2000). Yet evaluations of mandatory arrest laws find the laws actually discourage victims from reporting out of fear of retribution from their abuser who may return home soon after the arrest and further harm their partner (Iyengar, 2009; Lyon, 1999). In fact, Iyengar (2009) found that the decreased likelihood of reporting domestic violence under mandatory arrest laws resulted in an increase in intimate partner homicides. Other reasons for not reporting abuse or remaining in a violent relationship include financial dependence on a partner, distrust in the police, and the extensive time commitment involved in criminal investigations and court procedures (Aizer & Dal Bó, 2009; Hamilton & Coates, 1993; Smith, 2000; Strube & Barbour, 1984).

### **2.3 Hypotheses on the Relationship Between Ordinances and Violence**

There are several ways in which nuisance ordinances could affect domestic violence. Becker (1968) theorizes a potential offender weighs the costs and benefits of committing a crime before taking action. If the expected utility from committing the crime exceeds the expected utility from not committing the crime, then the offender will commit the crime. Under Becker's theory, the threat of eviction associated with nuisance ordinances increases the cost of engaging in

nuisance activities penalized in a municipality's ordinance – including domestic violence crimes. Numerous studies have documented that abusers are responsive to policies that change the cost of perpetrating domestic violence, lessening their abuse when the costs increase (Markowitz, 2000; Raissian, 2016; Stevenson & Wolfers, 2006; Tauchen & Witte, 1995). Thus, one could expect domestic violence to decrease in response to the increased costs of perpetrating domestic violence that nuisance ordinances impose.

However, nuisance ordinances also make it costlier for domestic violence victims to report their abuse to the police. Most ordinances do not distinguish between perpetrating acts of domestic violence and being the victim of such violence when identifying “nuisance” conduct (Kanovsky, 2016). In this way, abusers are not the only actors influenced by nuisance ordinances; victims, too, must weigh the costs and benefits of reporting their abuse. In addition to the barriers to reporting facing domestic violence victims as outlined in Section 2.2, nuisance ordinances could similarly deter victims from calling the police because they may incur the potential cost of eviction alongside their abuser. Consequently, domestic violence incidence could increase if victims cannot access police services.

Since nuisance ordinances can plausibly impact abusers' likelihood of perpetrating domestic violence as well as victims' likelihood of reporting their abuse, it is important to analyze the effect of nuisance ordinances on both reported domestic violence and the underlying occurrence of domestic violence. Drawing conclusions about the effect of nuisance ordinances on domestic violence from changes in domestic violence reporting alone fails to consider how much of the change is due to an actual change in domestic violence incidence and if any of the change is due to a change in the probability domestic violence is reported. I hypothesize that if a nuisance ordinance is enacted in a municipality, then reports of domestic violence will decrease, while domestic violence incidence will not decrease and may even increase if victims are unable to call for help.

### **3 Empirical Strategy**

Leveraging the variation in nuisance ordinance enactment across municipalities and over time, I specify a difference-in-differences (DD) model comparing domestic violence before and

after a nuisance ordinance is enacted in municipalities with a nuisance ordinance relative to municipalities without a nuisance ordinance:

$$domestic\ violence_{it} = \beta_0 + \beta_1 post\ ordinance_{it} + \alpha_i + \gamma_t + \varepsilon_{it}$$

where  $domestic\ violence_{it}$  is a measure of domestic violence reporting or incidence in municipality  $i$  at year  $t$ , and  $post\ ordinance_{it}$  is an indicator that the measure of domestic violence was recorded after a nuisance ordinance was enacted in the municipality.  $\alpha_i$  and  $\gamma_t$  represent municipality and year fixed effects, respectively. Municipality fixed effects control for any variation in the prevalence of domestic violence driven by inherent differences in municipality characteristics. Year fixed effects control for broad changes in domestic violence that have occurred over time.

The coefficient  $\beta_1$  estimates differential changes in domestic violence in municipalities with nuisance ordinances after those ordinances were enacted relative to changes in domestic violence in municipalities without nuisance ordinances over the same time period. Comparing the sign of  $\beta_1$  when  $domestic\ violence_{it}$  is a measure of reported domestic violence versus when  $domestic\ violence_{it}$  is a measure of domestic violence incidence informs the extent of nuisance ordinances' impact. For example, if  $\beta_1$  is consistently negative both when  $domestic\ violence_{it}$  is a measure of reported domestic violence and when  $domestic\ violence_{it}$  is a measure of domestic violence incidence, then this would suggest that the decrease in reported domestic violence reflects nuisance ordinances discouraging the incidence of domestic violence – consistent with the intended goal of nuisance ordinances as a crime prevention policy. Alternatively, if  $\beta_1$  is negative when  $domestic\ violence_{it}$  is a measure of reported domestic violence but positive when  $domestic\ violence_{it}$  is a measure of domestic violence incidence, then this would indicate that nuisance ordinances only impact reporting behavior and not the underlying occurrence of the crime itself.

In order to interpret  $\beta_1$  as the causal effect of nuisance ordinances on domestic violence, I assume trends in domestic violence would have been parallel in municipalities with and without nuisance ordinances in the absence of an ordinance being enacted. The inclusion of municipality fixed effects in the DD model captures time-invariant differences in domestic violence trends between these municipalities. To strengthen the validity of the parallel trends assumption, I add

county-specific linear trends as well as other time-varying control variables to account for any time-varying differences that could bias my results.

An event study analysis further evaluates potential bias from differential trends. This estimation takes the form:

$$\begin{aligned}
 & \textit{domestic violence}_{it} \\
 & = \beta_0 + \beta_1 5 \textit{ or more years pre ordinance}_{it} + \beta_2 4 \textit{ years pre ordinance}_{it} \dots \\
 & + \beta_5 1 \textit{ year pre ordinance}_{it} + \beta_6 1 \textit{ year post ordinance}_{it} \\
 & + \beta_7 2 \textit{ years post ordinance}_{it} \dots + \beta_{10} 5 \textit{ or more years post ordinance}_{it} \\
 & + \alpha_i + \gamma_t + \varepsilon_{it}
 \end{aligned}$$

I generate dummy variables for each number of years since a nuisance ordinance was enacted in a municipality. These coefficients estimate differences in domestic violence between municipalities with a nuisance ordinance and municipalities without a nuisance ordinance at varying numbers of years since an ordinance’s enactment. Zero years since nuisance ordinance enactment is the omitted time period. For the event study results to support the parallel trends assumption, the pre-ordinance coefficients should not be statistically different from zero or exhibit a particular pattern. If the nuisance ordinances have an effect on domestic violence, then the post-ordinance coefficients should also be statistically significant. An additional benefit of this specification is the flexibility in estimating the impact of the ordinances, allowing the effect to be growing over time, decreasing over time, or even non-monotonic.

## 4 Data and Results

My dataset of nuisance ordinances consists of ordinances in 83 U.S. municipalities, including information on the date an ordinance was enacted and whether domestic violence is explicitly exempt or included in the ordinance’s list of nuisance activities. Nuisance ordinances in the dataset span 29 states and range in year of enactment from 1962 to 2017. I compiled data on these ordinances from the Temple University Policy Surveillance Program’s “City Nuisance Property Ordinances” database (2017), which tracked nuisance ordinances across the 40 most populous U.S. cities, the online supplement to Desmond and Valdez (2012), which additionally recorded nuisance ordinances outside the most populous cities through a systematic Internet search, and Fais’s (2008) law review article on nuisance ordinances. I assume municipalities that



are not included in any of these sources have never enacted nuisance ordinances. However, it is possible that there are municipalities with nuisance ordinances unrecorded in these sources. To the extent that any municipalities' nuisance ordinances were missed, my estimates will be conservatively biased against finding significant effects of ordinance enactment on domestic violence between municipalities with and without nuisance ordinances. Table 1 summarizes the location and characteristics of the nuisance ordinances in my dataset.<sup>2</sup>

## **4.1 California Analysis**

I rely on California for my analysis of reported domestic violence because of data availability.<sup>3</sup> To track domestic violence reporting in California, I utilize 911 call data for domestic violence-related incidents received by municipal law enforcement agencies in California. For domestic violence incidence in California, I use the California Women's Health Survey, which contains survey questions about women's experiences with domestic violence as well as municipality identifiers.

### **4.1.1 Effect on Reported Domestic Violence in California**

#### *Data*

To measure reported domestic violence in California, I use the Domestic Violence-Related Calls for Assistance dataset from the California Department of Justice. The data provides the total number of domestic violence-related calls for assistance reported by 809 law enforcement agencies on a monthly basis between the years 1995 and 2016. Because nuisance ordinances are primarily enacted through municipal governments, I restrict my analyses to the 473 municipality-level agencies in the data; highway and transit authorities, university police departments, county sheriffs, and any other non-municipality-level agencies were excluded from my analyses. Seven municipalities in California have enacted nuisance ordinances: San Jose (1998), San Diego (2007),

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<sup>2</sup> See Appendix Table A1 for a full list of the 83 nuisance ordinances in my dataset by state, municipality, and year of enactment.

<sup>3</sup> The FBI's National Incident Based Reporting System (NIBRS) of the Uniform Crime Reports (UCR) program, which compiles reporting data on crimes (including the victim-offender relationship) from law enforcement agencies, is an available measure of reported domestic violence on a national scale. However, agency participation in the UCR is voluntary; few agencies in municipalities with nuisance ordinances were represented in the NIBRS.

Los Angeles (2008), Long Beach (2011), Fresno (2016), Sacramento (2016), and San Francisco (2016).<sup>4</sup>

Table 2 compares the average monthly domestic violence-related calls for assistance in municipalities with ordinances versus municipalities without ordinances in the first and last year of the data. Average calls fell overall by 2016, by which point all nuisance ordinances in California had gone into effect. This decline is especially evident in municipalities with ordinances; average calls decreased by 48.21 percent between 1995 and 2016, whereas municipalities without ordinances only experienced a 20.24 percent reduction in average calls over the same period. The differences in average calls between municipalities with ordinances and municipalities without ordinances – even in the earliest year of data before any ordinances were enacted – are large and statistically significant, suggesting that municipalities with ordinances differ from municipalities without ordinances. To the extent that these differences are time-invariant, they will be captured in the municipality fixed effects included in my model and will not bias my results. The inclusion of county-specific linear trends or county-year interaction fixed effects helps eliminate bias from differences across these areas that might be time-varying.<sup>5</sup> To capture time-varying trends at the municipality-level, I also control for total homicides in a municipality as a proxy for a municipality’s susceptibility to violent crime in addition to whether a municipality enacted a no-drop policy, which could affect rates of domestic violence (Aizer & Dal Bó, 2009).<sup>6</sup>

## *Results*

Table 3 estimates the effect of nuisance ordinance enactment on domestic violence-related calls for assistance in a municipality. All regressions include both municipality and call date

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<sup>4</sup> Note that because Fresno, Sacramento, and San Francisco enacted nuisance ordinances in the final year of available data (2016), the post-ordinance period for these municipalities only includes a few months of data.

<sup>5</sup> Nuisance ordinances are municipality-level policies; however, some counties include multiple municipalities where nuisance ordinances have been enacted. Of municipalities with nuisance ordinances in California, Los Angeles and Long Beach are located in the same county (Los Angeles County). Thus, specifications with county-year interaction fixed effects are estimated only from variation in same-county nuisance ordinances in Los Angeles and Long Beach.

<sup>6</sup> Data on homicides is from the California Department of Justice’s Crimes and Clearances dataset, which provides the total number of offenses and clearances for various crimes reported annually by municipal law enforcement agencies as part of the Federal Bureau of Investigation’s Uniform Crime Reporting program between the years 1985 and 2017. Data on no-drop policies is from Aizer & Dal Bó (2009). Similar to state-level mandatory arrest policies as discussed in Section 2.2, municipalities enact no-drop policies to compel prosecutors to continue with the prosecution of domestic violence cases even if the victim wishes to drop charges.

(month-year) fixed effects. I use the log of total calls to estimate a proportional effect, which also allows for comparability across other specifications.<sup>7</sup> The coefficient on “post-ordinance” – an indicator variable that equals one if the total calls in a given month were recorded after a nuisance ordinance had been enacted in that municipality – is -0.232, as shown in the main specification in column 1. This means that municipalities with nuisance ordinances experienced a 23.2 percent reduction in domestic violence-related calls for assistance after their ordinance was enacted relative to municipalities without nuisance ordinances. The estimate is statistically significant and economically significant in magnitude (nearly one fifth of a standard deviation in the log of total calls for municipality-level agencies). I add county-specific linear trends, county-year interaction fixed effects, and municipality-level controls in subsequent columns to further control for any time-varying differences in pre-existing domestic violence reporting trends between municipalities. Although slightly smaller in magnitude, the estimated effect of ordinance enactment on reported domestic violence remains negative and statistically significant.

To clarify whether this decrease in reported domestic violence can actually be attributed to a decline in the underlying occurrence of domestic violence or if the decrease merely reflects the impact of nuisance ordinances on reporting behavior, I next explore the effect of nuisance ordinances on domestic violence incidence.

#### **4.1.2 Effect on Domestic Violence Incidence in California**

##### *Data*

The California Women’s Health Survey (CWHS) provides a measure of domestic violence incidence in California. The CWHS is an annual telephone survey conducted between 1997 and 2012 by the California Department of Healthcare Services and Department of Public Health. Each year, the survey collects information on a variety of demographic characteristics and health indicators – including respondents’ experiences with domestic violence – from a random sample

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<sup>7</sup> The results are robust to a linear specification; see Appendix Table A2. Municipalities that received zero calls in a given month (40.05 percent of observations) were excluded when generating the log of total calls. Given the larger population sizes of cities with ordinances, the exclusion of these smaller municipalities that received zero calls from the control group likely improves the balance between municipalities with and without ordinances. Appendix Table A3 reports the regressions from Table 3 using the log of total domestic violence-related calls plus one in order to avoid dropping municipalities that received zero calls in a given month. These results are slightly larger in magnitude but consistent with the results in Table 3.

of approximately 4,000 adult women. Although the majority of years of the CWHS include questions on respondents' experiences with domestic violence, the particular survey questions vary by year.<sup>8</sup> Therefore, I generated an indicator variable across all survey years that equals one when a respondent answers affirmatively to any of the questions in that survey year related to physical or sexual abuse by an intimate partner.<sup>9</sup> This indicator is the dependent variable in my main results for this section. The dataset includes survey respondents from 84 major municipalities in California, including four municipalities – San Jose (1998), San Diego (2007), Los Angeles (2008), and Long Beach (2011) – that enacted nuisance ordinances during the dataset's time period. The data also includes respondents whose municipality was redacted due to privacy concerns; all municipalities that span less than five zip codes were redacted to limit the ability to identify individual respondents.<sup>10</sup>

Table 4 provides summary statistics for the survey data, including variable comparisons between municipalities that enacted nuisance ordinances and those that did not. P-values of difference show that respondents' experiences with domestic violence in municipalities with ordinances were not significantly different from those of respondents in municipalities without ordinances, supporting my identifying assumption. To address potential demographic differences between the two groups of municipalities, I demonstrate my results are robust to the inclusion of demographic controls.

## *Results*

Panel A of Table 5 estimates the effect of nuisance ordinance enactment on the probability a respondent experienced physical or sexual abuse by her partner in a municipality. All regressions include municipality and year fixed effects. Because municipality was redacted for municipalities with fewer than five zip codes, I also include county fixed effects to further control for differences in pre-existing domestic violence trends between municipalities since these redacted

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<sup>8</sup> No domestic violence-related questions were asked in 1997-1999, 2002, or 2010. Thus, these survey years are not considered in my analyses.

<sup>9</sup> The questions included in this indicator variable ask whether in the past 12 months, the respondent's partner beat them up, choked them, pushed them, slapped them, kicked or hit them (or threatened to do so), tried to hit them with an object or threw an object at them, used a knife or gun on them (or threatened to do so), or forced sex.

<sup>10</sup> Respondents in redacted municipalities represent 50.04 percent of respondents across all years in the dataset; no municipalities with ordinances were redacted.

municipalities span different counties. The coefficient on “post-ordinance” is 0.007, as shown in the main specification in column 1. This means that the probability a respondent experienced domestic violence increased by 0.7 percentage points after a nuisance ordinance was enacted in the respondent’s municipality relative to respondents in municipalities without nuisance ordinances. The estimate is statistically significant and robust to the addition of county-specific linear trends, county-year interaction fixed effects, and municipality-level controls in subsequent columns. The estimate in the column 6 specification with both county-year fixed effects and municipality controls is smaller in magnitude and lacks statistical significance but remains positive in sign.<sup>11</sup> Overall, these results are supportive of an increase in domestic violence incidence in municipalities with nuisance ordinances following ordinance enactment.

Panel B estimates the effect of nuisance ordinance enactment on the probability a respondent sought help from the police for an incident of domestic violence. Because this survey question was only asked between 2007 and 2009, the result is estimated off of temporal variation in Los Angeles where a nuisance ordinance was enacted in 2008; all other nuisance ordinances in California were enacted outside of the 2007-2009 time period. Nevertheless, respondents were 8.6 percentage points less likely to request police services for a domestic violence incident after the enactment of a nuisance ordinance according to the main specification in column 1. This estimate is robust to the addition of county-specific linear trends, county-year fixed effects, and municipality-level controls in subsequent columns, and grows to -11.9 percentage points with the addition of county trends in column 3 and with the addition of both county-year fixed effects and municipality controls in column 6. All results are statistically significant at the 95 percent confidence level and support the hypothesis that nuisance ordinances discourage domestic violence reporting, thus allowing the incidence of domestic violence to persist.

### **4.1.3 Addressing Threats to Identification**

#### *Parallel Trends Assumption*

The identifying assumption necessary to infer a causal effect of nuisance ordinances on domestic violence depends on parallel trends in domestic violence across municipalities with and

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<sup>11</sup> Recall that specifications including county-year interaction fixed effects are only driven by variation in nuisance ordinances enacted in municipalities within the same county (Long Beach and Los Angeles of Los Angeles County) as discussed in Footnote 5.

without nuisance ordinances in the absence of an ordinance being enacted. If rates of domestic violence reporting or incidence in municipalities with nuisance ordinances are evolving on different trends than municipalities without nuisance ordinances, then this would violate the parallel trends assumption and suggest that my estimates do not identify a causal effect of nuisance ordinances on domestic violence. In order to bias my results, these differential trends for domestic violence reporting and incidence would need to evolve in opposite directions; differential trends would need to simultaneously explain a decrease in domestic violence reporting and an increase in domestic violence incidence. Note that the results presented in the previous two sections are largely robust to the inclusion of county-specific linear trends, county-year interaction fixed effects, and municipality-level controls. Nevertheless, I further investigate potential bias from differential trends through event study analyses.

Figure 1 plots estimates from an event study for the effect of nuisance ordinance enactment on reported domestic violence, as measured by the log of total domestic violence-related calls for assistance in a municipality. I add both municipality and call date (month-year) fixed effects.<sup>12</sup> As explained in Section 3, the event study decomposes the effect of nuisance ordinance enactment on a year-by-year basis to observe the resulting trends in domestic violence over time. Each dot on the figure represents a coefficient estimate of the difference between domestic violence in that year relative to the year of ordinance enactment. The coefficients are centered around the year of enactment (zero years since ordinance enactment), which is the omitted time period. Following the drop in reported domestic violence at five or more years pre-ordinance, the pre-ordinance coefficients do not appear in a particular pattern in the years immediately preceding ordinance enactment. Except the coefficient on five or more years pre-ordinance, the pre-ordinance coefficients are not individually statistically significant.<sup>13</sup> Although an F-test of the joint significance of all pre-ordinance coefficients produces a statistically significant p-value, the coefficients up to four years pre-ordinance are not jointly significant when the five or more years

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<sup>12</sup> See Appendix Table A4 (column 1) for the full results of the event study that correspond with the plotted estimates in Figure 1. I add county-specific linear trends, county-year interaction fixed effects, and municipality-level controls in subsequent columns.

<sup>13</sup> To assess whether the results – particularly, the estimates on the five or more years pre-ordinance coefficient – are driven by one city’s ordinance, I estimated four separate regressions (following the main specification presented in Figure 1), dropping one treatment city from each. This did not significantly change my results.

pre-ordinance coefficient is excluded from the F-test.<sup>14</sup> These observations all lend credibility to the parallel trends assumption. An F-test of the joint significance of the post-ordinance coefficients produces a statistically significant p-value at the 99 percent confidence level and the post-ordinance coefficients form a downward trend, further supporting my finding that nuisance ordinance enactment decreased domestic violence reporting.

Figure 2 plots estimates from an event study for the effect of nuisance ordinance enactment on domestic violence incidence, as measured by the probability a respondent of the California Women’s Health Survey experienced physical or sexual abuse by her partner. I include municipality (when available), county, and year fixed effects as described in Section 4.1.2.<sup>15</sup> While estimates for the pre-ordinance coefficients are not consistently significant individually, an F-test of the joint significance of all pre-ordinance coefficients produces a statistically significant p-value. Nevertheless, the pre-ordinance coefficients do not exhibit a particular trend; if anything, domestic violence incidence appears to have been decreasing in the years immediately preceding ordinance enactment. By contrast, an F-test of the joint significance of all post-ordinance coefficients produces a statistically significant p-value at the 99 percent confidence level and the post-ordinance coefficients exhibit a clear upward trend, confirming my results that nuisance ordinance enactment increased domestic violence incidence.

### *Endogeneity of Ordinance Enactment*

A related threat to identifying the causal effect of nuisance ordinances concerns whether municipalities’ enactment of nuisance ordinances was endogenous. It is plausible that a municipality chooses to adopt a nuisance ordinance in response to crime waves in the municipality. If such an endogeneity problem exists, then differences in domestic violence reporting or incidence may merely reflect these crime waves – including fluctuations in the prevalence of domestic violence – rather than the effect of the ordinances. Furthermore, the endogenous enactment of nuisance ordinances during a period of rising crime unique to a municipality would likely bias the

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<sup>14</sup> An F-test of the joint significance of the coefficients for pre-1 year, pre-2 years, pre-3 years, and pre-4 years produces a p-value of 0.203 for the regression specified in column 1, 0.286 for column 2, 0.721 for column 3, 0.309 for column 4, 0.341 for column 5, and 0.798 for column 6.

<sup>15</sup> See Appendix Table A5 (column 1) for the full results of the event study that correspond with the plotted estimates in Figure 2. I add county-specific linear trends, county-year interaction fixed effects, and municipality-level controls in subsequent columns.

estimated effect of the ordinances. An ordinance effect that appears to reduce reported crime could simply represent crime returning from a heightened state to prior levels. Thus, the endogenous enactment of nuisance ordinances in response to a municipality's spike in crime could cause the ordinances to appear overly impactful in reducing reported crimes such as domestic violence.

It is worth noting that the event studies presented above did not demonstrate a spike in domestic violence reporting or incidence prior to ordinance enactment. Since nuisance ordinances address criminal activity more broadly, Figure 3 plots estimates from an event study for other reported crimes in California.<sup>16</sup> I include both municipality and year fixed effects.<sup>17</sup> The majority of the pre-ordinance coefficients are not statistically significant; an F-test of the joint significance of all pre-ordinance coefficients does not yield a statistically significant p-value.<sup>18</sup> Overall, these results suggest that nuisance ordinances were not enacted in response to sharp increases in a municipality's crime levels, thereby easing the threat of endogenous ordinance enactment. In fact, other reported crimes appear to form a downward trend following ordinance enactment, perhaps suggestive of nuisance ordinances' claimed objective of crime deterrence. Thus, my finding of increased domestic violence incidence in municipalities with nuisance ordinances following ordinance enactment as described in Section 4.1.2 is particularly compelling in contrast to this decline in other reported crimes. Nevertheless, evaluating an adverse effect of increased domestic violence incidence alongside a favorable reduction in other reported crimes complicates welfare calculations for the overall impact of nuisance ordinances; I leave this to future work.

Considering nuisance ordinances' purported goal as a crime prevention effort, it is also plausible that municipalities enacted nuisance ordinances as part of a broader "tough on crime"

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<sup>16</sup> To measure other reported crimes in California, I use the Crimes and Clearances dataset from the California Department of Justice as described in Footnote 6. "Reported crimes" is defined as the sum of homicide, rape, robbery, and aggravated assault, burglary, larceny-theft, motor vehicle theft, and arson.

<sup>17</sup> See Appendix Table A6 (column 1) for the full results of the event study that correspond with the plotted estimates in Figure 3. I add county-specific linear trends in column 2 and county-year interaction fixed effects in column 3.

<sup>18</sup> An F-test of the joint significance of all pre-ordinance coefficients when county-year interaction fixed effects are included in the event study regression specification (column 3 of Appendix Table A6) does produce a statistically significant p-value (0.000). However, specifications including county-year interaction fixed effects are only driven by variation in nuisance ordinances enacted in municipalities within the same county (Long Beach and Los Angeles of Los Angeles County) as discussed in Footnote 5. Although not all individually significant, the pre-ordinance coefficients in this specification are negative in sign in the years immediately preceding ordinance enactment. Thus, even if reported crime was indeed trending downward prior to ordinance enactment, these municipalities do not appear to have enacted ordinances in response to a notable rise in crime.



policy agenda. If so, the effect of nuisance ordinances could be confounded by the effects of other similarly-timed policies that also impact domestic violence or other crimes.<sup>19</sup> I am less concerned with this possibility, however, because most policies during this period were enacted at the federal- or state-level; my analyses of nuisance ordinances focused exclusively on California municipalities would only be biased if other municipality-level crime policies coincided with California municipalities' nuisance ordinance enactment. That my results are robust to controlling for municipality-level no-drop policies provides further evidence against this concern.

### *Resident Mobility*

Local population changes following nuisance ordinance enactment could confound my results if there is a substitution of residents who are more likely to experience domestic violence but less likely to report domestic violence. To test this possibility, I first re-estimate my results from Table 5 – the effect of nuisance ordinance enactment on the probability a CWHS respondent experienced domestic violence and the probability a respondent sought help from the police for an incident of domestic violence – but also include controls for demographic characteristics. The demographic controls inform whether particular demographic groups experience or report domestic violence differently. In addition, I utilize migration data from the American Community Survey to test whether any of these demographic groups that were more likely to experience but less likely to report domestic violence were also more likely to move into municipalities with nuisance ordinances in California following nuisance ordinance enactment.<sup>20</sup>

Table 6 presents these results. Columns 1 and 2 evaluate whether certain demographic groups are more likely to experience domestic violence but less likely to report domestic violence, respectively. These regressions include municipality, county, and year fixed effects. Note that the estimates on the main “post-ordinance” coefficient are consistent with my initial estimates from Table 5, even after demographic controls are added to this version of the regression specification.

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<sup>19</sup> Previous studies evaluating the effects of various policy changes on domestic violence have accounted for the timing of other domestic violence-related policies such as the Family Violence Prevention Services Act, unilateral divorce laws, and mandatory arrest laws (Aizer & Dal Bó, 2009; Iyengar, 2009; Miller & Segal, 2018; Stevenson & Wolfers, 2006).

<sup>20</sup> I use data from the American Community Survey (ACS) of the U.S. Census between the years 1990 and 2017. The ACS defines migration as changing residence (either moving within state, moving between states, or abroad) since a reference point of one year ago. I limit my ACS sample to adult females in California to match the sample of the CWHS.

The probability a respondent experienced domestic violence increased after a nuisance ordinance was enacted in the respondent's municipality, while the probability a respondent requested police services for a domestic violence incident decreased following ordinance enactment. Although not consistently significant, Black respondents, Asian/Pacific Islander respondents, and respondents of other races are more likely to experience domestic violence but less likely to seek help for the abuse from the police relative to White respondents.

Column 3 estimates the effect of nuisance ordinance enactment on migration for females in California, including interaction variables for demographic characteristics to inform whether ordinance enactment has a differential effect on migration for respondents of certain demographic groups. The regression includes municipality and year fixed effects. The coefficient estimates for the post-ordinance interaction terms with demographic groups that are more likely to experience domestic violence but less likely to report domestic violence to the police – Black and Asian/Pacific Islander respondents as well as respondents of other races – are all negative in sign, meaning female respondents of these demographic groups are less likely to have recently moved into municipalities with nuisance ordinances in California after an ordinance was enacted. This suggests that my findings of decreased domestic violence reporting and increased domestic violence incidence following nuisance ordinance enactment are not biased by a substitution of residents who are more likely to experience domestic violence but less likely to report their abuse. It is worth noting that Medi-Cal recipients (which I use as a proxy for low-income status) are also significantly less likely to have recently moved into municipalities with nuisance ordinances following ordinance enactment. That respondents of racial minorities and of low-income status have a lower propensity to move-in in response to ordinance enactment hints to the ulterior motive of nuisance ordinances as an exclusionary mechanism, as discussed in Section 2.1.

## **4.2 Nationwide Proxies for Domestic Violence Incidence**

In this section, I expand my analysis of the effect of nuisance ordinances on domestic violence incidence to a national scale. Nationwide data on domestic violence incidence with geographic identifiers smaller than state is difficult to find.<sup>21</sup> Here, I utilize two proxies for

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<sup>21</sup> The FBI's Supplementary Homicide Reports (SHR) provides a nationwide database of homicides at the agency-level, including information on the victim-offender relationship necessary to identify incidents of intimate partner homicide, as part of the Uniform Crime Reports (UCR) program. Yet as noted in Footnote 3, agency participation in

domestic violence incidence: county-level vital statistics data on female mortality from assault, and Google Trends data to assess metro-level changes in the prevalence of online search activity related to domestic violence.

#### **4.2.1 Female Mortality from Assault**

##### *Data*

Female mortality from assault can serve as a proxy for intimate partner homicide, which may provide insight into the impact of nuisance ordinances on domestic violence incidence. This is an imperfect but advantageous measure of domestic violence incidence for several reasons. First, mortality data avoids reporting bias because, unlike survey data, it does not require that women admit they experienced abuse. Second, although women are not the only gender to experience domestic violence, an estimated 76 percent of women who are assaulted are assaulted by an intimate partner (Tjaden & Thoennes, 2000). Thus, female mortality from assault is a reasonable proxy for intimate partner homicide even if the victim-offender relationship is uncertain. However, as noted above, mortality is an extreme outcome of domestic violence. It is possible that nuisance ordinances affect domestic violence incidence, but not at this margin.

Data on female mortality from assault comes from the National Vital Statistics System of the Centers for Disease Control and Prevention. The data is at the incident-level and includes information on the cause of death, place of death, and demographic characteristics of the deceased as well as county identifiers from 1990 to 2016. I consider a death as an intimate partner homicide if the cause of death was assault, the victim was female, and the place of death was at home. I additionally restrict the data to ages 20-54, the age group for which domestic violence is most prevalent (Aizer & Dal Bó, 2009). I aggregated these incidents of intimate partner homicide in each county for each year of data. Nuisance ordinances were enacted in municipalities within 60 counties during the dataset's time period. Note that nuisance ordinances are enacted at the municipality-level, but the mortality data is aggregated to the county-level, which introduces

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the UCR is voluntary; few agencies in municipalities with nuisance ordinances are represented in the SHR. An ideal data source for a nationwide analysis of domestic violence incidence would be the National Crime Victimization Survey (NCVS), which gathers detailed data on domestic violence incidence – even when not reported to the police. However, matching observations with municipality-level ordinances is impossible without difficult-to-obtain restricted use approvals.

measurement error in my independent variable of interest.<sup>22</sup> I also aggregated incidents of male deaths by assault to include as a control.

Table 7 compares the average totals of female deaths by assault at home in municipalities with nuisance ordinances versus municipalities without nuisance ordinances in the first and last years of the data. Although average levels of female mortality from assault increased overall by 2016, this increase was more pronounced for municipalities without ordinances. The differences in female mortality between municipalities with and without ordinances are also statistically significant, which could bias my results. I add state-specific linear trends, state-year interaction fixed effects, and county-level controls to capture any time-varying differences at the county- and state-levels, in addition to county and year fixed effects.

### *Results*

Table 8 estimates the effect of nuisance ordinance enactment on female deaths by assault that occurred in the home. All regressions include both county and year fixed effects. I add state-specific linear trends, state-year interaction fixed effects, and county-level controls in subsequent columns to further control for potential differential trends in domestic violence incidence between counties. Although the estimates are consistently positive in sign, I do not observe a significant effect of nuisance ordinance enactment on domestic violence incidence as measured by female mortality from assault.<sup>23</sup> However, I hesitate to draw conclusions from these results. First, as noted above, I applied municipality-level nuisance ordinance data to county-level mortality data. Yet

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<sup>22</sup> Phoenix (1962) and Mesa (2009) both enacted ordinances within Maricopa County; however, the date of ordinance enactment in Phoenix is outside the time scope of the mortality data. Thus, Mesa (2009) represents Maricopa County, AZ. Elm Grove Village (2008) and Hartland Village (2008) jointly represent Waukesha County, WI; Madison (2008) and Middleton (2008) jointly represent Dane County, WI. The following ordinances were excluded from the mortality analyses because multiple municipalities within the same county enacted ordinances in different years: Long Beach (2011) and Los Angeles (2008) in Los Angeles County, CA; Mundelein (2004) and Village of Wauconda (2008) in Lake County, IL; Village of Addison (2009) and West Chicago City (2008) in DuPage County, IL; Beaverton (1998) and Tigard (2003) in Washington County, OR; Hazleton (2009) and Wilkes-Barre (2005) in Luzerne County, PA; Menasha (2008) and Oshkosh (2010) in Winnebago County.

<sup>23</sup> As a placebo, Appendix Table A7 estimates the effect of ordinance enactment on male deaths by assault outside the home. Because these deaths occurred away from home, they would not have been influenced by a nuisance ordinance policy. Indeed, the estimates are not statistically significant, indicating male assaults outside the home did not change following ordinance enactment. Though not significant overall, that the estimates on female mortality at home (positive) and male mortality from assault away from home (negative) are opposite in sign suggests any effect of ordinance enactment on intimate partner homicide was not confounded by broader trends in declining crime.

changes in domestic violence incidence in a municipality that enacted a nuisance ordinance may not represent changes in domestic violence in other municipalities within the same county. Thus, one municipality's ordinance effect on domestic violence aggregated to the county-level may not accurately reflect changes in domestic violence for all municipalities in the county. Second, mortality is an extreme outcome of domestic violence. That nuisance ordinances do not appear to wield a significant influence over intimate partner homicides does not necessarily imply that nuisance ordinances did not impact less severe forms of abuse, such as those asked about in the California Women's Health Survey.

#### **4.2.2 Google Trends**

##### *Data*

Measuring changes in online search activity related to domestic violence serves as a less extreme proxy for domestic violence incidence on a national scale. Web search data can provide insight on domestic violence incidence in several ways. Although many victims of domestic violence choose not to report their abuse as discussed in Section 2.2, victims may still search for information on available resources. Family members, friends, or neighbors suspicious of abuse might also look for information or resources online. In both scenarios, web search data could account for cases of domestic violence that were otherwise unreported.

To analyze online search activity related to domestic violence, I use data from Google Trends. Google Trends records the number of annual searches for a given search term as a fraction of the total number of searches conducted that year in a Designated Market Area (DMA), a geographic unit similar to a metropolitan area. These search rates comprise an indexed value for the frequency at which Google users in a particular DMA searched for the term relative to users in other DMAs that year. Due to the high volume of searches conducted on Google, Google Trends calculates search indices by sampling from its underlying database each day.<sup>24</sup> Because Google

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<sup>24</sup> To reduce sampling variability, I repeated the process of downloading the Google Trends data for five days. Search index values in my dataset represent the averaged index values across the five days. Only DMA-years present in all five days of data were included in the averages; 87 DMA-year observations were dropped from the "domestic violence" search topic, 378 DMA-year observations were dropped from the "women's shelter" search topic, 195 DMA-year observations were dropped from the "eviction" search topic, and 45 DMA-year observations were dropped from the "gender" search topic because that DMA-year was not present in all five days of data. Appendix Table A8 reports the regressions from Table 9 using the full dataset of DMA-years to calculate the averaged search index values

Trends presents the search data as indexed values, my results relying on Google Trends should be interpreted as changes in the relative direction of search activity rather than changes in the absolute number of searches for a particular search term. Similar search index values for different search terms do not necessarily reflect similar quantities in the absolute number of searches for those search terms; a DMA with a high search index value simply means that searches for the given search term comprised a substantial proportion of the total searches conducted in the DMA relative to other DMAs that year. Thus, my analysis is limited to evaluating whether nuisance ordinance enactment is associated with an increase or decrease in searches for topics related to domestic violence.

My dataset from Google Trends includes 210 DMAs' search index values between 2006 to 2018 for four search topics: "domestic violence", "women's shelter", "eviction", and "gender".<sup>25</sup> Nuisance ordinances were enacted in municipalities within 35 DMAs during the dataset's time period.<sup>26</sup> Given the increase in domestic violence incidence following nuisance ordinance enactment in California as reported in Section 4.1.2, I would expect ordinance enactment to be associated with an increase in searches within the "domestic violence" topic. Similarly, searches related to "women's shelter" and "eviction" may also increase if more women wish to flee abusive relationships, or if more women face homelessness or eviction as a result of the ordinances. I include "gender" as a placebo search topic that could trend similarly to search topics related to domestic violence. It is plausible that changes in search activity related to domestic violence could be driven by broader changes in Google users' interest in issues affecting women rather than actual changes in domestic violence incidence. As a placebo, the relative frequency of searches related to "gender" should not change following ordinance enactment.

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without dropping any DMA-years that were present in less than five days of data. These results are consistent with the results in Table 9.

<sup>25</sup> Google Trends groups related search terms into search topics. For example, the search topic "women's shelter" encompasses search terms such as "shelters for women and kids", "domestic violence shelter near me", and names of specific emergency shelters for women experiencing homelessness.

<sup>26</sup> Some DMAs span multiple municipalities that have enacted ordinances; however, no DMA spanned multiple municipalities in which multiple ordinances were enacted within the time scope of the Google Trends data. Thus, Fort Worth (2014) represents the Dallas (1975)-Fort Worth, TX DMA; San Francisco (2016) represents the San Francisco-Oakland-San Jose (1998), CA DMA. Note that applying municipality-level ordinance data to DMA-level Google Trends data could introduce slight measurement error in my independent variable of interest.

## *Results*

Table 9 estimates the effect of nuisance ordinance enactment on changes in the relative frequency of searches for each of the four search topics. All regressions include DMA and year fixed effects. The results indicate that nuisance ordinance enactment is associated with a statistically significant increase in searches for “women’s shelter” (Panel B) and “eviction” (Panel C); these estimates are robust to the addition of state-specific linear trends in column 2 and state-year interaction fixed effects in column 3. Searches for “domestic violence” (Panel A) also increased following ordinance enactment, but the results were not statistically significant. The results on the placebo search topic “gender” (Panel D) were neither statistically significant nor consistent in sign, validating that the results for “women’s shelter” and “eviction” were not necessarily confounded by trends in broader online search interest in gender topics. Overall, these patterns in search activity suggest domestic violence incidence nationwide did not decrease and may have increased in metropolitan areas with nuisance ordinances following ordinance enactment.

## **5 Conclusion**

This paper contributes the first econometric analysis of the effect of nuisance ordinances on domestic violence. Many municipalities have turned to nuisance ordinances as a strategy to delegate crime control efforts to landlords at residential properties. Although nuisance ordinances appear to reduce other reported crimes, my results suggest that nuisance ordinances additionally have the unintended consequence of discouraging domestic violence victims from reporting their abuse, thereby allowing the incidence of domestic violence to persist. Using a difference-in-differences strategy, I find that nuisance ordinance enactment leads to a 16.5-23.2 percent reduction in domestic violence-related 911 calls for assistance and a 0.4-0.7 percentage point increase in self-reported domestic violence incidence in California. Nationwide, I also find nuisance ordinance enactment is associated with a statistically significant increase in online search activity related to domestic violence as a proxy for domestic violence incidence.

While this paper leverages several proxies to measure domestic violence, my analyses remain constrained by data availability on domestic violence. Considering survey evidence that suggests domestic violence offenses are underreported, it is difficult to disentangle changes in

domestic violence reporting from changes in underlying behavior that can result from interventions intending to reduce the incidence of domestic violence. In future work, I hope to collect additional crime data sources that include information on the victim-offender relationship in order to more accurately identify incidents of domestic violence. I also hope to explore the impact of nuisance ordinances on other outcomes, such as eviction or homelessness, to better understand the pathways through which nuisance ordinances threaten domestic violence survivors' access to housing.

My findings of nuisance ordinances' adverse effects on domestic violence underscore that policies aimed at deterring crime should consider their impact not only on potential offenders, but also on victims. Policymakers motivated to end domestic violence should ensure a policy's deterrence of a potential offender from perpetrating abuse is not outweighed by a deterrence of victims from reporting their abuse. A handful of states and municipalities have recently introduced legislation to reform nuisance ordinances. For example, the California State Assembly's passage of AB-2413 in August 2018 prohibits ordinance enforcement against victims of domestic violence and other crimes (Tenancy: Law Enforcement and Emergency Assistance, 2018). There have also been a growing number of litigation efforts to challenge the constitutionality of and repeal nuisance ordinances altogether (American Civil Liberties Union).<sup>27</sup> In future work, I intend to investigate whether exemptions of domestic violence from nuisance activities as specified in select ordinances and ordinance reforms are sufficient to avoid harming domestic violence victims.

More broadly, studying the impact of nuisance ordinances on domestic violence reveals a societal tendency to scrutinize victims' behavior rather than offenders' crimes. Blurring the distinction between victim and offender when enforcing nuisance ordinances against tenants effectively erases the crime of domestic violence. Consequently, nuisance ordinances penalize victims' decision to call 911 while failing to address the abuse that prompted their calls. Not only do nuisance ordinances place blame on victims for being unable to stop their abuse or for calling for help, but ordinances also reduce domestic violence to a "nuisance" unworthy of police action. As Diane, a survivor of domestic violence evicted under a nuisance ordinance, reflects: "*To me,*

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<sup>27</sup> Legal advocates have filed several claims against nuisance ordinances on the grounds that ordinances violate the First Amendment (right to petition, or to call the police), Fourth Amendment (unlawful seizure of property), Eighth Amendment (excessive fines or punishment), and Fourteenth Amendment (due process, without the opportunity to challenge nuisance citations, and equal protection for female domestic violence victims). Statutory challenges have also been proposed under the Fair Housing Act, which prohibits housing discrimination on the basis of sex, race, or disability (Cameron, 2012; Fais, 2008; Gavin, 2014; Mead et al., 2018).



*that's what I heard, 'We don't care...You a nuisance. You all are nuisance to us''* (Arnold, 2019, p. 1119). Understanding the barriers impeding domestic violence survivors' access to housing and safety – including those posed by nuisance ordinances – can help answer why survivors often do not “just leave”. With improved awareness of the implications of nuisance ordinances for domestic violence survivors, policymakers, landlords, and police forces alike should respond to domestic violence as an issue not simply to be repressed or displaced, but to be resolved.

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## Tables

TABLE 1: Nuisance ordinance summary statistics

	Frequency	Percent
Region		
Northeast	13	15.66%
Midwest	27	32.53%
South	14	16.87%
West	29	34.94%
Ordinance characteristics		
Domestic violence exempt from nuisance activities	10	12.05%
Domestic violence, assault, sexual abuse, or battery explicitly included in nuisance activities	58	69.88%
N = 83		

TABLE 2: California domestic violence-related calls for assistance summary statistics

VARIABLES	Mean	SD	N	Municipalities w/ ordinance (N = 84)	Municipalities w/o ordinance (N = 5,592)	P-value of difference
Monthly DV calls in 1995	36.07	189.41	5,676	1,029.46	21.15	0.020**
Monthly DV calls in 2016	24.51	98.30		533.13	16.87	0.015**

*Notes:* Data is at the agency-month-level, restricted to the 473 municipality-level agencies. San Jose, San Diego, Los Angeles, Long Beach, Fresno, Sacramento, and San Francisco comprise the municipalities with ordinances. Note that because Fresno, Sacramento, and San Francisco enacted nuisance ordinances in the final year of available data (2016), the post-ordinance period for these municipalities only includes a few months of data. The final column provides the p-value of the difference between municipalities with ordinances and municipalities without ordinances, using robust standard errors clustered by municipality. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 3: Effect of nuisance ordinance enactment on reported domestic violence in California

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Log of total domestic violence-related calls for assistance					
Post-ordinance	-0.232*** (0.0562)	-0.170** (0.0736)	-0.176** (0.0713)	-0.216*** (0.0550)	-0.165** (0.0728)	-0.171** (0.0706)
Observations	108,484	108,484	108,484	108,484	108,484	108,484
R-squared	0.856	0.861	0.866	0.857	0.861	0.866
County trends	No	Yes	No	No	Yes	No
County*year fixed effects	No	No	Yes	No	No	Yes
Municipality controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality. Municipality controls include the log of total homicides and an indicator that the dependent variable was recorded after a no-drop policy was enacted in a municipality. All regressions include municipality and call date (month-year) fixed effects.



TABLE 4: California Women’s Health Survey summary statistics

VARIABLES	Sample Percent	N	Municipalities w/ ordinance	Municipalities w/o ordinance	P-value of difference
Race					
White only, non-Hispanic	59.83%	62,893	48.73%	61.97%	0.087*
Black only, non-Hispanic	5.03%		10.29%	4.02%	0.028**
Asian/Pacific Islander only, non-Hispanic	6.01%		8.32%	5.56%	0.117
Other race, non-Hispanic	1.09%		0.84%	1.13%	0.034**
Hispanic	28.04%		31.82%	27.31%	0.460
Medi-Cal coverage	11.97%	62,886	14.22%	11.54%	0.271
Married	59.58%	61,916	50.91%	61.28%	0.004***
Has child(ren) in household	44.95%	62,895	43.14%	45.30%	0.469
High school graduate	50.39%	61,822	44.62%	51.53%	0.436
Employed	51.10%	61,788	52.52%	50.83%	0.174
Domestic violence					
Experienced physical or sexual abuse by partner	0.85%	40,736	0.99%	0.83%	0.192
Sought help for DV from police (2007-2009 only)	0.63%	5,583	0.88%	0.59%	0.543

Notes: Data is at the person-year-level, including municipality identifiers. San Jose, San Diego, Los Angeles, and Long Beach comprise the municipalities with ordinances. The final column provides the p-values of the differences between municipalities with ordinances and municipalities without ordinances, using robust standard errors clustered by municipality. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 5: Effect of nuisance ordinance enactment on domestic violence incidence in California

	(1)	(2)	(3)	(4)	(5)	(6)
<b>PANEL A</b>						
	Probability respondent experienced physical or sexual abuse by partner					
Post-ordinance	0.00739*** (0.00161)	0.00724*** (0.00204)	0.00525* (0.00278)	0.00744*** (0.00187)	0.00641** (0.00263)	0.00405 (0.00347)
Observations	38,945	38,945	38,945	38,945	38,945	38,945
R-squared	0.004	0.005	0.011	0.004	0.005	0.011
<b>PANEL B</b>						
	Probability respondent sought help for DV incident from police (2007-2009 only)					
Post-ordinance	-0.0857** (0.0326)	-0.0916** (0.0421)	-0.119** (0.0522)	-0.0859** (0.0333)	-0.0939** (0.0435)	-0.119** (0.0530)
Observations	5,336	5,336	5,336	5,336	5,336	5,336
R-squared	0.070	0.112	0.164	0.070	0.112	0.164
County trends	No	Yes	No	No	Yes	No
County*year fixed effects	No	No	Yes	No	No	Yes
Municipality controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality. Municipality controls include total homicides and an indicator that the dependent variable was recorded after a no-drop policy was enacted in a municipality. All regressions include municipality, county, and year fixed effects. County fixed effects are included in addition to municipality fixed effects to control for differences in pre-existing domestic violence trends between redacted municipalities.

TABLE 6: Effect of nuisance ordinance enactment on domestic violence incidence, reporting, and migration in California, by demographic characteristics

VARIABLES	(1) Probability respondent experienced physical or sexual abuse by partner	(2) Probability respondent sought help for DV incident from police (2007-2009 only)	(3) Probability respondent moved in last year	
Post-ordinance	0.00712*** (0.00168)	-0.0877*** (0.0316)	0.0391*** (0.0133)	<i>Demographics interacted w/ post-ordinance:</i>
<i>Demographics:</i>				
Black	0.00504** (0.00253)	-0.00225 (0.00454)	0.0106 (0.0125)	-0.00681 (0.00945)
Asian	0.00183 (0.00113)	-0.00199 (0.00338)	0.0423*** (0.00546)	-0.00880 (0.00679)
Other race	0.00724 (0.00454)	-0.0104*** (0.00386)	0.0381*** (0.00172)	-0.0121 (0.00887)
Hispanic	0.00351*** (0.00116)	0.00355 (0.00418)	0.0181*** (0.00622)	-0.0266 (0.0169)
Medi-Cal coverage	0.00961*** (0.00165)	0.00309 (0.00461)	0.0259*** (0.00127)	-0.0325*** (0.00116)
Married	-0.0104*** (0.000901)	-0.00597** (0.00239)	-0.0839*** (0.00137)	0.0149*** (0.00135)
Has child(ren)	0.00628*** (0.000594)	0.000511 (0.00194)	0.0167*** (0.00430)	-0.0496*** (0.00389)
HS graduate	-0.000296 (0.00170)	0.00526 (0.00452)	0.0219*** (0.00357)	0.0137*** (0.00229)
Employed	0.000 (0.000744)	0.000200 (0.00207)	0.0390*** (0.00476)	-0.0241*** (0.00712)
Observations	38,945	5,336	3,028,292	<i>F-test (p-value):</i>
R-squared	0.011	0.072	0.174	0.000

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality. Columns 1 and 2 use data from the California Women's Health Survey and include municipality, county, and year fixed effects; county fixed effects are included in addition to municipality fixed effects to control for differences in pre-existing domestic violence trends between redacted municipalities. Column 3 uses data from the American Community Survey and includes municipality and year fixed effects. All respondents are adult women in California.

TABLE 7: Mortality data summary statistics

VARIABLES	Mean	SD	N	Counties w/ ordinance (N = 68)	Counties w/o ordinance (N = 3,086)	P-value of difference
Female deaths by assault at home in 1990	0.39	1.66	3,154	4.65	0.29	0.000***
Female deaths by assault at home in 2016	0.62	1.85		6.07	0.49	0.000***

*Notes:* Data is at the county-year-level, restricted to ages 20-54. Municipalities within 68 counties have enacted ordinances; ordinances were enacted in municipalities within 60 counties during the dataset's time period. The final column provides the p-value of the difference between counties with municipalities that have enacted ordinances and counties without ordinances, using robust standard errors clustered by state. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 8: Effect of nuisance ordinance enactment on female mortality from assault

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total female deaths by assault at home					
Post-ordinance	0.932* (0.504)	0.908* (0.464)	0.933* (0.509)	0.222 (0.250)	0.220 (0.257)	0.243 (0.267)
Observations	85,158	85,158	85,158	81,682	81,682	81,682
R-squared	0.765	0.770	0.782	0.814	0.815	0.824
State trends	No	Yes	No	No	Yes	No
State*year fixed effects	No	No	Yes	No	No	Yes
County controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality within a county. County controls include the proportion of a county's total population that is male, the proportion of a county's total population that is Black, the proportion of a county's total population that is Hispanic, total male deaths by assault in a county, an indicator that the dependent variable was recorded after a no-drop policy was enacted in a municipality within a county, and the lag number of female deaths by assault in a county, following Aizer and Dal Bó (2009). Data is restricted to ages 20-54. All regressions include county and year fixed effects.

TABLE 9: Effect of nuisance ordinance enactment on Google Trends search topics related to domestic violence

	(1)	(2)	(3)
<hr/>			
PANEL A	"domestic violence"		
<hr/>			
Post-ordinance	1.237 (0.942)	0.918 (1.021)	1.156 (1.193)
Observations	2,617	2,617	2,617
R-squared	0.841	0.852	0.882
<hr/>			
PANEL B	"women's shelter"		
<hr/>			
Post-ordinance	3.456*** (0.840)	4.021*** (0.865)	4.675*** (1.515)
Observations	1,732	1,732	1,732
R-squared	0.689	0.712	0.788
<hr/>			
PANEL C	"eviction"		
<hr/>			
Post-ordinance	4.526*** (0.886)	4.182*** (0.695)	5.558*** (1.119)
Observations	2,423	2,423	2,423
R-squared	0.801	0.825	0.860
<hr/>			
PANEL D	"gender"		
<hr/>			
Post-ordinance	0.813 (0.839)	-0.288 (0.691)	-0.618 (0.776)
Observations	2,671	2,671	2,671
R-squared	0.917	0.923	0.940
<hr/>			
State trends	No	Yes	No
State*year fixed effects	No	No	Yes
<hr/>			

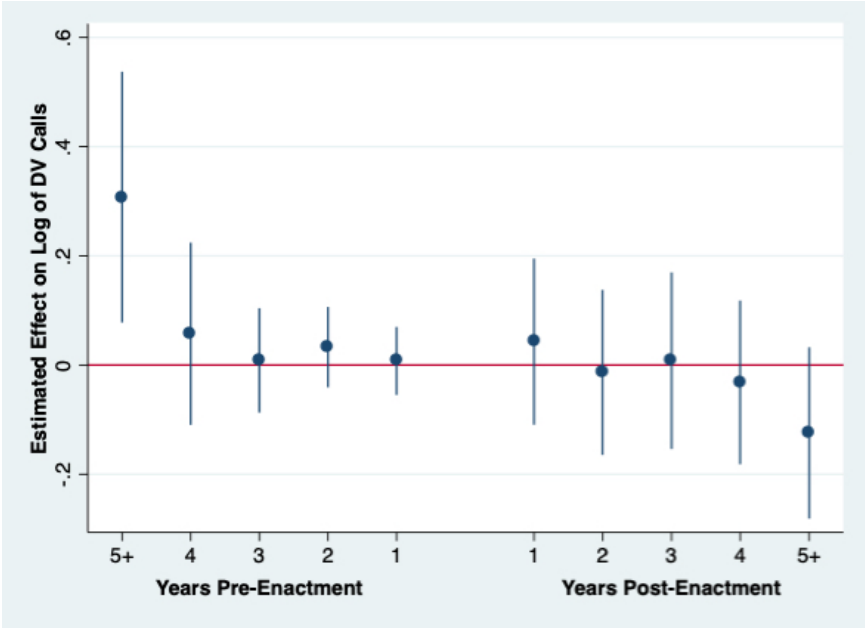
Robust standard errors clustered by state in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality within a DMA. All regressions include DMA and year fixed effects. Search index values were averaged across five days of data collection; only DMA-years present in all five days of data were included in the analyses.

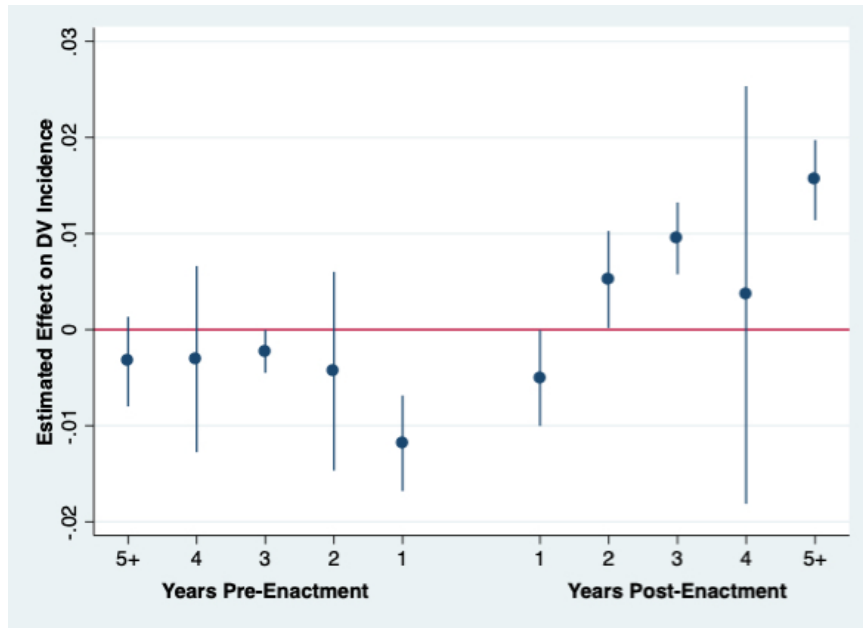
# Figures

FIGURE 1: Event study estimates for reported domestic violence



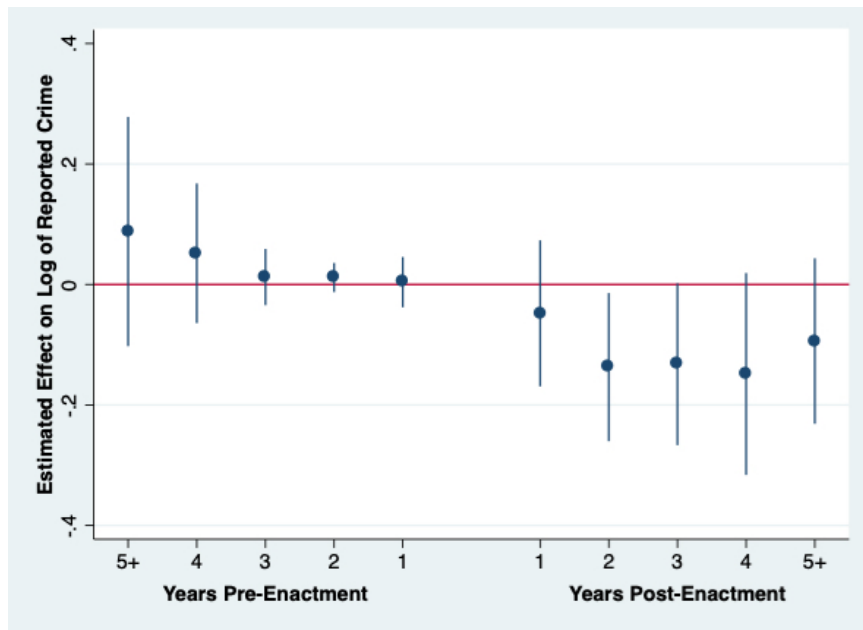
Notes: Zero years since nuisance ordinance enactment is the omitted time period, which also includes municipalities that never enacted an ordinance. Municipality and call date (month-year) fixed effects are included.

FIGURE 2: Event study estimates for domestic violence incidence



Notes: Zero years since nuisance ordinance enactment is the omitted time period, which also includes municipalities that never enacted an ordinance. Municipality, county, and year fixed effects are included.

FIGURE 3: Event study estimates for other reported crimes



Notes: Zero years since nuisance ordinance enactment is the omitted time period, which also includes municipalities that never enacted an ordinance. Municipality and year fixed effects are included.

## Appendix

TABLE A1: Nuisance ordinances by state, municipality, and year of enactment

Municipality	State	Year Enacted
Mesa <sup>b</sup>	AZ	2009
Phoenix	AZ	1962
Tucson <sup>b</sup>	AZ	2017
Fresno <sup>a</sup>	CA	2016
Long Beach <sup>b</sup>	CA	2011
Los Angeles <sup>b</sup>	CA	2008
Sacramento <sup>b</sup>	CA	2016
San Diego	CA	2007
San Francisco <sup>b</sup>	CA	2016
San Jose <sup>b</sup>	CA	1998
Boulder <sup>b</sup>	CO	2005
Colorado Springs <sup>b</sup>	CO	2001
Denver <sup>b</sup>	CO	1998
Fort Collins	CO	2000
Washington <sup>b</sup>	DC	2016
Jacksonville <sup>b</sup>	FL	2017
Miami	FL	2004
Atlanta <sup>b</sup>	GA	2017
Aurora <sup>b</sup>	IL	2008
Bloomington <sup>b</sup>	IL	2009
Chicago <sup>a</sup>	IL	2010
Freeport <sup>b</sup>	IL	2005
Kankakee <sup>b</sup>	IL	1996
Mundelein	IL	2004
Village of Addison <sup>b</sup>	IL	2009
Village of Wauconda <sup>b</sup>	IL	2008
West Chicago City <sup>b</sup>	IL	2008
Indianapolis	IN	2009
Louisville <sup>b</sup>	KY	2016
Boston <sup>b</sup>	MA	2014
Baltimore <sup>b</sup>	MD	2007
Brunswick <sup>b</sup>	ME	2008
Detroit	MI	1964
Minneapolis	MN	1994
St. Paul	MN	2001



Kansas City <sup>a, b</sup>	MO	2008
St. Louis <sup>b</sup>	MO	2007
Charlotte <sup>a</sup>	NC	2013
Omaha	NE	2008
Phillipsburg <sup>a</sup>	NJ	2005
Albuquerque <sup>b</sup>	NM	2017
Carson City	NV	2005
Las Vegas <sup>b</sup>	NV	2017
Auburn	NY	2004
New York <sup>b</sup>	NY	2017
Rome <sup>b</sup>	NY	1998
Village of East Rochester <sup>a</sup>	NY	2009
Cincinnati <sup>b</sup>	OH	2006
Cleveland <sup>b</sup>	OH	2009
Columbus	OH	2005
Oklahoma City <sup>b</sup>	OK	2015
Beaverton <sup>b</sup>	OR	1998
Bend <sup>b</sup>	OR	2010
Clackamas County <sup>b</sup>	OR	2002
Dalles <sup>b</sup>	OR	1998
Dayton <sup>b</sup>	OR	1999
Medford <sup>b</sup>	OR	1998
Portland <sup>b</sup>	OR	2002
Salem <sup>b</sup>	OR	2009
Tigard <sup>b</sup>	OR	2003
Coaldale	PA	2006
Hazleton <sup>b</sup>	PA	2009
Philadelphia	PA	2016
Pittsburgh	PA	2005
Wilkes-Barre	PA	2005
York <sup>b</sup>	PA	2007
Nashville <sup>b</sup>	TN	2000
Dallas <sup>a</sup>	TX	1975
Fort Worth <sup>b</sup>	TX	2014
Houston <sup>a</sup>	TX	2006
San Antonio <sup>b</sup>	TX	2007
Bremerton <sup>b</sup>	WA	2008
Everett <sup>b</sup>	WA	2008
Seattle <sup>b</sup>	WA	2009
Elm Grove Village <sup>b</sup>	WI	2008

Green Bay <sup>b</sup>	WI	2006
Hartland Village <sup>b</sup>	WI	2008
Janesville <sup>b</sup>	WI	2008
Madison <sup>a</sup>	WI	2008
Menasha <sup>b</sup>	WI	2008
Middleton <sup>b</sup>	WI	2008
Milwaukee <sup>a, b</sup>	WI	2007
Oshkosh <sup>b</sup>	WI	2010

<sup>a</sup> Domestic violence exempt from nuisance activities

<sup>b</sup> Domestic violence, assault, sexual abuse, or battery explicitly included in nuisance activities

TABLE A2: Effect of nuisance ordinance enactment on reported domestic violence in California

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total domestic violence-related calls for assistance					
Post-ordinance	-396.8** (201.3)	-393.2* (201.0)	-403.5* (206.9)	-135.8*** (52.37)	-133.0*** (51.08)	-135.7*** (51.98)
Observations	124,872	124,872	124,872	124,872	124,872	124,872
R-squared	0.923	0.924	0.929	0.955	0.955	0.959
County trends	No	Yes	No	No	Yes	No
County*year fixed effects	No	No	Yes	No	No	Yes
Municipality controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality. Municipality controls include total homicides and an indicator that the dependent variable was recorded after a no-drop policy was enacted in a municipality. All regressions include municipality and call date (month-year) fixed effects.

TABLE A3: Effect of nuisance ordinance enactment on reported domestic violence in California

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total domestic violence-related calls for assistance					
Post-ordinance	-0.275*** (0.0672)	-0.320** (0.135)	-0.275** (0.112)	-0.227*** (0.0623)	-0.294** (0.133)	-0.251** (0.110)
Observations	124,872	124,872	124,872	124,872	124,872	124,872
R-squared	0.834	0.842	0.853	0.836	0.843	0.854
County trends	No	Yes	No	No	Yes	No
County*year fixed effects	No	No	Yes	No	No	Yes
Municipality controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* "Post-ordinance" is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality. Municipality controls include the log of total homicides and an indicator that the dependent variable was recorded after a no-drop policy was enacted in a municipality. When generating the logged variables, I added 1 to each observation to include municipalities in which zero calls or zero homicides occurred. All regressions include municipality and call date (month-year) fixed effects.

TABLE A4: Event study for the effect of nuisance ordinance enactment on reported domestic violence in California

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Log of total domestic violence-related calls for assistance					
Years since ordinance enactment						
Pre-5+ years	0.308*** (0.117)	0.255** (0.113)	0.202* (0.113)	0.308*** (0.113)	0.260** (0.112)	0.210* (0.110)
Pre-4 years	0.0573 (0.0850)	0.0450 (0.0931)	0.0275 (0.0960)	0.0452 (0.0888)	0.0391 (0.0950)	0.0254 (0.0980)
Pre-3 years	0.00856 (0.0487)	-0.00994 (0.0502)	-0.0472 (0.0931)	0.00558 (0.0505)	-0.0112 (0.0512)	-0.0462 (0.0916)
Pre-2 years	0.0328 (0.0375)	0.0201 (0.0388)	0.0120 (0.0644)	0.0304 (0.0414)	0.0189 (0.0410)	0.00866 (0.0645)
Pre-1 year	0.00753 (0.0317)	0.00107 (0.0323)	-0.0188 (0.0446)	0.00340 (0.0323)	-0.00121 (0.0326)	-0.0200 (0.0436)
0 years (omitted)						
Post-1 year	0.0429 (0.0775)	0.0377 (0.0737)	0.0293 (0.0635)	0.0571 (0.0743)	0.0443 (0.0719)	0.0355 (0.0609)
Post-2 years	-0.0135 (0.0769)	-0.0125 (0.0689)	-0.0785 (0.0655)	0.00770 (0.0738)	-0.00217 (0.0668)	-0.0668 (0.0619)
Post-3 years	0.00792 (0.0823)	0.0150 (0.0822)	-0.00894 (0.0722)	0.0359 (0.0797)	0.0291 (0.0800)	0.00431 (0.0697)
Post-4 years	-0.0318 (0.0763)	-0.0178 (0.0763)	-0.0764 (0.0680)	-0.0152 (0.0726)	-0.0112 (0.0736)	-0.0681 (0.0655)
Post-5+ years	-0.124 (0.0798)	-0.0782 (0.0672)	-0.121* (0.0651)	-0.112 (0.0814)	-0.0730 (0.0664)	-0.113* (0.0634)
Observations	108,484	108,484	108,484	108,484	108,484	108,484
R-squared	0.856	0.861	0.866	0.857	0.861	0.866
F-tests (p-value)						
Pre-ordinance enactment	0.003	0.000	0.039	0.000	0.000	0.011
Post-ordinance enactment	0.000	0.000	0.007	0.000	0.000	0.010
County trends	Yes	Yes	No	No	Yes	No
County*year fixed effects	No	No	Yes	No	No	Yes
Municipality controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The independent variables are indicators of the number of years since a nuisance ordinance was enacted in a municipality relative to the year the dependent variable was recorded in that municipality. Zero years since nuisance ordinance enactment is the omitted time period, which also includes municipalities that never enacted an ordinance. Municipality controls include the log of total homicides and an indicator that the dependent variable was recorded

after a no-drop policy was enacted in a municipality. All regressions include municipality and call date (month-year) fixed effects.

TABLE A5: Event study for the effect of nuisance ordinance enactment on domestic violence incidence in California

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Probability respondent experienced physical or sexual abuse by partner					
Years since ordinance enactment						
Pre-5+ years	-0.00329 (0.00236)	-0.00368 (0.00285)	-0.00865** (0.00387)	-0.00246 (0.00396)	-0.00199 (0.00471)	-0.00740 (0.00580)
Pre-4 years	-0.00303 (0.00488)	-0.00293 (0.00546)	-0.00353 (0.00535)	-0.00229 (0.00476)	-0.00142 (0.00573)	-0.00241 (0.00603)
Pre-3 years	-0.00224* (0.00113)	-0.00231* (0.00128)	-0.00117 (0.00267)	-0.00168 (0.00225)	-0.00115 (0.00280)	-0.000298 (0.00357)
Pre-2 years	-0.00429 (0.00521)	-0.00434 (0.00540)	-0.00739 (0.00557)	-0.00383 (0.00517)	-0.00339 (0.00545)	-0.00661 (0.00602)
Pre-1 year	-0.0118*** (0.00251)	-0.0118*** (0.00256)	-0.0165*** (0.00431)	-0.0117*** (0.00251)	-0.0117*** (0.00257)	-0.0164*** (0.00434)
0 years (omitted)						
Post-1 year	-0.00501** (0.00251)	-0.00496* (0.00264)	-0.00770 (0.00545)	-0.00526** (0.00228)	-0.00547** (0.00248)	-0.00805 (0.00550)
Post-2 years	0.00529** (0.00254)	0.00523* (0.00297)	-0.00224 (0.00423)	0.00534** (0.00251)	0.00503* (0.00289)	-0.00216 (0.00418)
Post-3 years	0.00956*** (0.00188)	0.00997*** (0.00232)	0.00398 (0.00424)	0.00885*** (0.00232)	0.00858*** (0.00287)	0.00284 (0.00418)
Post-4 years	0.00365 (0.0109)	0.00338 (0.0106)	-0.000547 (0.0116)	0.00331 (0.0112)	0.00253 (0.0108)	-0.00115 (0.0116)
Post-5+ years	0.0157*** (0.00215)	0.0142*** (0.00332)	0.00802* (0.00482)	0.0156*** (0.00198)	0.0138*** (0.00329)	0.00797* (0.00477)
Observations	38,945	38,945	38,945	38,945	38,945	38,945
R-squared	0.004	0.005	0.011	0.004	0.005	0.011
F-tests (p-value)						
Pre-ordinance enactment	0.000	0.000	0.000	0.000	0.000	0.000
Post-ordinance enactment	0.000	0.000	0.000	0.000	0.000	0.000
County trends	Yes	Yes	No	No	Yes	No
County*year fixed effects	No	No	Yes	No	No	Yes
Municipality controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

*Notes:* The independent variables are indicators of the number of years since a nuisance ordinance was enacted in a municipality relative to the year the dependent variable was recorded in that municipality. Zero years since nuisance ordinance enactment is the omitted time period, which also includes municipalities that never enacted an ordinance. Municipality controls include total homicides and an indicator that the dependent variable was recorded after a no-drop policy was enacted in a municipality. All regressions include municipality, county, and year fixed effects. County fixed effects are included in addition to municipality fixed effects to control for differences in pre-existing domestic violence trends between redacted municipalities.

TABLE A6: Event study for the effect of nuisance ordinance enactment on other reported crimes in California

VARIABLES	(1)	(2)	(3)
	Log of total crimes reported		
Years since ordinance enactment			
Pre-5+ years	0.0879 (0.0968)	0.166* (0.0994)	0.144* (0.0807)
Pre-4 years	0.0518 (0.0590)	0.0580 (0.0613)	-0.0233 (0.0761)
Pre-3 years	0.0123 (0.0238)	0.0251 (0.0332)	-0.0786 (0.0571)
Pre-2 years	0.0115 (0.0123)	0.0198 (0.0193)	-0.0635* (0.0330)
Pre-1 year	0.00385 (0.0212)	0.00822 (0.0246)	-0.0560*** (0.0209)
0 years (omitted)			
Post-1 year	-0.0479 (0.0617)	0.0501 (0.0640)	0.00413 (0.0590)
Post-2 years	-0.137** (0.0626)	-0.0339 (0.0673)	-0.0764 (0.0577)
Post-3 years	-0.132* (0.0684)	-0.0234 (0.0721)	-0.0395 (0.0524)
Post-4 years	-0.149* (0.0853)	-0.0339 (0.0910)	-0.0333 (0.0626)
Post-5+ years	-0.0939 (0.0699)	0.0160 (0.0637)	-0.0237 (0.0501)
Observations	120,792	120,792	120,792
R-squared	0.974	0.977	0.981
F-tests (p-value)			
Pre-ordinance enactment	0.690	0.147	0.000
Post-ordinance enactment	0.000	0.000	0.000
County trends	No	Yes	No
County*year fixed effects	No	No	Yes

Robust standard errors clustered by municipality in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* The independent variables are indicators of the number of years since a nuisance ordinance was enacted in a municipality relative to the year the dependent variable was recorded in that municipality. “Reported crimes” is defined as the sum of homicide, rape, robbery, and aggravated assault, burglary, larceny-theft, motor vehicle theft, and arson. Zero years since nuisance ordinance enactment is the omitted time period, which also includes municipalities that never enacted an ordinance. All regressions include municipality and year fixed effects.

TABLE A7: Effect of nuisance ordinance enactment on male mortality from assault

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Total male deaths by assault, not at home					
Post-ordinance	-2.928 (3.337)	-2.481 (3.251)	-2.477 (3.448)	-2.805 (3.263)	-2.436 (3.338)	-2.442 (3.379)
Observations	85,158	85,158	85,158	84,823	84,823	84,823
R-squared	0.767	0.774	0.778	0.770	0.777	0.782
State trends	No	Yes	No	No	Yes	No
State*year fixed effects	No	No	Yes	No	No	Yes
County controls	No	No	No	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* “Post-ordinance” is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality within a county. County controls include the proportion of a county’s total population that is male, the proportion of a county’s total population that is Black, and the proportion of a county’s total population that is Hispanic. Data is restricted to ages 20-54. All regressions include county and year fixed effects.



TABLE A8: Effect of nuisance ordinance enactment on Google Trends search topics related to domestic violence

	(1)	(2)	(3)
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PANEL A	“domestic violence”		
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Post-ordinance	1.649* (0.971)	1.092 (0.894)	1.256 (1.195)
Observations	2,704	2,704	2,704
R-squared	0.819	0.830	0.861
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PANEL B	“women’s shelter”		
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Post-ordinance	5.334*** (0.855)	5.776*** (1.248)	7.014*** (1.286)
Observations	2,110	2,110	2,110
R-squared	0.599	0.631	0.704
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PANEL C	“eviction”		
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Post-ordinance	7.276*** (1.237)	5.617*** (1.166)	7.301*** (1.515)
Observations	2,618	2,618	2,618
R-squared	0.733	0.778	0.818
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PANEL D	“gender”		
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Post-ordinance	1.361 (0.897)	0.0343 (0.800)	-0.214 (0.831)
Observations	2,716	2,716	2,716
R-squared	0.896	0.906	0.924
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State trends	No	Yes	No
State*year fixed effects	No	No	Yes
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Robust standard errors clustered by state in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

*Notes:* “Post-ordinance” is an indicator that the dependent variable was recorded after an ordinance was enacted in a municipality within a DMA. All regressions include DMA and year fixed effects. Search index values were averaged across five days of data collection; no DMA-years present in less than five days of data were dropped when calculating the averages.