DISCUSSION PAPERS IN ECONOMICS

Working Paper No. 21-05

Do Shelters Reduce Domestic Violence?

Lauren Schechter
University of Colorado Boulder

October 22, 2021

Department of Economics

University of Colorado Boulder
Boulder, Colorado 80309

Do Shelters Reduce Domestic Violence?

Lauren Schechter*

October 22, 2021

Abstract

This paper estimates the effect of having a shelter in a small to mid-size county on intimate partner homicide rates. Results from state-of-the-art difference-in-differences models exploiting county-by-year changes on the extensive margin of shelter availability suggest that opening a shelter where there were no shelters previously can reduce the rate of intimate partner homicides with female victims by about one homicide per 100,000 people every three to four years. Emergency shelter services are often treated as the bare minimum standard of care for housing-insecure families at high risk for intimate partner violence, but 33% of counties in the sample did not have a shelter at any point between 1998 and 2016.

Keywords: Domestic violence, crime, gender, housing
JEL Codes: H4, I3, J1, K4
Latest version available here.

*University of Colorado, Boulder. This publication utilizes data from the NCANDS Child File which have been provided by the National Data Archive on Child Abuse and Neglect (NDACAN), a service of the Children's Bureau, U.S. Department of Health & Human Services. Nothing herein should be construed to indicate the support or endorsement of its content by the collector of the original data, their funding agency, NDACAN, or ACF/DHHS. Many thanks to Brian Cadena, Terra McKinnish, Tania Barham, Rick Mansfield, Steve Billings, Chloe East, Angelica Meinhofer, and additional seminar/conference participants at CU Boulder, UC Irvine, APPAM, and WEAI for their helpful comments on this paper. Additional thanks to Kevin Schnepel, Ben Hansen, Jen Doleac, and Alexa Prettyman for their important insights about key data sources. Lastly, thanks to Kate Schwab for excellent research assistance.

1 Introduction

The CDC estimates that about 1 in 4 women in the United States experiences intimate partner violence (IPV), including physical, sexual, or stalking violence at the hands of an intimate partner during her lifetime (Smith et al., 2018). Approximately one third of homicides with female victims are perpetrated by intimate partners, and most of these female intimate partner homicides are preceded by prior intimate parter violence (Campbell et al., 2007). Domestic violence, in addition to the trauma and physical injury it causes, is known to be associated with lower employment rates and higher rates of housing insecurity among women (Lindhorst et al., 2007; Baker et al., 2010). Infants exposed to domestic violence in utero experience higher rates of adverse birth outcomes (Currie et al., 2018) and children who witness domestic violence are at higher risk for cognitive, behavioral, and mental health problems (Stiles, 2002), which may even spill over to create adverse outcomes for their peers (Carrell and Hoekstra, 2010).

Outside of criminal justice system interventions, which focus primarily on arresting or incarcerating the offender, shelters are one of the few resources targeted towards combating domestic violence. Domestic violence hotlines often serve the function of referring callers to shelters in their local area, which may provide in-house assistance with other services such as counseling, legal advocacy, and assistance with paperwork to apply for government benefits. Despite being one of the few services commonly available to domestic violence survivors and being regarded as the bare minimum standard of care for housing-insecure families at high risk for domestic violence, little is known about shelters' effectiveness in preventing violence.[1]

In this paper, I estimate the causal effect of the availability of a temporary shelter on intimate partner homicides using a difference-in-differences design, which exploits variation from shelter openings in counties that previously had no shelters and total closures of all shelters in counties that previously had at least one shelter. Data on shelter availability come from the Census Bureau's County Business Patterns data, and reliably report whether a county has a shelter but do not differentiate between designated domestic violence shelters and other types of homeless shelters. The sample is limited to counties small enough to be plausibly at risk of not having a shelter, and hand-collected data for a subset of these counties show that a large proportion of shelters in these small counties are domestic violence shelters.[2] In addition to the supplemen-

[1] Domestic violence advocates and service providers often prefer the use of the term "survivor" rather than "victim" when referring to individuals who have experienced domestic violence. I alternate between the two terms in this paper due to the use of homicide as an outcome variable.

[2] See Section 7.1 in the Appendix for details.

tary services they provide, domestic violence shelters are often required by law to keep their locations confidential to increase resident safety and are likely to prioritize bed space to severe domestic violence cases (Koppa, 2020; Messing et al., 2015).

Regardless, any shelter has the potential to mitigate domestic violence. Family violence is highly correlated with poverty (Harrell et al., 2014) and homeless families experience even higher rates of domestic violence than other low-income families (Wood et al., 1990). The consensus in the literature on homelessness is that homeless individuals experience higher rates of violence in general, and that many domestic violence survivors cycle in and out of homelessness throughout their lives (Diette and Ribar, 2018; O'Flaherty, 2019). However, most existing work linking homelessness and domestic violence has focused on large urban areas and has therefore operated in a context where some type of emergency shelter is always available, making it difficult to study the effects of shelters directly.

For those already living with a violent partner, the availability of a shelter may reduce the cost of permanently leaving the abusive relationship or of creating a temporary physical separation from the abuser, which may allow for de-escalation of conflict, changes in bargaining power within the relationship, or at least a temporary reprieve from continued violence (Chanley et al., 2001; Farmer and Tiefenthaler, 1997). While many domestic violence survivors leave abusive relationships temporarily and eventually return, the availability of a shelter that provides a safe place to stay, often along with other services, may increase the probability of permanently leaving the relationship (Sims, 2021). Alternatively, the availability of a shelter may prevent those at risk of homelessness who are considering moving in with a violent or potentially violent partner or ex-partner from doing so.

Data on intimate partner homicides come from the FBI Uniform Crime Reporting Program's Supplementary Homicide Reports, which identify the relationship between the offender and the victim. I use intimate partner homicides as the outcome variable because other measures of domestic violence often have embedded selection bias due to underreporting, while nearly all homicides are reported. Stigma, shame, mistrust of police, or fear of retaliation from the abuser are all unobserved factors that may influence a victim's decision of whether or not to self-select into reporting domestic assaults. If the services afforded by a shelter embolden survivors and make them more likely to report their abuse to the authorities, using reported domestic assaults as an outcome could bias any negative effects of shelters on domestic and intimate partner violence towards zero.

To estimate this relationship, I use state-of-the-art difference-in-differences estimators, both the DID-M estimator relying on first differences developed by De Chaisemartin and d'Haultfoeuille (2020) and the staggered implementation estimator allowing for long-term effects developed by Callaway and Sant'Anna (2020). I find that small and mid-size counties that that open their first shelter see a decline of approximately one female intimate partner homicide per 100,000 people every three to four years compared to other counties where shelter availability has not changed. As long as changes in intimate partner homicide rates in counties with shelter openings and closings would have been similar to those in comparison counties in the absence of those openings/closings, this decrease can be interpreted as a causal effect of shelter availability on intimate partner violence. This finding would not have been possible using the traditional OLS two-way fixed effects (TWFE) and first-differences (FD) estimators despite relatively constant treatment effects over time, as the weighting problems with these estimators documented by Goodman-Bacon (2021), De Chaisemartin and d'Haultfoeuille (2020), Baker et al. (2021), and others attenuate the effect to the point that it is not clearly detectable. To my knowledge, this is the first paper to present causal evidence that shelters can reduce the incidence of intimate partner violence; older studies were unable to detect an effect, likely due to the use of the TWFE estimator (Dugan et al., 1999, 2003).

This paper contributes to a growing literature on the causal effects of targeted policy interventions to combat domestic violence. Most papers in this literature have evaluated criminal justice interventions focused on convicting or incarcerating offenders, such as no-drop policies[3] (Aizer and Dal Bo, 2009), discretionary arrest policies[4] (Chin and Cunningham, 2019), sanctuary policies[5] (Amuedo-Dorantes and Deza, 2020), or restrictions on gun purchases for those convicted of domestic violence misdemeanors (Raissian, 2016). Koppa (2020) and Messing et al. (2015), on the other hand, evaluate a victim-focused intervention called a "lethality assessment" in which police use a series of questions to estimate a complainant's indicated risk for intimate partner homicide and find that this intervention can reduce both fatal and non-fatal future victimization. While these policy levers are important in protecting survivors who are willing to call the police, work by Ellsberg et al. (2001) finds that not all survivors are willing to self-report assaults to law enforcement. The findings in this paper build on this existing body of work by evaluating a victim-focused intervention that can be administered with or without the involvement of law

[3] No drop policies prevent a victim from requesting that domestic violence charges be dropped once they have been filed.

[4] Discretionary arrest policies allow police to arrest a perpetrator without a warrant during domestic violence calls for service.

[5] Sanctuary policies prohibit police from sharing information with immigration enforcement authorities when crimes are reported.

enforcement and criminal justice authorities.

This paper also contributes to a broader literature on the economic conditions surrounding domestic violence. Past findings indicate that domestic violence responds to the timing of food and cash assistance benefits (Hsu, 2017; Carr and Packham, 2021), the gender wage gap (Aizer, 2010), and gender-specific unemployment rates (Anderberg et al., 2016). Additionally, domestic violence 911 calls increased sharply during the COVID-19 pandemic (Leslie and Wilson, 2020; Bullinger et al., 2021), a time of heightened economic stress and increased time at home with domestic partners. The paper contributes to the literature on domestic violence and economic conditions as well by evaluating the effects of an intervention that provides victims with a non-fungible and non-transferable economic resource that can help them separate from or avoid moving in with abusive partners.

Lastly, another emerging literature studies the relationship between housing assistance and domestic violence, but this paper makes an important contribution by focusing on whether or not there is any local shelter available. While the HUD-funded Family Options Study finds that subsidies for permanent housing reduce intimate partner violence more than access to shelters alone (Gubits et al., 2016), it does not provide evidence on the effectiveness of shelters themselves because all cities in the study had emergency shelters.[6] The most closely related paper is a contemporaneously developed working paper by Sims (2021), which models survivors' stay/leave decision and empirically estimates the effects of existing shelters' capacity expansions on intimate partner homicides, finding no effect. This finding is surprising, as data from the National Network to End Domestic Violence (2008-2017) suggests that shelters are highly utilized and often face capacity constraints (see Table 12). In Section 5, I posit that law enforcement, hotline workers, and shelter staff may successfully prioritize bed space for domestic violence victims who are at the highest risk of intimate partner homicide. The results in this paper are complementary to those of Sims (2021), and suggest that funding for shelters may be more effective in reducing intimate partner homicides when targeted toward the opening of new shelters in areas where none exist rather than toward capacity expansions for existing shelters. Over the course of the panel used in this paper, spanning from 1998-2016, 62% of counties in the sample experienced at least one year with no shelters and 33% never had a shelter, differentiating the paper from past studies that take the existence of some type of emergency shelter services as a baseline level of homeless services and suggesting scope for new shelters to prevent additional homicides.

[6] In fact, participants in the study were recruited from the existing clientele of emergency shelters in each city, explicitly excluding domestic violence shelters.

Section 2 describes the data sources used in the analysis, including details on how intimate partner homicide rates are constructed and why focusing on the extensive margin is best suited to the data and question. Section 3 details the identifying assumptions required for these estimates to be considered causal, describes the difference-in-differences estimators used to obtain both on-impact and long-term effects of shelters on intimate partner homicide rates, and explains their advantages over the classic OLS first differences (FD) and two-way fixed effects (TWFE) estimators. Section 4 presents main results on the effect of shelters on intimate partner homicides, as well as robustness tests and secondary results exploring effects on divorce, marriage, and child maltreatment. Section 5 puts the results in context, discusses their policy implications, and estimates the number of homicides averted by shelters for counties in the sample over the 18-year analysis period.

2 Data

I focus exclusively on the extensive margin: whether a county has any shelters at all. Data on shelters are available from the Census Bureau's County Business Patterns data from 1998-2016.[7] The NAICS code "624221" covers all temporary shelters, including domestic violence shelters, general homeless shelters, and youth shelters. One limitation of the data is that I cannot distinguish how many shelters in each county are domestic violence shelters. Although domestic violence shelters often provide additional services such as location confidentiality, legal and social services assistance, counseling, and comfortable accommodations for children, general homeless shelters may serve as an imperfect substitute where domestic violence shelters are not available. Hand-collected data on shelters from 2016 that appear to still be open as of 2021 suggest that, in small to mid-size counties with only one shelter, about 73% are domestic violence shelters; in those with multiple shelters, about 90% have at least one domestic violence shelter.[8] Another limitation is that the actual size of each shelter is not observed. In theory, the CBP data reports employment counts for each industry and establishment; however, in practice, the employment counts are censored to the extent that they are not useful for determining whether a shelter that opens or closes is small or large relative to other shelters in the county, or whether a county experiences a change in shelter capacity without changing the number of shelters[9]. For these reasons,

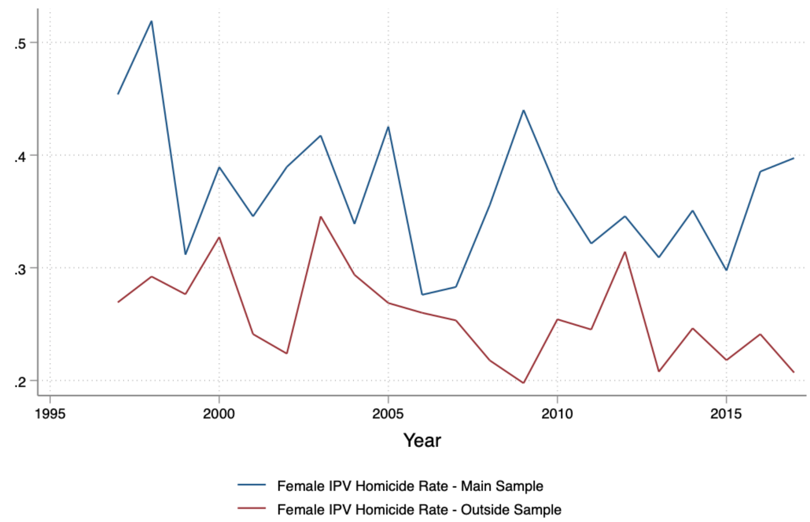
[7] See Appendix for details on the time horizon of the panel.

[8] More details on these rough estimates of shelter types are presented in Appendix Tables 13, 14, and 15.

[9] For details on employment censoring in the County Business Patterns data, see Eckert et al. (2020) They impute employment for county-industry cells from 1975 to 2016; however, Temporary Shelters did not have their own code under

and because of the potential for the allocation of shelter services to clients with the most severe need after screening by law enforcement and service providers, the extensive margin is best suited to the context and data availability.

Figure 1: Trends in IPV Homicide Rates: Main Sample vs. Larger Counties



Notes: Figure shows average female intimate partner homicide rate (homicides per 100,000 people per year) in police departments in the main sample vs. in police departments excluded from the main sample due to large population and no variation in whether the county has a shelter.

Many large, urban counties have multiple shelters in every year in the panel and do not provide any useful variation in shelter availability. However, limiting the sample based on observed treatment status throughout the panel could introduce selection bias. To mitigate these concerns, I limit the sample to counties whose populations are less than or equal to that of the largest county that begins the panel with zero shelters in 1998, the first year that the number of shelters is observed.[10] Figure 1 shows that counties excluded from the sample by this population threshold do in fact have different trends in intimate partner homicide rates than counties in the main sample, so they are unlikely to provide a useful counterfactual for counties at risk of not having any shelters. Counties in the main sample of small counties have higher intimate partner homicide rates in general, and the divergence in trends between the main sample and the large counties is particularly notable during the time period of the Great Recession.

the SIC system, instead falling under code "8322" which covers Individual and Family social services more generally, so they are excluded from this imputed dataset.

[10] As discussed in Section 4.3, results are robust to the manipulation of this threshold.

Table 1 reports descriptive statistics on shelters for this main sample. [11]

Table 1: Descriptive Statistics: Shelters

	Mean	Std. Dev.	N	Min	Max
Zero shelters in 1998	0.536	0.499	1344	0	1.0
Minimum number of shelters	0.632	1.075	1344	0	12.0
Maximum number of shelters	1.885	2.342	1344	0	20.0
Ever had Zero Shelters	0.619	0.486	1344	0	1.0
Ever had Any Shelters	0.670	0.470	1344	0	1.0
Ever Change Whether County has Any Shelters	0.289	0.454	1344	0	1.0

Sample includes all counties whose population is less than or equal to that of the largest county that begins the panel with zero shelters in 1998. Observations are at the county level. Appendix Table 18 presents the same measures for the portion of this sample that starts with zero shelters, and Appendix Table 19 presents the same for the portion that starts with at least one shelter.

Outcome data come from the FBI Uniform Crime Reporting Program's Supplementary Homicide Reports (Kaplan, 2021). Domestic violence is underreported and victims may self-select into reporting based on unobserved factors such as cultural values, religious beliefs, trust in police, or fear of retaliation from the abuser; using intimate partner homicides as an outcome measure mitigates this potential selection bias, since virtually all homicides are reported to the authorities. The Supplementary Homicide Reports have the unique advantage of reporting the relationship between the victim and the offender(s), allowing the direct identification of domestic violence and intimate partner homicides. This relationship, however, is only reported for the first victim listed. Because most domestic violence homicides are intimate partner homicides, I limit the scope of the main analysis to homicides where any offender is a current or former intimate partner of the victim.[12]

The Uniform Crime Reports are a voluntary program for police agencies. While most agencies participate in the Uniform Crime Reports at some point during the analysis period from 1998 to 2016, many report to the UCR inconsistently. Because the Supplementary Homicide Reports are incident-level data, it is not possible to directly distinguish between no homicides and no reporting. However, since the Supplementary Homicide Reports are part of the broader Uniform Crime Reporting program, I assume an agency reports to the Supplementary Homicide Reports in a given year if it appears in the other UCR datasets at least 10 months out of the year, since there are probably no true zeroes on all other types of crime in the UCR. The sample is limited to a balanced panel of agencies that report to the UCR consistently

[11] Appendix Tables 18 and 19 report the same for the portions of the main sample that start the panel with no shelters and with at least one shelter, respectively. Approximately 29% of counties in the main sample experience at least one change on the extensive margin of shelter availability.

[12] Section 4.2 presents results for homicides where the relationship between the victim and perpetrator is unknown.

from 1998-2016, and because most consistently reporting agencies are local police departments, other types of agencies are dropped.[13] This means that in most cases, the population served by the reporting police agency is that of a single city.

In addition to intimate partner homicide rates, I use the UCR Supplementary Homicide Reports to construct rates of homicides with unknown victim-offender relationship and of of other homicides where the relationship between the victim and the offender is known and is not intimate in nature.[14] Relationships identified in the Supplementary Homicide Reports are those known to police at the time the homicide is recorded. Homicides do not need to result in a conviction of the listed offender for the relationship to appear in the Supplementary Homicide Reports.

I construct overall intimate partner homicide rates, gender-specific intimate partner homicide rates, and other homicide rates as $HomRate_{at} = \frac{Homicides_{at}}{(\frac{AgencyPopulation_{at}}{100,000})}$, using the total population served by the reporting police agency regardless of whether the rate is gender-specific[15].

Although the number of shelters and most control variables are observed at the county level, homicides are observed at the reporting agency level, so the unit of observation will be the agency-year. Identifying the effect using within-agency variation controls for variation in policing and police data reporting behavior between agencies within the same county, such as that arising from agencies that have different rates of identifying an offender in the Supplementary Homicide Reports.

Table 2 shows descriptive statistics on the rate of intimate partner homicides for the main sample. Most intimate partner homicides have female victims, and about 7% of agencies report at least one intimate partner homicide each year. [16]

Additional county-level controls come from SEER county-level population estimates and Local Area Unemployment Statistics. Descriptive statistics for county-level demographics are reported in Appendix Table 20 for the main sample. [17] The main sample includes local police departments located counties

[13] Other types of agencies include, for example, university police departments and county sheriff's departments. Very few of these types of agencies would have been included in the balanced panel, and the populations they serve may have overlapped with those of local police departments in the panel.

[14] Includes homicides committed by strangers, friends, co-workers, neighbors, parents, step-parents, siblings, or other family members besides partners and children. Results for other homicides, decomposed by relationship, are presented in Section 4.2

[15] Although gender balance is available at the county level, it is not available at the agency level. Therefore, at the agency level, all homicide rates use the same denominator and $TotalIPVHomRate_{at} = FemaleIPVHomRate_{at} + MaleIPVHomRate_{at}$.

[16] Table 16 in the Appendix shows the same measures for counties that begin the panel with no shelters, and Appendix Table 17 for those that begin with at least one shelter. Those that begin with at least one shelter are on average more populated and have a slightly lower intimate partner homicide rate.

[17] The same measures are reported in Appendix Table 21 for counties that start with no shelters, and in Appendix Table 22 for counties that start with at least one shelter.

Table 2: Descriptive Statistics: Homicides, Main Small-County Sample

	Mean	Std. Dev.	N	Min	Max
Intimate Partner Homicides/100,000 People	0.473	3.674	56297	0	237.5
Female Intimate Partner Homicides/100,000 People	0.362	3.151	56297	0	237.5
Male Intimate Partner Homicides/100,000 People	0.111	1.892	56297	0	193.4
Other Homicides/100,000 People	0.659	4.266	56297	0	406.5
Any Intimate Partner Homicides (Agency-Year)	0.070	0.256	56297	0	1.0
Any Female Intimate Partner Homicides	0.058	0.233	56297	0	1.0
Any Male Intimate Partner Homicides	0.020	0.139	56297	0	1.0
Population served by reporting police agency	18331.658	35375.861	56297	63	453017.0

Sample includes all counties whose population is less than or equal to that of the largest county that begins the panel with zero shelters in 1998. Observations are at the agency-year level, and populations used in homicide rates are the total population served by the reporting agency according to the UCR. Appendix Table 16 presents the same measures for the portion of this sample that starts with zero shelters, and Appendix Table 17 presents the same for the portion that starts with at least one shelter.

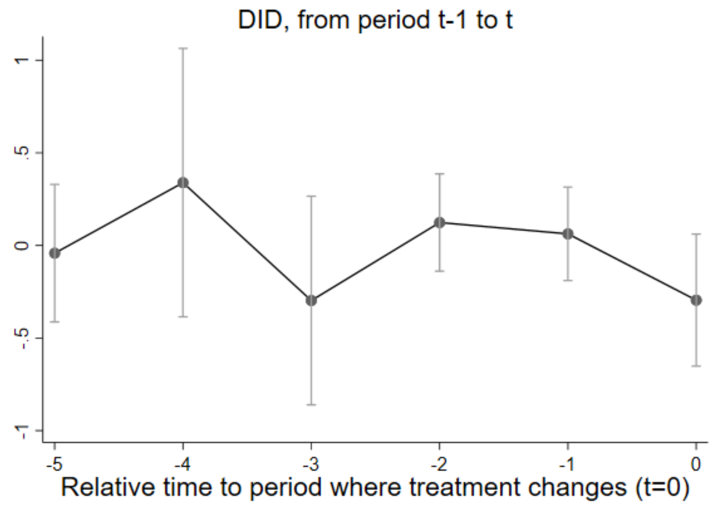
with populations up to 943,742 people. This means police departments in the sample serve cities of up to 453,017 people. As noted in Section 4.3, results are not sensitive to this threshold.

3 Methodology

This paper employs a difference-in-differences research design, exploiting variation that comes from counties that previously had no shelters opening their first shelter and counties that previously had at least one shelter closing their last shelter. The fundamental comparisons of interest in this design are 1) changes in intimate partner homicide rates in counties with a shelter opening versus those that continue to have no shelters and 2) changes in intimate partner homicide rates in counties with a total closure of all shelters versus those that continue to have at least one shelter. For estimates arising from these comparisons to yield the causal effect of shelter availability on intimate partner homicides, I must assume that trends in intimate partner homicides in counties with and without shelter openings and closings would have been similar in the absence of those changes. Results in Figure 5 and Figure 6 in Section 4.1 show estimated placebo effects leading up to shelter openings and closings, respectively. These placebo effects show that there is little evidence of a clear difference in trends prior to shelter openings. Placebo effects leading up to shelter closings are a bit more noisy and show more of a difference in trends, but, as noted throughout Section 4, results appear to be driven primarily by openings.

Figure 2 shows placebo effects in the years leading up to openings and closings, with little evidence of pre-treatment differences in trends on average between counties with openings/closings and comparison counties. On

Figure 2: Pre-Treatment Placebo Effects: DID-M Estimator, Female Intimate Partner Homicide Rate



Notes: Outcome variable is the female intimate partner (IPV) homicide rate, the number of intimate partner homicides with female victims per 100,000 people served by the reporting police agency per year. Time variable is years relative to the change in whether the county has any shelters. Sample includes all counties whose population is less than or equal to that of the largest county that begins the panel with zero shelters in 1998. The level of observation is the agency-year.

the whole, pre-trends appear to be noisy but similar in counties with shelter openings to those in counties with no openings or no openings yet; this suggests that, for the equal counterfactual trends assumption to be violated, some other shock would have had to systematically change the trends in intimate partner homicide rates in counties with shelter openings and not in those without, or vice versa. [18] One potential threat to identification would be if counties endogenously opened shelters in response to recent spikes in intimate partner homicide rates, but these rates do not appear to increase in the years immediately prior to a shelter opening.

Recently, a growing literature has documented the heterogeneity bias and weighting problems present in the two-way fixed effects (TWFE) and first differences (FD) estimators commonly used in difference-in-differences designs with more than two time periods. This paper uses state-of-the-art difference-in-differences estimators developed by De Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020) and compares them with the conventional TWFE and FD estimators.

The "DID-M" estimator by De Chaisemartin and d'Haultfoeuille (2020) is most readily suited to this setting, as it allows for a county' shelter availability

[18] This assumption is more questionable for analyses relying exclusively on shelter closings; this concern will be discussed in more detail in Section 4.1.

to change more than once and in both directions. County Business Patterns data measure the number of shelters per county from 1998-2016, and over that time period, many counties experience both an opening of their first shelter and a closing of their last one. This estimator computes effects based on first differences for "joiners" (counties in the first year a shelter opens) and "leavers" (counties that previously had a shelter in their first year without one) and computes an average that weights all changes in treatment status equally. In a given period t, when no controls are used, the average DID-M treatment effect for openings (where $Shelt_{a,t}$ takes a value of 0 for local police departments that have a shelter in their county and a value of 1 for those who do not) is:

$$
\begin{aligned}
&ATEOpen_t = \\
&\quad \sum_{a:Shelt_{a,t}=1,Shelt_{a,t-1}=0} \frac{N_{a,t}}{NOpen_t}(IPVHomRate_{a,t} - IPVHomRate_{a,t-1}) \\
&- \sum_{a:Shelt_{a,t}=0,Shelt_{a,t-1}=0} \frac{N_{a,t}}{NStayatZero_t}(IPVHomRate_{a,t} - IPVHomRate_{a,t-1}),
\end{aligned}
\tag{1}
$$

or the difference in the changes intimate partner homicide rates from $t-1$ to t between counties that open a shelter in year t and counties that have no shelters in year $t-1$ or year t. This component is the effect of opening a new shelter where there was none in year t. The average treatment effect for closings in a given period t is:

$$
\begin{aligned}
&ATEStayOpen_t = \\
&\quad \sum_{a:Shelt_{a,t}=1,Shelt_{a,t-1}=1} \frac{N_{a,t}}{NStayOpen_t}(IPVHomRate_{a,t} - IPVHomRate_{a,t-1}) \\
&- \sum_{a:Shelt_{a,t}=0,Shelt_{a,t-1}=1} \frac{N_{a,t}}{NClose_t}(IPVHomRate_{a,t} - IPVHomRate_{a,t-1}),
\end{aligned}
\tag{2}
$$

or the difference in the changes in intimate partner homicide rates from $t-1$ to t between counties that have at least one shelter in both year $t-1$ and year t and counties that experience a total closure of all shelters in year t. This is the effect of at least one shelter continuing to remain open as opposed to all local shelters closing in year t.

This estimator then estimates an overall average treatment effect by simply taking the differences-in-differences for openings ($ATEOpen_t$) and closings ($ATEStayOpen_t$) for each year t after the first year and averaging them together as follows:

$$DID-M = \sum_{t=1999}^{2016} \left(\frac{NOpenings_t}{NChanges_t} ATEOpen_t + \frac{NClosings_t}{NChanges_t} ATEStayOpen_t\right), \tag{3}$$

essentially taking the average effect estimated using openings and closings in each year and then averaging those annual effects into an overall treatment effect parameter. The preferred specification includes controls for the percentage of the county's population in their twenties, thirties, and forties, county-level percentage Black and Hispanic, county-level unemployment rates, and an agency-level linear trend; in this case, the estimator first regresses the outcome on the controls and then uses the residual intimate partner homicide rate, rather than the simple intimate partner homicide rate, in the calculations in equations 1 and 2. Agency-level linear trends are particularly important, as intimate partner homicide rates for many agencies in the sample take the form of one or two homicides every few years, with the majority of agency-year observations having no intimate partner homicides. Thus, because this estimation relies on first differences, it is important to allow for an agency-level trend to account for recent fluctuations in the agency's intimate partner homicide rate since it is a relatively rare outcome. Standard errors are clustered at the county level.[19]

The DID-M estimates are robust to heterogeneity across counties in ways that the traditional first-differences estimator is not, and do not suffer from the same weighting issues that can change the magnitude of the estimated first differences parameter $\hat{\beta}_{FD}$, or even cause it to be the opposite sign of all the average treatment effects of which it is comprised. However, if effects are asymmetric for openings and closings or if they grow over time, this estimator could suffer from attenuation bias.

To mitigate this concern, I also estimate the effects of openings and closings separately using the Callaway and Sant'Anna (2020) estimator, developed for staggered implementation designs. Results using this estimator will split the main small-county sample into two sub-samples: a staggered openings sample made up of counties within the main sample that start with zero shelters in

[19] The DID-M estimator requires that standard errors be bootstrapped. All standard errors for this estimator use 500 replications unless otherwise specified.

1998, and a staggered closings sample made up of counties that start with at least one shelter in 1998. In addition to being necessary for the use of the Callaway and Sant'Anna (2020) estimator, these sub-samples are informative in their own right due to the fact that the path of treatment status prior to 1998 is not known. This estimator does not accommodate post-treatment covariates, so no controls are used. This estimator estimates group-specific average treatment effects on the treated, denoted as $ATT_{g,t}$ for groups of counties treated in year g, estimated in year t. These group-time average treatment effects are numerically identified in a manner similar to $ATEOpen_t$ and $ATEClose_t$ in Equations 1 and 2, but include long differences in all periods after the treatment occurs in addition to the first-differences that the DID-M estimator uses to compute the instantaneous effect. I use both never-treated and not-yet-treated counties as comparison units in computing the $ATT_{g,t}$.

I aggregate all of the $ATT_{g,t}$ obtained by the Callaway and Sant'Anna (2020) estimator in two ways: in a balanced event study showing the evolution of dynamic effects over time among groups treated for at least 5 periods before the end of the panel, and in a simple summary parameter that gives a point estimate for the overall effect. The event study aggregation takes the estimated $ATT_{g,t}$ for each group treated in year g measured in year t and aggregates them such that the time-specific effect e periods after the treatment, $\theta_{es}(e)$, is:

$$\theta_{ES}(e) = \sum_{g:g+5\leq 2016} \frac{N(g,t) : G = g}{N(g,t)} ATT_{g,t=g+e} \tag{4}$$

where $N(g,t)$ is the number of group-time pairs for which there is an estimated $ATT_{g,t}$.

The overall treatment effect parameter θ_O is merely a group-size-weighted average of the group effects ATT_g, which are themselves simple averages of the group-time effects $ATT_{g,t}$:

$$\theta_O = \sum_{g=1999}^{2016} \frac{N(a : G = g)}{N(a)} \sum_{t=1999}^{2016} \frac{1}{N(t)} ATT_{g,t} \tag{5}$$

where $N(t)$ is the number of time periods for which there are estimated $ATT_{g,t}$ and $N(a)$ is the number of units (in this case, reporting police agencies in the UCR).

Although the actual path of shelter openings and closings is not exactly a staggered implementation design since counties can have a shelter open and then close, or have all shelters close and then have a new one open again, the use of this estimator allows for long-term treatment effects. Allowing for effects after the first year of treatment may be important for two reasons. First, it allows time for the survivor to have left an abusive relationship before the violence would have escalated to the level of a homicide. Second, in the sample of small counties that start off the panel with no shelters, IPV homicides are a rare outcome - only about 4% of city-year observations have at least one intimate partner homicide, so it may take a few years for a homicide that otherwise would have occurred to be prevented. The limitation of this approach is that once a shelter opens (closes), I am forced to assume that it remains open (closed) for the rest of the panel, regardless of true changes in treatment status. If anything, this limitation should bias effects toward zero. I also present results dropping counties where this assumption does not hold, but this requires selecting the sample based on the path of future treatment status, which may be endogenous.

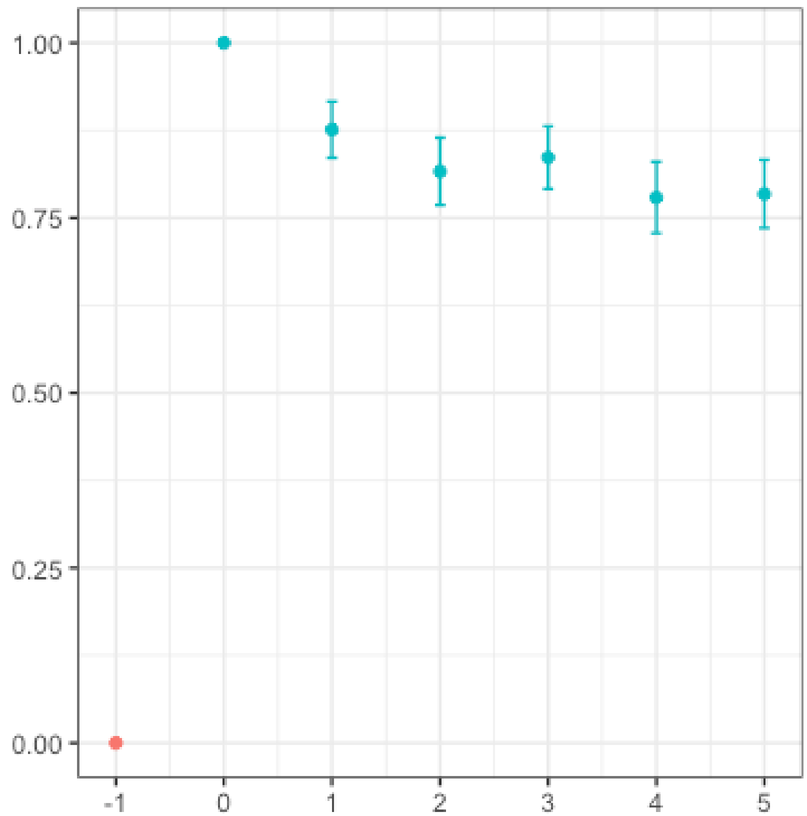
Although treatment status is not observed prior to 1998, pre-trends are estimated using homicide data starting in 1993. Although a county can open its first shelter or close its last one and have further changes in treatment status later on in the panel, Figure 3 indicates that about 93% of shelter openings persist beyond the first year and 80% of counties that open their first shelter continue to have at least one shelter for at least the next five years.

Appendix Figure 8 shows an event study in the number of shelters in a county after the first opening, and point estimates greater than one in years 2-5 suggest that, on average, counties tend to open more shelters after opening the first one.

The sample for analysis of shelter openings using this estimator includes all counties that begin the panel with at least one shelter in 1998. However, the analysis for closings using this estimator is less straightforward. As noted in Appendix Figure 9 showing the time path of a county's total number of shelters relative to the year of the first total closure of all shelters, some counties begin closing shelters a few years prior to closing their last one. For this reason, estimates for closings using this estimator allow for two years of possible anticipatory effects. Additionally, Figure 4 indicates that only about 75% of total closures of all shelters last more than one year (as opposed to 93% of openings), and only about 60% of total closures (as opposed to 80% of openings) persist for at least the next five years

Because total shelter closures are far less "sticky" than shelter openings, and the staggered implementation estimator estimates long term effects, these effects may be biased downwards. In fact, in Section 4.1, I find negative effects for both openings and closings, although the effects for closings are much noisier and not statistically significant. When dropping counties that open a shelter and then later have a total closure, results for openings remain

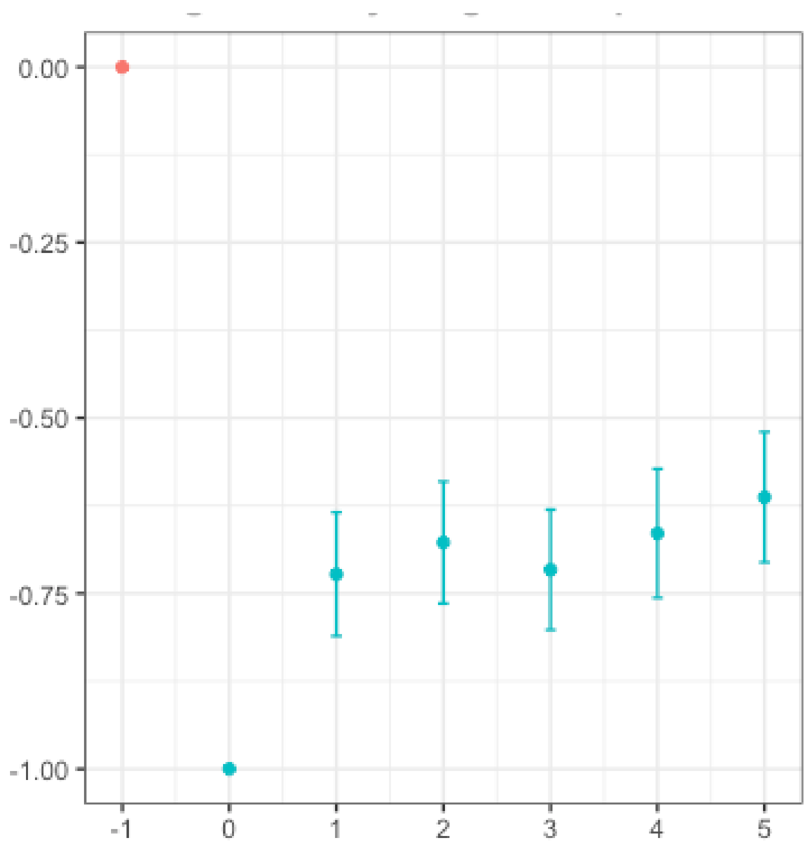
Figure 3: Event Study: Any Shelters After First Opening



Notes: Outcome variable is an indicator for any shelter in county, time variable is years since first shelter opening. Treatment Sample includes all counties that begin the panel with zero shelters in 1998. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have any shelters.

largely unchanged. However, when dropping counties that have a total closure and then later have an opening, results change drastically, producing a fairly precise null result. This suggests that results relying solely on closings are likely quite unreliable due to the high rate of reversals in treatment status overpowering any true effect.

Figure 4: Event Study: Any Shelters After First Total Closure



Notes: Outcome variable is number of shelters in county, time variable is years since first total closure. Treatment Sample includes all counties that begin the panel with at least one shelter in 1998 and have at least 5 years of follow-up after the first total closure. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have zero shelters.

Lastly, I compare the results of these estimators with the problematic but familiar two-way fixed effects and first differences estimators. Work by Goodman-Bacon (2021), Baker et al. (2021), Jakiela (2021), and others establishes the properties of the two-way fixed effects estimator both within and outside of staggered implementation settings. Although the two-way fixed effects estimator nearly always suffers from negative weighting problems, the two-way fixed effects treatment parameter $\hat{\beta}_{TWFE}$ is less biased when treatment effects do not grow over time, or, in other words, when $\hat{\beta}_{TWFE}$ and $\hat{\beta}_{FD}$ are similar to each other. The staggered implementation estimator produces little evidence of within-unit treatment effect heterogeneity over time, as noted in Figure 5 in Section 4.1. Since De Chaisemartin and d'Haultfoeuille (2020) estimates rely primarily on first differences but allow treatment status to change multiple times and in both directions, and Callaway and Sant'Anna (2020) estimates include both first differences and long differences but allow treatment status to change only once, I report TWFE estimates from the following regression despite their problems in an attempt to capture all the useful variation in the panel,

$$IPVHomRate_{act} = \beta_0 + \beta_{TWFE} AnyShelt_{ct} + X_{ct}\gamma + \alpha_a + \alpha_t + \epsilon_{act} \quad (6)$$

where X_{ct} includes county-level age, race, ethnicity and unemployment controls. The FD estimates from the following regression are also reported,

$$\Delta IPVHomRate_{act} = \beta_0 + \beta_{FD}\Delta AnyShelt_{ct} + \Delta X_{ct}\gamma + \alpha_t + \epsilon_{act} \quad (7)$$

where $\Delta IPVHomRate_{act}$, $\Delta AnyShelt_{ct}$, and ΔX_{ct} (which includes the same controls as the TWFE specification) are differences between years t and $t-1$. These estimates provide a useful comparison with the TWFE estimates as a heuristic for within-unit treatment effect heterogeneity over time, which is often the largest source of bias in the TWFE estimator. The FD estimator is also the closest OLS relative of the DID-M estimator, and produces similar point estimates in this case.

4 Results

4.1 Main Results: Intimate Partner Homicides

Results in Columns (1) and (2) of Table 3 imply that the existence of a shelter prevents one female intimate partner homicide per 100,000 people every three to four years, about a 68% reduction. Results in Columns (3) and (4) show no effect for men, suggesting the point estimates for overall intimate partner homicide rates in Table 4 are driven entirely by intimate partner homicides with female victims. [20] This is unsurprising for a few reasons: female intimate partner homicide rates in the sample are about 3.5 times those of men, and domestic violence shelters are often marketed as "women's shelters" even if they also serve men[21]. Furthermore, work by Campbell et al. (2007) notes that

[20] Recall that gender-specific intimate partner homicide rates each use the total population served by the local police department; they therefore add to the overall intimate partner homicide rate and it is unsurprising that the point estimate for overall intimate partner homicide rates is nearly identical to that for women.

[21] The NAICS code used to identify shelters in the County Business Patterns data includes both domestic violence shelters and general homeless shelters. Although homeless shelters may serve as an imperfect substitute when there are no local domestic violence shelters or they are full, there are key differences: homeless shelters do have confidential locations, and often do not provide comfortable accomodations for children. For these reasons, it's likely that most domestic violence survivors would prefer domestic violence shelters, which cater primarily to women.

the majority of intimate partner homicides with both female and male victims are preceded by intimate partner violence against the female partner. Thus, in the months leading up to a potential homicide, it's likely that the woman is more likely to flee and seek a safe place to stay outside of the relationship; in other words, she has more reason to take protective measures that may save her life. [22] Figure 2 shows pre-treatment placebo effects for the specification in Column (2) of Table 3, and none are significant at the 10% level. This suggests that the equal counterfactual trends assumption should be satisfied provided there were no shocks systematically affecting intimate partner homicides in either treated counties or comparison counties but not both.

Table 3: DID-M Estimator, Effect of Openings and Closings by Victim Gender

	(1) IPV Homicide Rate (Women)	(2) IPV Homicide Rate (Women)	(3) IPV Homicide Rate (Men)	(4) IPV Homicide Rate) (Men)
Any shelters in county	-0.2969+	-0.2954+	0.0185	0.0232
	(0.1726)	(0.1739)	(0.0554)	(0.0595)
County-level age distribution controls	No	Yes	No	Yes
County-level race/ethnicity controls	No	Yes	No	Yes
County-level unemployment rate	No	Yes	No	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes
Mean	0.362	0.362	0.111	0.111
Agencies	2693	2693	2693	2693
Counties	1344	1344	1344	1344

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides with female (male) victims per 100,000 people served by the reporting police agency per year. Sample includes all counties with population less than or equal to that of the largest county that begins the panel with zero shelters in 1998. The level of observation is the agency-year. Results weighted by population are presented in Appendix Table 26 and pre-trends for the specification shown in column (2)are presented in Figure 2.

Table 4 shows results obtained using the DID-M estimator, which relies on first differences in whether a county has any shelters and in the intimate partner homicide rate (De Chaisemartin and d'Haultfoeuille, 2020). Results in Column (2), including the full set of controls and a linear trend for the police agency reporting homicides to the UCR, suggest an average treatment effect on treated counties of -.27 intimate partner homicides per 100,000 people in the first year a county has a shelter (or +27 per 100,000 in the first year it does not have a shelter). This effect is noisily estimated and not statistically significant; in magnitude, it translates to about a 65% decrease in a city's intimate partner homicide rate when it has a shelter in its county. This is equivalent a reduction on the order of one intimate partner homicide per 100,000 people every four years.

[22] More work is needed on intimate partner homicides and intimate partner violence in same-sex relationships. Same-sex IPV homicides are included in the analysis and disaggregated by victim gender in the same way as those in heterosexual relationships, as there is insufficient statistical power to evaluate same-sex IPV homicides separately.

Table 4: DID-M Estimator, Effect of Openings and Closings

	(1) IPV Homicide Rate (All)	(2) IPV Homicide Rate (All)	(3) IPV Homicide Rate (All)
Any shelters in county	-0.2783	-0.2720	-0.1637
	(0.1987)	(0.1947)	(0.1609)
County-level age distribution controls	No	Yes	Yes
County-level race/ethnicity controls	No	Yes	Yes
County-level unemployment rate	No	Yes	Yes
Agency-level linear trend	Yes	Yes	None
Mean	0.473	0.473	0.473
Agencies	2693	2693	2693
Counties	1344	1344	1344

Standard errors clustered at the county level

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homcides per 100,000 people served by the reporting police agency per year. Sample includes all counties with population less than or equal to that of the largest county that begins the panel with zero shelters in 1998. The level of observation is the agency-year. Analogous results for counties who begin the panel with zero shelters in 1998 are presented in Appendix Table 25.

Importantly, results from shelter openings and shelter closings may not be symmetric. The DID-M estimator simply averages opening and closing effects together without accounting for this potential asymmetry. To account for this, effects of openings and closings are estimated separately using the staggered difference-in-differences estimator developed by Callaway and Sant'Anna (2020). Aggregated staggered opening results are presented in Table 5 and event study results are presented in Figure 5. Column (2) of Table 5 suggests that the existence of a shelter prevents about .28 female intimate partner homicides per 100,000 people per year (about a 68% reduction, again equating to one homicide per 100,000 people every three to four years), and the effect is again statistically significant at the 10% level. Figure 5 suggests that the effect remains relatively stable over time and that there is little evidence of a clear difference in trends in the years leading up to the shelter openings, suggesting that counties with and without shelters would have likely had similar trends in intimate partner homicides in the absence of the shelter. Column (4) of Table 5 estimates a placebo effect on other homicides where the relationship between the victim and offender is known and is not intimate or domestic in nature, and finds a noisy null effect.

Table 5: Staggered Implementation Estimator, First Opening

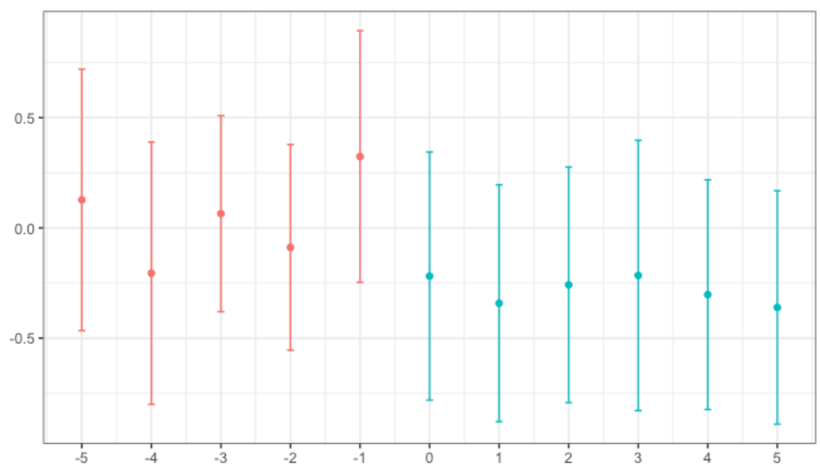
	(1) IPV Homicide Rate (All)	(2) IPV Homicide Rate (Female)	(3) IPV Homicide Rate (Male)	(4) Other Homicide Placebo (All)
Any Shelters in County	-0.2396	-0.2861^{+}	0.0463	-0.1092
	(0.1872)	(0.1651)	(0.0883)	(0.2907)
Mean	0.596	0.419	0.177	0.838
Agencies	1145	1145	1145	1145
Counties	720	720	720	720

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides per 100,000 people served by the reporting police agency per year. Sample includes all counties that begin the panel with zero shelters in 1998. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have any shelters, and remain so for the rest of the panel regardless of actual changes in treatment status. Figure 8 shows the dynamics for the number of shelters after the first shelter opening, which indicate that on average, counties tend to continue opening more shelters after opening the first. The comparison group includes both never-treated and not-yet-treated counties.

Figure 5: First Opening Event Study: Female Intimate Partner Homicide Rate

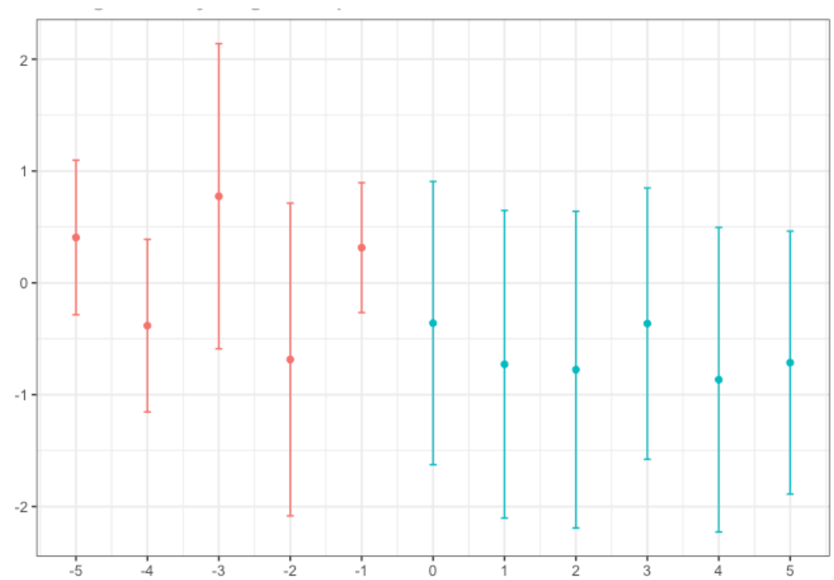


Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides with female victims per 100,000 people served by the reporting police agency per year. Treatment Sample includes all counties that begin the panel with zero shelters in 1998. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have any shelters, and remain so for the rest of the panel regardless of further actual changes in treatment status. Event study is balanced among units that have at least 5 years of follow-up after their first shelter opening.

Results for total shelter closures are presented in Figure 6 and Table 6. A county is considered to have experienced a total closure the first time it moves from having at least one shelter to having no shelters. Appendix Figure 9 shows the dynamics of the number of shelters leading up to and following the first total closure. Counties appear to close shelters in the years leading up to the first total closure of all shelters, and some appear to bounce back and open shelters again after a total closure. To account for other closings leading up to a total closure, these effects are estimated with an allowance for up to two years of anticipatory effects.

Figure 6 shows an event study of female intimate partner homicide rates

Figure 6: First Total Closure Event Study: Female Intimate Partner Homicide Rate

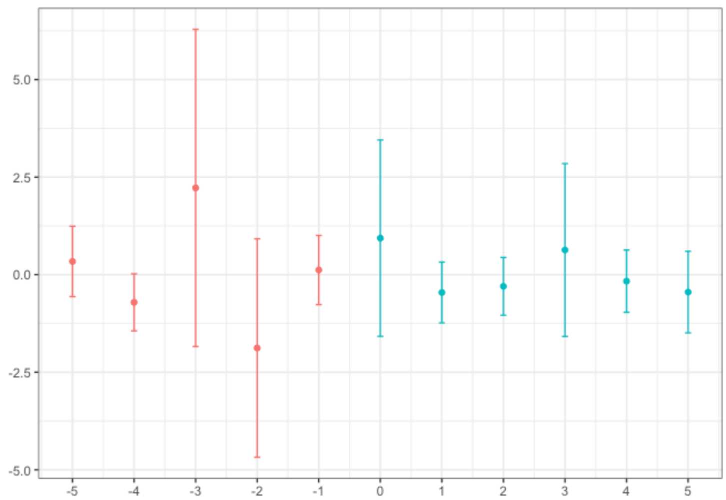


Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides with female victims per 100,000 people served by the reporting police agency per year. Sample includes all counties that begin the panel with at least one shelter in 1998. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have zero, and remain so for the rest of the panel regardless of further actual changes in treatment status. Event study is balanced among units that have at least 5 years of follow-up after their first total closure of all shelters.

after the first total closure, and Columns (1) and (2) of Table 6 show aggregated effects on counties experiencing a total closure. Pre-closure placebo effects are noisier than those for openings, and dynamics in the number of shelters indicate that total closures may be a less "sticky" change in treatment status than openings, resulting in the possibility that treatment effects may be biased downward by future shelter openings. Furthermore, as noted in Equations 4 and 5, effects are estimated at the group level, and the Callaway and Sant'Anna (2020) R package for this estimator indicated a warning that some of the treatment timing groups may be too small to produce reliable estimates. These issues indicate that results relying solely on closings should be interpreted with caution. This is of more concern for the use of the staggered implementation estimator than for the DID-M estimator, as the DID-M estimator estimates instantaneous or "on-impact" effects.

Nonetheless, the large negative point estimates are puzzling. These estimates appear to be driven by subsequent shelter openings after the first total closure. Figure 7 and Column (3) of Table 6 present estimates for the effect of total closures, limiting the sample to counties that have a total closure of all shelters and no subsequent shelter openings in the treatment group and counties that never have or have not yet had a total closure in the comparison group. These results, though they still have noisy pre-trends and even smaller treatment timing groups, yield a fairly precise null result, so it is unsurprising that including counties that experience subsequent shelter openings produces spurious negative point estimates. In contrast, results in Appendix Figure 10 show that results for openings, dropping counties with subsequent total closures, are qualitatively similar to the main results in Figure 5 and Table 5.

Figure 7: Event Study: Female Intimate Partner Homicide Rates, Permanent Total Closures



Notes: Outcome variable is the female intimate partner (IPV) homicide rate, the number of intimate partner homicides with female victims per 100,000 people served by the reporting police agency per year. Time variable is years relative to the permanent total closure of all shelters in the county. Sample includes all counties whose population is less than or equal to that of the largest county that begins the panel with zero shelters in 1998, who begin the panel with at least one shelter in 1998, and who do not experience a total closure of all shelters and then a subsequent shelter opening. The level of observation is the agency-year.

Table 6: Staggered Implementation Estimator, Total Closure

	(1) IPV Homicide Rate (All) Any Closure	(2) IPV Homicide Rate (Female) Any Closure	(3) IPV Homicide Rate (Female) Permanent Closure
Any Shelters in County	-0.6563	-0.5819	0.0032
	(0.5426)	(0.5522)	0.257
Mean	0.487	0.329	0.376
Agencies	1818	1818	1503
Counties	624	624	540

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides per 100,000 people served by the reporting police agency per year. Sample includes all counties that begin the panel with zero shelters in 1998. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they do not have any shelters, and in Columns (1) and (2) they remain so for the rest of the panel regardless of actual changes in treatment status. Appendix Figure 9 shows the dynamics for the number of shelters after the first shelter opening, which indicate that some counties may open shelters again after a total closure. The comparison group includes both never-treated and not-yet-treated counties.

Table 7 presents results using two-way-fixed effects and first-differences estimators. Columns (1) and (2) present results for the same sample used in the DID-M estimation presented in Tables 4 and 3. Results from the first differences estimator in Column (2) are similar to those produced by the DID-M estimator. Columns (3) and (4) present results for the same sample as the staggered openings analysis in Table 5 and Figure 5, meaning that they rely primarily on variation from shelter openings, and are qualitatively similar to those produced by the other estimators. Columns (5) and (6) present results using the same sample as the staggered closings analysis in Appendix Table 6 and Figures 6. Point estimates are uniformly negative in Columns (1) through (4), corresponding to the samples used in the DID-M and staggered openings analysis. The TWFE coefficient in Column (4) is statistically significant at the 10% level, but is smaller than those produced by the other estimators; it appears to be attenuated by the weighting issues in the TWFE estimator despite little evidence of within-unit treatment effect heterogeneity over time. However, since openings appear to have more of an effect than closings, the TWFE estimates may be attenuated by the heterogeneity bias and weighting issues documented in Goodman-Bacon (2021) and elsewhere. The TWFE and FD estimates in Columns (5) and (6) produce opposite-signed noisy null effects, again suggesting no effect in a sample where the identifying variation comes primarily from closings. However, in general, results for these estimators should be interpreted with caution and are provided primarily for completeness. Further TWFE and FD estimates are provided in the Appendix.

Table 7: TWFE vs. FD: Female Intimate Partner Homicide Rate

	(1) Small Counties	(2) Small Counties	(3) Start at 0	(4) Start at 0	(5) Start at >0	(6) Start at > 0
Any Shelters in County	-0.0733		-0.164^{+}		0.136	
	(0.0628)		(0.0842)		(0.0892)	
First Difference: Any Shelters in County		-0.257		-0.346		-0.144
		(0.164)		(0.238)		(0.189)
Observations	56283	53313	21755	20610	34528	32703
Mean	0.362	-0.00743	0.414	-0.0101	0.329	-0.00577
Agencies	2963	2963	1145	1145	1818	1818
Counties	1344	1344	720	720	624	624

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Outcome is intimate partner (IPV) homicide rates: the number of intimate partner homicides with female victims per 100,000 people served by the reporting police agency per year. "Small Counties" sample in columns (1) and (2) includes all counties that begin the panel with zero shelters in 1998. Columns (3) and (4) include all counties within the "Small Counties" sample that start with zero shelters in 1998 and columns (5) and (6) include all counties within the "Small Counties" sample that start with at least one shelter at the beginning of the panel in 1998. All specifications include year fixed effects, and columns (1), (3), and (5) include agency fixed effects. All specifications include controls for county-level age and race/ethnicity demographics, county-level unemployment rates, and agency-level rates of other homicides where the relationship between the victim and the perpetrator is known and is not intimate or domestic in nature. Appendix Tables 29 and 30 show TWFE and FD results for overall intimate partner homicide rates and male intimate partner homicide rates. Appendix Table 28 shows TWFE and FD results weighted by population for female intimate partner homicide rates.

Overall, the results presented here suggest that the opening of a new shelter can reduce intimate partner homicide rates by approximately one homicide per 100,000 people every four years, about a 65% effect, concentrated almost entirely among female victims. In interpreting this effect size, it is worth noting that intimate partner homicide rates are a rare outcome, so any detectable effect is likely to be quite large in magnitude. Furthermore, as noted in De Chaisemartin and d'Haultfoeuille (2020), these new difference-in-differences estimators are far less efficient than the TWFE estimator they are intended to replace. It may be prudent to account for efficiency differences between these estimators and the traditional TWFE estimator when interpreting results of marginal significance; the standard errors produced by the DID-M and staggered implementation estimators for female intimate partner homicides are about twice the size of those produced by the TWFE estimator.

It is not obvious ex ante why the effects for openings and closings should be so asymmetric. One possible explanation is that the availability of a shelter gives a domestic violence survivor the courage and support to leave a relationship for the first time, and though she may eventually return, she may be emboldened to leave again in the future if she has left before. Another candidate mechanism could be collaborations with local police departments, especially in light of the findings by Koppa (2020) and Messing et al. (2015). If the existence of a shelter and the expertise of its staff facilitate the adoption new practices in the local police department that are more likely to protect domestic violence survivors, these changes could persist after the shelter has closed. Alternatively, the asymmetry could be driven purely by power and measurement issues. There are fewer closings than openings, openings are more persistent than closings, and the path of treatment status after a county's first shelter opening in the panel is likely more accurately measured. Because treatment status is not observed prior to 1998, it is not known whether a shelter that already exists in 1998 (as is the case for all shelters in the staggered closings sample) has just opened in the prior year or has been open for decades, and whether other shelters have recently opened or closed in the same county. Given the asymmetry in the staggered implementation estimates and the fact that the point estimates for closings (though quite noisy) are negative, the fact that the DID-M estimate for openings and closings and the staggered implementation estimate for openings are nearly identical may be a bit puzzling. However, it is worth remembering that the DID-M estimator estimates only the effect on impact when a shelter first opens or closes, whereas the staggered implementation estimator assumes that a county experiencing a total closure of all shelters continues to have no shelters throughout the panel, which is not true given the results in Figure 9. Because of this measurement limitation, the DID-M estimator is far more suited to handle the inclusion of shelter closings than the staggered implementation estimator. Taken together, the results from both estimators suggest that the effect is driven primarily by openings rather than closings and appears fairly stable over time.

4.2 Additional Measures of Homicide

The main analysis relies on police departments to accurately identify the relationship between the victim and the offender in the Supplementary Homicide Reports. In practice, this may be easier said that done. In the main sample, 32% of all homicides with female victims and 45% of female homicides with known offenders are indicated to be intimate partner homicides. Table 8 presents results for additional measures of homicide. "Other homicides" refers to homicides where the relationship between the victim and the offender(s) is known, and none of the offenders have been identified as an intimate partner. This includes friends, neighbors, and other family members, so some of these may be domestic violence homicides; however, because these family members may or may not be members of the victim's household, they are not included in the main analysis, as a domestic violence survivor is likely to flee to a shelter only if the violence is occurring inside their home.

Results in Column (1) of Table 8 suggest that shelters can prevent an additional -.13 female homicides per 100,000 people per year in cases where the relationship between the victim and the perpetrator is not identified in the Supplementary Homicide Reports. This effect is statistically significant at the

Table 8: DID-M: Additional Homicide Measures

	(1) Unknown Offender Homicide Rate (Women)	(2) Unknown Offender Homicide Rate (Men)	(3) Other Homicide Rate (All)	(4) IPV Homicide Rate (Women, Future Shelters)
Any shelters in county	-0.1281+	0.1556	-0.2506	0.2098
	(0.0676)	(0.1435)	(0.1712)	(0.1427)
County-level age distribution controls	Yes	Yes	Yes	Yes
County-level race/ethnicity controls	Yes	Yes	Yes	Yes
County-level unemployment rate	Yes	Yes	Yes	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes
Mean	0.129	0.590	0.700	0.370

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

10% level and equates to the prevention of one homicide per 100,000 people every seven to eight years. This result is informative for a few reasons. If the analysis produced a significant negative effect on intimate partner homicides and a positive effect on homicides with an unknown relationship between the victim and the offender, it would be possible that the results were driven by changes in the propensity of police departments to identify an offender as an intimate partner. The fact that shelters appear to reduce female homicide rates when this relationship is unknown could be explained by non-identification of offenders who are actually partners or by general improvement of safety conditions for women vulnerable to homelessness. It should not be surprising if this relationship is not always correctly identified, as 1) police are not always able to correctly identify homicide offenders in general and 2) there is no relationship category in the Supplementary Homicide Reports for former dating partners. On the other hand, because the sample includes both domestic violence shelters and general homeless shelters, this result is also consistent with general improvements in the safety of women experiencing homelessness. Unfortunately, due to limited information about shelter type, it is difficult to disentangle these mechanisms.

Results in Column (2) suggest no effect on homicide rates for homicides with male victims and unknown perpetrator relationships. Results in Column (3) produce a large but noisily estimated and statistically insignificant point estimate for other homicides where the relationship between the victim and the perpetrator is known and is not a partner, which will be explored further in Table 9. Column (4) presents a placebo test of the effect of future shelters in year $t + 1$ on female intimate partner homicide rates in year t and finds a noisy null effect.

It appears that the large negative point estimate in Column (2) of Table 8 is driven by other homicides with male victims and a non-family perpetrator, as noted in Column (4) of Table 9. Upon further investigation, 80% of homicides in this category are perpetrated by strangers. 33% are at the hands of a civilian and are related to another form of criminal activity, 14% are at the hands of

Table 9: DID-M: Other Homicide Rates by Victim Gender and Family Status

	(1) Other Homicide Rate (Women, Family)	(2) Other Homicide Rate (Women, Non-Family)	(3) Other Homicide Rate (Men, Family)	(4) Other Homicide Rate (Men, Non-Family)
Any shelters in county	0.0271	0.0172	-0.0879	-0.2070
	(0.0399)	(0.0336)	(0.0787)	(0.1367)
County-level age distribution controls	Yes	Yes	Yes	Yes
County-level race/ethnicity controls	Yes	Yes	Yes	Yes
County-level unemployment rate	Yes	Yes	Yes	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes
Mean	0.059	0.075	0.142	0.424

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

police, and 25% are classified as being related to a brawl or argument. Because the County Business Patterns data does not distinguish between domestic violence shelters and general homeless shelters, this large negative estimate may be noise or may be driven by general availability of additional resources for men experiencing homelessness. This is consistent with previous work finding that the opening of a new homeless shelter decreases breaking and entering in the immediate area of the shelter (Faraji et al., 2018). More investigation of the relationship between homeless shelters and criminal activity in general is warranted but beyond the scope of this paper. Columns (1) through (3) of table 9 suggest no effect on rates of female homicide rates perpetrated by any known offenders other than intimate partners, or on rates of male homicides perpetrated by family members other than partners.

4.3 Robustness

Table 10 presents results testing the robustness of the main results in Table 3 to the population threshold for inclusion in the sample. The main sample includes all counties with populations smaller that that of the largest county that begins the panel with zero shelters in 1998. However, results appear to be robust to limiting the sample to 80% 90%, 110%, or 120% of that population. Point estimates are nearly identical for all thresholds.

Table 10: DID-M: Robustness to Population Threshold

	(1) Main Sample IPV Homicide Rate (Women)	(2) 80% of Main Threshold IPV Homicide Rate (Women)	(3) 90% of Main Threshold IPV Homicide Rate (Women)	(4) 110% of Main Threshold IPV Homicide Rate (Women)	(5) 120% of Main Threshold IPV Homicide Rate (Women)
Any shelters in county	-0.2954+	-0.2719	-0.2480	-0.2950	-0.2948+
	(0.1726)	(0.1873)	(0.1780)	(0.1819)	(0.1729)
County-level age distribution controls	Yes	Yes	Yes	Yes	Yes
County-level race/ethnicity controls	Yes	Yes	Yes	Yes	Yes
County-level unemployment rate	Yes	Yes	Yes	Yes	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, $^{***}p < 0.001$

Table 11 presents results dropping counties that at some point in the panel appear to only have a shelter for one year (a transitory opening) or only have a total closure of shelters for one year (a transitory closing). This robustness test is important, as the County Business Patterns data come from tax filings. The appearance or disappearance of a shelter for a single year may be a result of a tax filing error in which an establishment files its taxes after the deadline or an industry code is erroneously changed. However, restricting the main sample to these counties would run the risk of conditioning on the future path of treatment status, which may be endogenous to previous shelter openings or closings. Results appear to be robust to dropping these counties.

Table 11: DID-M: Dropping Transitory Openings and Closings

	(1) Main Results IPV Homicide Rate (Women)	(2) Drop Transitory Openings IPV Homicide Rate (Women)	(3) Drop Transitory Closings IPV Homicide Rate (Women)	(4) Drop Both IPV Homicide Rate (Women)
Any shelters in county	-0.2954+	-0.3092+	-0.3087	-0.3838
	(0.1726)	(0.1869)	(0.2035)	(0.2467)
County-level age distribution controls	Yes	Yes	Yes	Yes
County-level race/ethnicity controls	Yes	Yes	Yes	Yes
County-level unemployment rate	Yes	Yes	Yes	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, $^{***}p < 0.001$

Results analogous to Table 3 with the sample limited to counties that begin with zero shelters at the beginning of the panel in 1998 are presented in Appendix Table 25 are nearly identical in magnitude but more noisily estimated due to a smaller sample size. Appendix Table 26 presents results for the main sample weighted by population, and Appendix Table 27 presents results using a binary outcome for whether there are any intimate partner homicides at all; in both cases, point estimates for female victims are small and noisily estimated but uniformly negative. The fact that point estimates decrease in magnitude in response to weighting by population suggests that results may be driven by smaller jurisdictions; while weighting is meant to improve efficiency and correct for heterogeneity in OLS models (Solon et al., 2015), it did not improve efficiency using the DID-M estimator, and the DID-M estimator is already robust to heterogeneity in ways that OLS difference-in-differences estimators are not.

4.4 Heterogeneity

Appendix Tables 31 and 32 present results on the effects of shelter availability on female intimate partner homicide rates disaggregated by victim race and marital status between the victim and the offender. Appendix Table 31 suggests that, in percentage terms, estimates are larger for white women and Hispanic women than for Black women. However, the sample does not include large urban counties, so it may not be representative of each racial group. Appendix Table 32 does not suggest a meaningful difference in the effects on intimate partner homicides where the victim is currently married to the perpetrator versus other types of relationships such as ex-spouses or dating partners. In general, intimate partner homicides are a relatively rare outcome and are even more rare when disaggregated by race and marital status, so these results should be interpreted with caution due to limited statistical power.

4.5 Secondary Outcomes

Because effects of shelters on intimate partner homicides may operate through permanent dissolution of long-term abusive relationships, it would be interesting to know whether shelters increase divorce rates by ending bad marriages or decrease marriage rates by ending bad relationships before they become marriages. Evidence from the introduction of unilateral divorce laws suggests that allowing women to more freely leave bad marriages reduces domestic violence (Stevenson and Wolfers, 2006), so it would be reasonable to believe divorce and marriage could be a mechanism for the effects of shelters on intimate partner homicides. County-level marriage and divorce counts aggregated by researchers at Bowling Green State University are available for 2000 and 2010 but not for other years in the panel, and are used in a long-differences model to estimate the effect of extensive margin changes on marriage and divorce rates (BGSU, 2016). Descriptive statistics for these data are reported in Appendix Table 24. For congruity, analysis of marriages and divorces is limited to counties who have at least one agency reporting to the UCR and are therefore included in the main sample. Results of this analysis are inconclusive. Appendix Tables 33 and 34 present results from long differences models estimating the effect of shelters on marriage rates and divorce rates between 2000 and 2010, the only years for which county-level marriage and divorce data are available. There is no evidence of any statistically significant effect on marriage or divorce rates, but these effects are noisily estimated.

Because most child maltreatment is reported by someone outside the family, such as a teacher or doctor, it is another measure of family violence that does not suffer from the same self-selection as reported domestic assaults. Many households where IPV is present are also susceptible to child maltreatment, often at the hands of the same abuser (Doyle Jr and Aizer, 2018), so if shelters make victims of intimate partner violence more likely to leave and take their children with them, they could have mitigating effects on child maltreatment as well. County-by-year counts of reported child maltreatment are constructed using data from the National Child Abuse and Neglect Data System's (NCANDS) Child File, which aggregates data on reported child abuse submitted by participating states. However, the county in which the incident was reported is censored in counties with fewer than 1000 incidents, so only 300 satisfying the population threshold for the main sample consistently appear in the NCANDS data. These counts, along with SEER county-level population estimates, are used to construct rates of child maltreatment at the county-year level from 2007-2016.[23] Because the sample is so small to begin with, the NCANDS sample is not limited to counties that have at least one agency reporting in the UCR. Descriptive statistics on child maltreatment, disaggregated by report source, are reported in Appendix Table 23. Results of this analysis are inconclusive as well. Table 35 presents DID-M estimates of the on-impact effects of changes in whether a county has any shelters on reported child maltreatment rates. There is no evidence of a significant effect on child maltreatment, whether reported by someone inside or outside the family. However, the criteria for inclusion in the NCANDS data and the criteria for inclusion in the analysis (counties small enough to be at risk of having no shelters) are directly at odds with each other, so these analyses are likely underpowered and this null result should be interpreted with caution.

5 Policy Implications

5.1 Results in Context

The results in this paper suggest that the existence of a shelter can prevent intimate partner homicides in its county. On the contrary, as noted above, a complementary contemporaneous paper by Sims (2021) uses novel survey data on a sample of existing domestic violence shelters to estimate the effect of capacity expansions on intimate partner homicides, finding no detectable effect. Data from the National Network to End Domestic Violence's annual "One Day in Time" census of domestic violence programs, aggregated over the period from 2008-2017, suggests that domestic violence shelters are highly utilized. Table 12, below, suggests that, on average, most programs are not able to provide housing services to all callers who request them due to capacity constraints. However, neither this fact nor the results in this paper are in conflict with the finding by Sims (2021) that additional shelter capacity may

[23] The NCANDS program is voluntary for states. The number of states participating in NCANDS increased in 2007, so NCANDS data prior to 2007 are not used.

not reduce intimate partner homicides.

Table 12: NNEDV One Day in Time Census by State/Year 2008-2017

	Mean	Std. Dev.	Min	Max	N
Total calls per program	11.64	5.04	1	27.90	492
Unmet requests for housing per program	3.55	3.44	0	35.83	492
Total unmet requests for services per program	5.62	4.42	0	37.17	492
Number of people sheltered per program	20.53	10.09	3	54.33	492

NNEDV "One Day in Time" census occurs each year in September and reports state totals as well as the number of participating programs in each state. The level of observation is the state-year.

Often, when victims call a local hotline seeking services, shelters that are full or nearly full will prioritize the remaining bed space based on the caller's situation, sometimes using lethality assessment questions to determine the caller's immediate risk factors for intimate partner homicide. Police departments also use these assessments when responding to domestic violence calls to refer complainants with the highest indicated risk of homicide victimization to advocacy services, including hotlines that often connect callers to shelter services (Koppa, 2020). Callers who are asked to participate in these assessments take more protective measures and are less likely to experience further physical violence in the immediate future (Messing et al., 2015), and evidence from the first implementations of these assessments by local police departments suggests that they reduce intimate partner homicide rates (Koppa, 2020). If shelters successfully prioritize bed capacity according to lethality risk, it is entirely plausible that, despite little evidence for an effect of additional capacity, it does matter whether there is a local shelter at all.

Likewise, these results add context to the finding by Gubits et al. (2016) that "housing first" policies subsidizing permanent housing for homeless families reduce intimate partner violence more than traditional shelter services. While these interventions may be the gold standard for families experiencing both homelessness and domestic violence, they may not always be available, especially for survivors who are not yet homeless. These survivors often face a choice between staying with their abusers or becoming homeless.

Most shelters are nonprofits that cobble together funding from grantmaking organizations (such as local United Way chapters), state and federal agencies, and private donors. Because shelters are likely resource-constrained, the finding in this paper that the existence of a shelter can reduce intimate partner homicides, when considered with the finding by Sims (2021) that capacity expansions do not, has clear policy implications. Given a limited amount of funding, opening new shelters may be more effective at reducing intimate partner homicides than expanding the capacity of existing shelters.

5.2 Back-of-the-envelope Calculation: Lives Saved

Results from Column (2) Table 3 suggest that the existence of a shelter in a county can prevent approximately .2954 homicides per 100,000 people per year. During the period from 1998-2016, there were a total of 257 female intimate partner homicides that occurred in a county that had a shelter during the year of the homicide. Based on the following calculation,

$$Lives\hat{S}aved = \sum_{a,t:Shelt_{a,t}=1} \frac{Population_{at}}{100,000} * .2954 \tag{8}$$

the availability of shelters prevented about 193 additional female intimate partner homicides from occurring over this 18-year period. On the contrary, 554 intimate partner homicides occurred during this period in counties where there was no shelter in the year the homicide occurred. If every county in the sample had a shelter in every year of the panel, the following calculation

$$Potential\hat{L}ivesSaved = \sum_{a,t:Shelt_{a,t}=0} \frac{Population_{a,t}}{100,000} * .2954 \tag{9}$$

suggests that 369 of those homicides could have been prevented. Note that these estimates only include small and mid-size counties that report consistently to the UCR (covering a total population of about 9 million people), so nationwide estimates would likely be much greater.

A full cost-benefit analysis is not feasible, as privacy censoring in the County Business Patterns data obscures shelters' payroll costs, there is no readily available data on shelters' real estate and other overhead costs, and estimates of benefits from non-fatal domestic assaults are beyond the scope of this paper. However, it should be noted that rates of intimate partner assault are much higher than rates of intimate partner homicide, so if shelters reduce both outcomes, then their benefits are far greater than those estimated here; homicide, while often better-measured than other measures of domestic violence, is the most rare and extreme outcome.

6 Conclusion

The results in this paper suggest that having a shelter in a county can significantly reduce female intimate partner homicides, on the order of about one homicide every three to four years per 100,000 people served by the reporting police agency, or about 68%. When considered in the context of the broader literature on services for domestic violence survivors, these results suggest that the mere existence of a shelter matters more for domestic violence prevention than its bed capacity, possibly due to police referrals and/or shelter staff accurately prioritizing bed space for callers with the highest lethality risk. This result implies that, given a limited amount of funds available to be distributed to service providers, the opening of new shelters may yield greater reductions in violence than the expansion of existing ones.

As reported in Table 1, 33% of the counties in the main sample did not have a shelter at any time during this period and 62% experienced at least one year with no shelter. This fact, along with back-of-the-envelope estimates of lives saved in Section 5, suggests that there is scope for more lives to be saved by opening additional shelters in local areas where there are none.

Shelters are a unique intervention in the suite of policies that affect domestic violence. We know that housing-insecure populations are at high risk for domestic violence, and that domestic violence responds to the broader economic and policy environment and to targeted interventions by law enforcement for those who are willing to self-report. Even survivors who do report are often referred by police to shelters if the abuser is not arrested or is likely to be released on bond and have the ability to engage in retaliatory violence. Overall, the results presented here suggest that, due to the close link between domestic violence and homelessness, shelters are an important piece in the toolkit available to policymakers to combat domestic violence.

7 Appendix

7.1 Additional Data Notes: County Business Patterns Data

The analysis is limited to the period from 1998-2016 due to features of the County Business Patterns data. In 1998, the County Business Patterns dataset switched from using the SIC industry classification system to the NAICS industry classification system, and in doing so introduced an industry code for shelters. Shelters were previously included in a more general SIC code that also covered other types of social services. Furthermore, starting in 2017, the County Business Patterns data no longer reports the number of establishments in each county-industry cell if there are less than three establishments, and does not report NAICS codes with zero establishments, therefore providing essentially no information in counties with few shelters.

While the County Business Patterns data do not identify the type of shelter, hand-collected data provide descriptive insights into the portion of shelters in the data that are likely to serve domestic violence survivors. For counties that had exactly one shelter as of 2016, a research assistant hand-collected data about how many shelters of each type are in these counties as of October 2021. The vast marjority of these counties still have at least one shelter, and most have more than one, as shown in Table 13.

Table 13: Counties with One Shelter in 2016

	Mean	Std. Dev.	N	Min	Max
Any shelters still open as of 2021	0.97	0.16	320	0	1
Number of shelters still open as of 2021	3.00	2.10	320	0	16

Table 14 shows descriptive statistics for the types of shelters in each county that had one shelter in 2016 and one shelter in 2021. 73% of these shelters are domestic violence shelters, and an additional 6% are family shelters. These data suggest that when a small or mid-size county has a single shelter, it is often a domestic violence shelter.

Table 15 shows descriptive statistics for the types of shelters in counties that had one shelter in 2016 and at least one shelter in 2021. 62% of these shelters are either domestic violence shelters, women's shelters, or family shelters, and are therefore likely to serve domestic violence survivors. 95% of these counties have at least one of these three types of shelters, and 90% of these counties have a domestic violence shelter. This means, that, when measuring whether a small to mid-size county has a shelter, I am likely measuring (with

Table 14: Counties with One Shelter in 2016 and One Shelter in 2021

	Mean	Std. Dev.	N	Min	Max
DV Shelter	0.73	0.45	63	0	1
Women's Shelter	0.00	0.00	63	0	0
Family Shelter	0.06	0.25	63	0	1
Homeless Shelter	0.16	0.37	63	0	1
Men's Shelter	0.03	0.18	63	0	1
Other Specialized Shelter	0.00	0.00	63	0	0
DV, Women's, or Family Shelter	0.79	0.41	63	0	1

some error) whether it has a shelter that serves clients experiencing domestic violence. This is error, if anything, likely attenuates the estimated effect of shelter availability on intimate partner homicides.

Table 15: Counties with One Shelter in 2016 and One Shelter in 2021

	Mean	Std. Dev.	N	Min	Max
Any DV Shelter	0.90	0.30	312	0	1
Proportion DV Shelters	0.46	0.30	312	0	1
Any Women's Shelter	0.16	0.37	312	0	1
Proportion Women's Shelters	0.04	0.11	312	0	0
Any Family Shelter	0.32	0.47	312	0	1
Proportion Family Shelters	0.12	0.20	312	0	1
Any General Homeless Shelters	0.69	0.46	312	0	1
Proportion General Homeless Shelters	0.34	0.27	312	0	1
Any DV, Women's or Family Shelters	0.95	0.21	312	0	1
Proportion DV, Women's or Family Shelters	0.62	0.29	312	0	2

7.2 Additional Data Notes: Supplementary Homicide Reports Data

Relatively few agencies report consistently to the UCR. Because the sample is limited to 1) police agencies that report consistently to the UCR every year from 1998-2016 and 2) counties that have a small enough population to be reasonably at risk of not having a shelter, the results in this paper should not be interpreted as nationally representative. They are most applicable to small and mid-size cities.

7.3 Additional Descriptive Statistics

Table 16: Descriptive Statistics: Homicides, Starting at Zero Shelters

	Mean	Std. Dev.	N	Min	Max
Intimate Partner Homicides/100,000 People	0.569	4.732	21755	0	193.4
Female Intimate Partner Homicides/100,000 People	0.414	3.853	21755	0	193.1
Male Intimate Partner Homicides/100,000 People	0.155	2.755	21755	0	193.4
Other Homicides/100,000 People	0.773	5.704	21755	0	406.5
Any Intimate Partner Homicides (Agency-Year)	0.044	0.205	21755	0	1.0
Any Female Intimate Partner Homicides	0.035	0.183	21755	0	1.0
Any Male Intimate Partner Homicides	0.011	0.102	21755	0	1.0
Population served by reporting police agency	9165.669	17143.040	21755	224	337610.0

Table 17: Descriptive Statistics: Homicides, Starting with Shelter(s)

	Mean	Std. Dev.	N	Min	Max
Intimate Partner Homicides/100,000 People	0.413	2.809	34542	0	237.5
Female Intimate Partner Homicides/100,000 People	0.329	2.613	34542	0	237.5
Male Intimate Partner Homicides/100,000 People	0.084	1.026	34542	0	70.5
Other Homicides/100,000 People	0.586	3.026	34542	0	200.0
Any Intimate Partner Homicides (Agency-Year)	0.087	0.282	34542	0	1.0
Any Female Intimate Partner Homicides	0.072	0.259	34542	0	1.0
Any Male Intimate Partner Homicides	0.025	0.157	34542	0	1.0
Population served by reporting police agency	24104.517	42051.441	34542	63	453017.0

Table 18: Descriptive Statistics: Shelters, Starting at Zero Shelters

	Mean	Std. Dev.	N	Min	Max
Zero shelters in 1998	1.000	0.000	720	1	1.0
Minimum number of shelters	0.000	0.000	720	0	0.0
Maximum number of shelters	0.576	0.913	720	0	6.0
Ever had Zero Shelters	1.000	0.000	720	1	1.0
Ever had Any Shelters	0.385	0.487	720	0	1.0
Ever Change Whether County has Any Shelters	0.385	0.487	720	0	1.0

Table 19: Descriptive Statistics: Shelters, Starting with Shelter(s)

	Mean	Std. Dev.	N	Min	Max
Zero shelters in 1998	0.000	0.000	624	0	0.0
Minimum number of shelters	1.361	1.223	624	0	12.0
Maximum number of shelters	3.396	2.568	624	1	20.0
Ever had Zero Shelters	0.179	0.384	624	0	1.0
Ever had Any Shelters	1.000	0.000	624	1	1.0
Ever Change Whether County has Any Shelters	0.179	0.384	624	0	1.0

Table 20: Descriptive Statistics: Demographics, Main Small-County Sample

	Mean	Std. Dev.	N	Min	Max
Percent ages 20-29 in County	0.120	0.010	26879	0	0.2
Percent ages 30-39 in County	0.119	0.010	26879	0	0.2
Percent ages 40-49 in County	0.115	0.008	26879	0	0.2
Percent non-Hispanic White in County	0.339	0.083	26879	0	1.0
Percent Black in County	0.242	0.051	26879	0	0.5
Percent Hispanic in County	0.397	0.077	26879	0	0.6
County Population	87926.522	103837.156	26879	902	943742.0

Sample includes all counties whose population is less than or equal to that of the largest county that begins the panel with zero shelters in 1998. Observations are at the county-year level. Appendix Table 21 presents the same measures for the portion of this sample that starts with zero shelters, and Appendix Table 22 presents the same for the portion that starts with at least one shelter.

Table 21: Descriptive Statistics: Demographics, Starting at Zero Shelters

	Mean	Std. Dev.	N	Min	Max
Percent ages 20-29 in County	0.122	0.011	14399	0	0.2
Percent ages 30-39 in County	0.121	0.010	14399	0	0.2
Percent ages 40-49 in County	0.117	0.009	14399	0	0.2
Percent non-Hispanic White in County	0.370	0.091	14399	0	1.0
Percent Black in County	0.238	0.062	14399	0	0.5
Percent Hispanic in County	0.375	0.077	14399	0	0.6
County Population	41889.186	50352.068	14399	902	808212.0

Table 22: Descriptive Statistics: Demographics, Starting with Shelter(s)

	Mean	Std. Dev.	N	Min	Max
Percent ages 20-29 in County	0.117	0.009	12480	0	0.2
Percent ages 30-39 in County	0.116	0.007	12480	0	0.2
Percent ages 40-49 in County	0.113	0.006	12480	0	0.2
Percent non-Hispanic White in County	0.304	0.055	12480	0	0.8
Percent Black in County	0.246	0.034	12480	0	0.5
Percent Hispanic in County	0.423	0.068	12480	0	0.5
County Population	141042.835	122600.685	12480	3767	943742.0

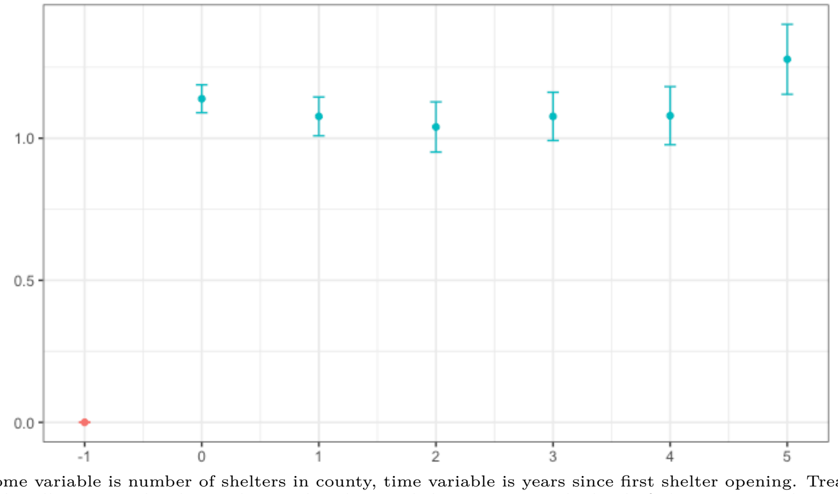
Table 23: Descriptive Statistics: NCANDS Child Maltreatment Reports

	Mean	Std. Dev.	N	Min	Max
Rate per 100,000 of Child Maltreatment Reports	1351.240	777.194	3000	35	5205.5
Rate per 100,000 of Incidents Reported by Family/Self	333.834	231.323	3000	0	3979.7
Rate per 100,000 of Incidents Reported Outside Family	846.334	456.562	3000	0	6062.8
Rate per 100,000 of Substantiated Reported Incidents	355.602	238.002	3000	10	1887.6
Rate per 100,000 of Unsubstantiated Reported Incidents	989.008	610.211	3000	4	4297.2

Table 24: Descriptive Statistics: Marriage and Divorce, Main Sample

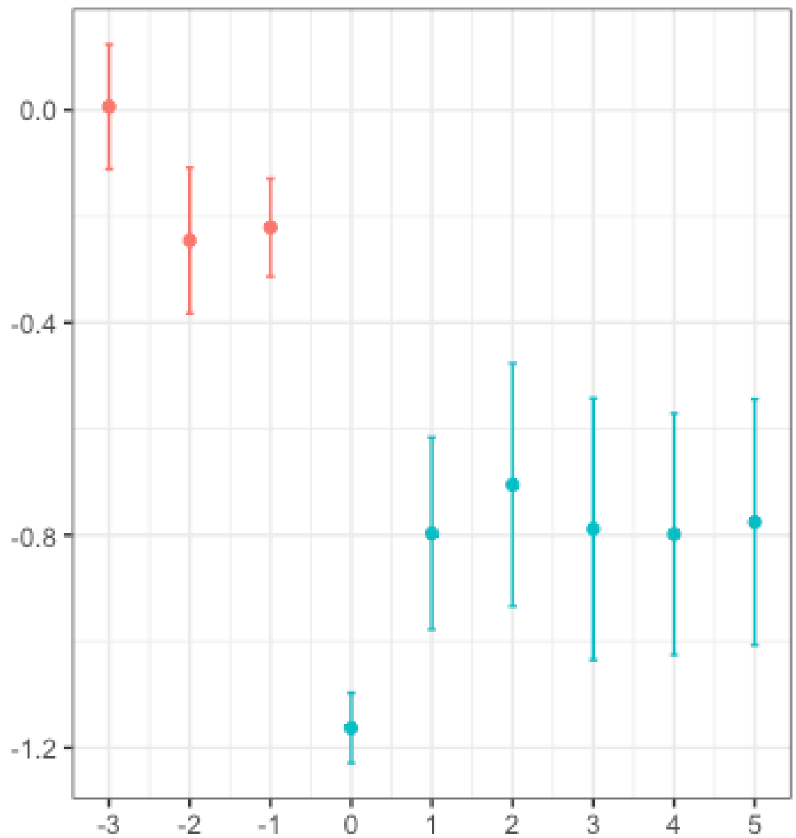
	Mean	Std. Dev.	N	Min	Max
2010 County Marriage Rate per 100,000	712.304	505.053	1248	0	13787.4
2010 County Divorce Rate per 100,000	424.936	1129.020	1248	0	39893.7
Long Difference in Marriage Rate	-170.161	661.115	1248	-20564	1878.7
Long Difference in Divorce Rate	-76.510	1072.242	1248	-37644	1240.9

Figure 8: Event Study: Number of Shelters after First Opening



Notes: Outcome variable is number of shelters in county, time variable is years since first shelter opening. Treatment Sample includes all counties that begin the panel with zero shelters in 1998. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have any shelters.

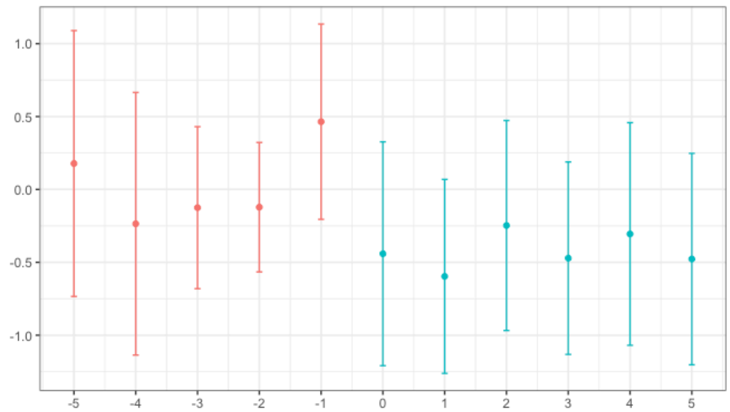
Figure 9: Event Study: Number of Shelters after First Total Closure



Notes: Outcome variable is number of shelters in county, time variable is years since first total closure. Treatment Sample includes all counties that begin the panel with at least one shelter in 1998 and have at least 5 years of follow-up after the first total closure. The level of observation is the agency-year. Counties are considered treated the first time in the panel that they have zero shelters.

7.4 Additional Results

Figure 10: Event Study: Female Intimate Partner Homicide Rates, Permanent Shelter Openings



Notes: Outcome variable is the female intimate partner (IPV) homicide rate, the number of intimate partner homicides with female victims per 100,000 people served by the reporting police agency per year. Time variable is years relative to the permanent opening of a shelter in the county. Sample includes all counties that begin the panel with zero shelters in 1998 and that do not experience a shelter opening and then a subsequent total closure of all shelters. The level of observation is the agency-year.

Table 25: DID-M Estimates, Openings and Closings, Starting at Zero Shelters

	(1) IPV Homicide Rate (Women)	(2) IPV Homicide Rate (Women)	(3) IPV Homicide Rate (Men)	(4) IPV Homicide Rate (Men)
Any shelters in county	-0.2969	-0.2753	0.0366	0.0423
	(0.2171)	(0.2094)	(0.0840)	(0.0845)
County-level age distribution controls	No	Yes	No	Yes
County-level race/ethnicity controls	No	Yes	No	Yes
County-level unemployment rate	No	Yes	No	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides with female (male) victims per 100,000 people served by the reporting police agency per year. Sample includes all counties that begin the panel with zero shelters in 1998. The level of observation is the agency-year.

Table 26: DID-M Estimates, Openings and Closings, Weighted

	(1) IPV Homicide Rate (Women)	(2) IPV Homicide Rate (Women)	(3) IPV Homicide Rate (Men)	(4) IPV Homicide Rate (Men)
Any shelters in county	-0.0658	-0.0622	0.0401	0.0438
	(0.0864)	(0.0791)	(0.0347)	(0.0374)
County-level age distribution controls	No	Yes	No	Yes
County-level race/ethnicity controls	No	Yes	No	Yes
County-level unemployment rate	No	Yes	No	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes
Mean	0.392	0.392	0.126	0.126
Agencies	2963	2963	2963	2963
Counties	1344	1344	1344	1344

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Intimate partner (IPV) homicide rates are the number of intimate partner homicides with female (male) victims per 100,000 people served by the reporting police agency per year. Estimates are weighted by the total population served by the reporting police agency. Sample includes all counties with population less than or equal to that of the largest county that begins the panel with zero shelters in 1998. The level of observation is the agency-year.

Table 27: DID-M Estimates, Openings and Closings, Binary Outcome Model

	(1) Any IPV Homicides (All)	(2) Any IPV Homicides (All)	(3) Any IPV Homicides (Women)	(4) Any IPV Homicides (Women)
Any shelters in county	-0.0017	-0.0017	-0.0053	-0.0054
	(0.0115)	(0.0116)	(0.0118)	(0.0110)
County-level age distribution controls	No	Yes	No	Yes
County-level race/ethnicity controls	No	Yes	No	Yes
County-level unemployment rate	No	Yes	No	Yes
Agency-level linear trend	Yes	Yes	Yes	Yes

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Outcome takes a value of 1 if there were any (female) intimate partner homicides for that agency-year and 0 if there were not. Sample includes all counties with population less than or equal to that of the largest county that begins the panel with zero shelters in 1998. The level of observation is the agency-year.

Table 28: TWFE vs. FD: Female Intimate Partner Homicide Rate, Weighted

	(1) Small Counties	(2) Small Counties	(3) Start at 0	(4) Start at 0	(5) Start at > 0	(6) Start at > 0
Any Shelters in County	-0.0356		-0.0385		0.0707	
	(0.0317)		(0.0468)		(0.0477)	
First Difference: Any Shelters in County		-0.0290		-0.0682		0.0327
		(0.0655)		(0.0942)		(0.0826)
Observations	56283	53313	21755	20610	34528	32703
Mean	0.392	-0.00446	0.427	-0.00767	0.384	-0.00369
Agencies	2963	2963	1145	1145	1818	1818
Counties	1344	1344	720	720	624	624

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Outcome is intimate partner (IPV) homicide rates: the number of intimate partner homicides with female victims per 100,000 people per year. "Small Counties" sample in columns (1) and (2) includes all counties that begin the panel with zero shelters in 1998. Columns (3) and (4) include all counties within the "Small Counties" sample that start with zero shelters in 1998 and columns (5) and (6) include all counties within the "Small Counties" sample that start with at least one shelter at the beginning of the panel in 1998. All specifications include year fixed effects, and columns (1), (3), and (5) include agency fixed effects. All specifications include controls for county-level age and race/ethnicity demographics, county-level unemployment rates, and agency-level rates of other homicides where the relationship between the victim and the perpetrator is known and is not intimate or domestic in nature, and are weighted by the total population served by the reporting police agency in the UCR, unlike the analogous results in Table 7. Appendix Tables 29 and 30 show TWFE and FD results for overall intimate partner homicide rates and male intimate partner homicide rates.

Table 29: TWFE vs. FD: Overall Intimate Partner Homicide Rate

	(1) Small Counties	(2) Small Counties	(3) Start at 0	(4) Start at 0	(5) Start at >0	(6) Start at > 0
Any Shelters in County	0.0308		-0.0517		0.177^{+}	
	(0.0861)		(0.119)		(0.104)	
First Difference: Any Shelters in County		-0.143		-0.176		-0.141
		(0.201)		(0.299)		(0.195)
Observations	56283	53313	21755	20610	34528	32703
Mean	0.473	-0.00825	0.569	-0.0109	0.413	-0.00655
Agencies	2963	2963	1145	1145	1818	1818
Counties	1344	1344	720	720	624	624

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Outcome is intimate partner (IPV) homicide rates: the number of intimate partner homicides per 100,000 people per year. "Small Counties" sample in columns (1) and (2) includes all counties that begin the panel with zero shelters in 1998. Columns (3) and (4) include all counties within the "Small Counties" sample that start with zero shelters in 1998 and columns (5) and (6) include all counties within the "Small Counties" sample that start with at least one shelter at the beginning of the panel in 1998. All specifications include year fixed effects, and columns (1), (3), and (5) include agency fixed effects. All specifications include controls for county-level age and race/ethnicity demographics, county-level unemployment rates, and agency-level rates of other homicides where the relationship between the victim and the perpetrator is known and is not intimate or domestic in nature.

Table 30: TWFE vs. FD: Male Intimate Partner Homicide Rate

	(1) Small Counties	(2) Small Counties	(3) Start at 0	(4) Start at 0	(5) Start at >0	(6) Start at > 0
Any Shelters in County	0.104^{+}		0.113		0.0410	
	(0.0593)		(0.0850)		(0.0533)	
First Difference: Any Shelters in County		0.114		0.170		0.00302
		(0.116)		(0.182)		(0.0405)
Observations	56283	53313	21755	20610	34528	32703
Mean	0.111	-0.000815	0.155	-0.000867	0.0836	-0.000782
Agencies	2963	2963	1145	1145	1818	1818
Counties	1344	1344	720	720	624	624

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Outcome is intimate partner (IPV) homicide rates: the number of intimate partner homicides per 100,000 people per year. "Small Counties" sample in columns (1) and (2) includes all counties that begin the panel with zero shelters in 1998. Columns (3) and (4) include all counties within the "Small Counties" sample that start with zero shelters in 1998 and columns (5) and (6) include all counties within the "Small Counties" sample that start with at least one shelter at the beginning of the panel in 1998. All specifications include year fixed effects, and columns (1), (3), and (5) include agency fixed effects. All specifications include controls for county-level age and race/ethnicity demographics, county-level unemployment rates, and agency-level rates of other homicides where the relationship between the victim and the perpetrator is known and is not intimate or domestic in nature.

Table 31: DID-M: Heterogeneity by Victim Race

	(1) IPV Homicide Rate (White Women)	(2) IPV Homicide Rate (Hispanic Women)	(3) IPV Homicide Rate (Black Women)
Any shelters in county	-0.2360	-0.0279	-0.0162
	(0.1559)	(0.0538)	(0.0438)
County-level age distribution controls	Yes	Yes	Yes
County-level race/ethnicity controls	Yes	Yes	Yes
County-level unemployment rate	Yes	Yes	Yes
Agency-level linear trend	Yes	Yes	Yes
Mean	0.251	0.025	0.079

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 32: DID-M: Heterogeneity by Marital Status with Perpetrator

	(1) IPV Homicide Rate (Women, Current Spouse)	(2) IPV Homicide Rate (Women, Other Partner)
Any shelters in county	-0.1714	-0.1240
	(0.1109)	(0.1261)
County-level age distribution controls	Yes	Yes
County-level race/ethnicity controls	Yes	Yes
County-level unemployment rate	Yes	Yes
Agency-level linear trend	Yes	Yes
Mean	0.170	0.198

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For this secondary analysis on divorces and marriages, I use the following long-differences specification since county-level marriage and divorce rates are only available for the years 2000 and 2010:

$$\Delta_{LD}DivorceRate_c = \beta_0 + \beta_1 \Delta_{LD}AnyShelt_c + \Delta_{LD}X_c\gamma + \epsilon_c \tag{10}$$

where Δ_{LD} denotes the difference in a given variable between 2010 and 2000. X_c includes the age, race/ethnicity, and unemployment controls but not the other homicides control [24].

Table 33: Long Differences - Divorce Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ_{LD} Divorce Rate	Δ_{LD} Divorce Rate	Δ_{LD} Divorce Rate	Δ_{LD} Marriage Rate	Δ_{LD} Marriage Rate	Δ_{LD} Marriage Rate
Δ_{LD} Any Shelters	4.231	8.774	8.208	1.717	21.78	24.29
	(21.29)	(24.61)	(24.37)	(20.82)	(21.58)	(22.51)
Age, race, ethnicity controls	No	Yes	Yes	No	Yes	Yes
State fixed effects	No	Yes	Yes	No	Yes	Yes
% Change county population	No	No	Yes	No	No	Yes
Observations	1248	1248	1248	1248	1248	1248
Mean	424.9	424.9	424.9	712.3	712.3	712.3

Standard errors in parentheses

$^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Divorce Rates are constructed as $DivorceRate_{ct} = \frac{Divorces_{ct}}{(CountyPopulation_{ct}/100{,}000)}$, and marriage rates are constructed similarly. Long differences are taken between 2010 and 2000. Data on the number of marriages and divorces are provided by BGSU (2016) and population data are from SEER Population Estimates.

Table 34: Long Differences - Divorce Rates, Proportion of Time With a Shelter

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ_{LD} Divorce Rate	Δ_{LD}Divorce Rate	Δ_{LD} Divorce Rate	Δ_{LD} Marriage Rate	Δ_{LD} Marriage Rate	Δ_{LD} Marriage Rate
Proportion of Time With a Shelter	54.34	-70.39	-71.42	-24.96	-44.03	-40.10
	(74.72)	(52.47)	(53.36)	(43.11)	(63.70)	(62.04)
Observations	1248	1248	1248	1248	1248	1248
Mean	424.9	424.9	424.9	712.3	712.3	712.3

Standard errors in parentheses

$^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Divorce Rates are constructed as $DivorceRate_{ct} = \frac{Divorces_{ct}}{(CountyPopulation_{ct}/100{,}000)}$, and marriage rates are constructed similarly. Long differences are taken between 2010 and 2000. Data on the number of marriages and divorces are provided by BGSU (2016) and population data are from SEER Population Estimates.

[24] Some specifications also include a state fixed effect and the county-level percent change in population in the intervening years

Table 35: DID-M: NCANDS Child Maltreatment Rates

	(1) Reported Outside Family	(2) Reported by Family/Self	(3) All
Any shelters in county	14.3445	-12.7782	-30.9006
	. (20.5230)	(12.2528)	(43.8927)
County-level age distribution controls	Yes	Yes	Yes
County-level race/ethnicity controls	Yes	Yes	Yes
County-level unemployment rate	Yes	Yes	Yes
County-level linear trend	Yes	Yes	Yes
Counties	300	300 300	

Notes: Outcome is child maltreatment reports per 100,000 people in the county per year. Sample includes all counties with population less than or equal to that of the largest county that begins the NCANDS panel with zero shelters in 2007. All specifications include controls for county-level age and race/ethnicity demographics, unemployment rates, and linear trend.

References

Aizer, A. (2010). The gender wage gap and domestic violence. American Economic Review, 100(4):1847–59.

Aizer, A. and Dal Bo, P. (2009). Love, hate and murder: Commitment devices in violent relationships. Journal of public Economics, 93(3-4):412–428.

Amuedo-Dorantes, C. and Deza, M. (2020). Can sanctuary polices reduce domestic violence?

Anderberg, D., Rainer, H., Wadsworth, J., and Wilson, T. (2016). Unemployment and domestic violence: Theory and evidence. The Economic Journal, 126(597):1947–1979.

Baker, A., Larcker, D. F., and Wang, C. C. (2021). How much should we trust staggered difference-in-differences estimates? Available at SSRN 3794018.

Baker, C. K., Billhardt, K. A., Warren, J., Rollins, C., and Glass, N. E. (2010). Domestic violence, housing instability, and homelessness: A review of housing policies and program practices for meeting the needs of survivors. Aggression and Violent Behavior, 15(6):430–439.

BGSU (2016). County-level marriage and divorce counts. http://www.bgsu.edu/ncfmr/resources/data/original-data/county-level-marriage-divorce-data-2010.html.

Bullinger, L. R., Carr, J. B., and Packham, A. (2021). Covid-19 and crime: Effects of stay-at-home orders on domestic violence. American Journal of Health Economics, 7(3):249–280.

Callaway, B. and Sant'Anna, P. H. (2020). Difference-in-differences with multiple time periods. Journal of Econometrics.

Campbell, J. C., Glass, N., Sharps, P. W., Laughon, K., and Bloom, T. (2007). Intimate partner homicide: Review and implications of research and policy. Trauma, Violence, & Abuse, 8(3):246–269.

Carr, J. B. and Packham, A. (2021). Snap schedules and domestic violence. Journal of Policy Analysis and Management, 40(2):412–452.

Carrell, S. E. and Hoekstra, M. L. (2010). Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. American Economic Journal: Applied Economics, 2(1):211–28.

Chanley, S. A., Chanley Jr, J. J., and Campbell, H. E. (2001). Providing refuge: the value of domestic violence shelter services. The American Review of Public Administration, 31(4):393–413.

Chin, Y.-M. and Cunningham, S. (2019). Revisiting the effect of warrantless domestic violence arrest laws on intimate partner homicides. Journal of Public Economics, 179:104072.

Currie, J., Mueller-Smith, M., and Rossin-Slater, M. (2018). Violence while in utero: The impact of assaults during pregnancy on birth outcomes. The Review of Economics and Statistics, pages 1–46.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. American Economic Review, 110(9):2964–96.

Diette, T. M. and Ribar, D. C. (2018). A longitudinal analysis of violence and housing insecurity. Economic Inquiry, 56(3):1602–1621.

Doyle Jr, J. J. and Aizer, A. (2018). Economics of child protection: Maltreatment, foster care, and intimate partner violence. Annual review of economics, 10:87–108.

Dugan, L., Nagin, D. S., and Rosenfeld, R. (1999). Explaining the decline in intimate partner homicide: The effects of changing domesticity, women's status, and domestic violence resources. Homicide Studies, 3(3):187–214.

Dugan, L., Nagin, D. S., and Rosenfeld, R. (2003). Exposure reduction or retaliation? the effects of domestic violence resources on intimate-partner homicide. Law & society review, 37(1):169–198.

Eckert, F., Fort, T. C., Schott, P. K., and Yang, N. J. (2020). Imputing missing values in the us census bureau's county business patterns. Technical report, National Bureau of Economic Research.

Ellsberg, M., Heise, L., Pena, R., Agurto, S., and Winkvist, A. (2001). Researching domestic violence against women: methodological and ethical considerations. Studies in family planning, 32(1):1–16.

Faraji, S.-L., Ridgeway, G., and Wu, Y. (2018). Effect of emergency winter homeless shelters on property crime. Journal of Experimental Criminology, 14(2):129–140.

Farmer, A. and Tiefenthaler, J. (1997). An economic analysis of domestic violence. Review of social Economy, 55(3):337–358.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. Journal of Econometrics.

Gubits, D., Shinn, M., Wood, M., Bell, S., Dastrup, S., Solari, C., Brown, S., McInnis, D., McCall, T., and Kattel, U. (2016). Family options study: 3-year impacts of housing and services interventions for homeless families. Available at SSRN 3055295.

Harrell, E., Langton, L., Berzofsky, M., and Smiley-McDonald, H. (2014). Household poverty and nonfatal violent victimization, 2008-2012. Technical report, Bureau of Justice Statistics.

Hsu, L.-C. (2017). The timing of welfare payments and intimate partner violence. Economic inquiry, 55(2):1017–1031.

Jakiela, P. (2021). Simple diagnostics for two-way fixed effects. arXiv preprint arXiv:2103.13229.

Kaplan, J. (2021). Jacob kaplan's concatenated files: Uniform crime reporting program data: Offenses known and clearances by arrest, 1960-2019. https://doi.org/10.3886/E100707V16.

Koppa, V. (2020). Can information save lives? effect of a victim-focused police intervention on intimate partner homicides. Effect of a Victim-focused Police Intervention on Intimate Partner Homicides (January 1, 2020).

Leslie, E. and Wilson, R. (2020). Sheltering in place and domestic violence: Evidence from calls for service during covid-19. Journal of Public Economics, 189:104241.

Lindhorst, T., Oxford, M., and Gillmore, M. R. (2007). Longitudinal effects of domestic violence on employment and welfare outcomes. Journal of interpersonal violence, 22(7):812–828.

Messing, J. T., Campbell, J., Webster, D. W., Brown, S., Patchell, B., and Wilson, J. S. (2015). The oklahoma lethality assessment study: A quasi-experimental evaluation of the lethality assessment program. Social Service Review, 89(3):499–530.

O'Flaherty, B. (2019). Homelessness research: A guide for economists (and friends). Journal of Housing Economics, 44:1–25.

Raissian, K. M. (2016). Hold your fire: Did the 1996 federal gun control act expansion reduce domestic homicides? Journal of Policy Analysis and Management, 35(1):67–93.

Sims, K. (2021). Seeking safe harbors: Emergency domestic violence shelters and family violence. Working Paper.

Smith, S. G., Zhang, X., Basile, K. C., Merrick, M. T., Wang, J., Kresnow, M.-j., and Chen, J. (2018). The national intimate partner and sexual violence survey: 2015 data brief–updated release.

Solon, G., Haider, S. J., and Wooldridge, J. M. (2015). What are we weighting for? Journal of Human resources, 50(2):301–316.

Stevenson, B. and Wolfers, J. (2006). Bargaining in the shadow of the law: Divorce laws and family distress. The Quarterly Journal of Economics, 121(1):267–288.

Stiles, M. (2002). Witnessing domestic violence: The effect on children. American Family Physician, 66(11):2052.

Wood, D., Valdez, R. B., Hayashi, T., and Shen, A. (1990). Homeless and housed families in los angeles: a study comparing demographic, economic, and family function characteristics. American Journal of Public Health, 80(9):1049–1052.